

# Multidisciplinary rehabilitation for older people with hip fractures (Review)

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[Intervention Review]

# Multidisciplinary rehabilitation for older people with hip fractures

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## ABSTRACT

### Background

Hip fracture is a major cause of morbidity and mortality in older people and its impact on society is substantial.

### Objectives

To examine the effects of multidisciplinary rehabilitation, in either inpatient or ambulatory care settings, for older patients with hip fracture.

### Search strategy

We searched the Cochrane Bone, Joint and Muscle Trauma Group Specialised Register (April 2009), *The Cochrane Library* (2009, Issue 2), MEDLINE and EMBASE (both to April 2009).

### Selection criteria

Randomised and quasi-randomised trials of post-surgical care using multidisciplinary rehabilitation of older patients (aged 65 years or over) with hip fracture. The primary outcome, 'poor outcome' was a composite of mortality and decline in residential status at long-term (generally one year) follow-up.

### Data collection and analysis

Trial selection was by consensus. Two review authors independently assessed trial quality and extracted data. Data were pooled where appropriate.

### Main results

The 13 included trials involved 2498 older, usually female, patients who had undergone hip fracture surgery. Though generally well conducted, some trials were at risk of bias such as from imbalances in key baseline characteristics.

There was substantial clinical heterogeneity in the trial interventions and populations. Multidisciplinary rehabilitation was provided primarily in an inpatient setting in 11 trials. Pooled results showed no statistically significant difference between intervention and

control groups for poor outcome (risk ratio 0.89; 95% confidence interval 0.78 to 1.01), mortality (risk ratio 0.90, 95% confidence interval 0.76 to 1.07) or hospital readmission. Individual trials found better results, often short-term only, in the intervention group for activities of daily living and mobility. There was considerable heterogeneity in length of stay and cost data. Three trials reporting carer burden showed no evidence of detrimental effect from the intervention. Overall, the evidence indicates that multidisciplinary rehabilitation is not harmful.

The trial comparing primarily home-based multidisciplinary rehabilitation with usual inpatient care found marginally improved function and a clinically significantly lower burden for carers in the intervention group. Participants of this group had shorter hospital stays, but longer periods of rehabilitation. One trial found no significant effect from doubling the number of weekly contacts at the patient's home from a multidisciplinary rehabilitation team.

### **Authors' conclusions**

While there was a tendency to a better overall result in patients receiving multidisciplinary inpatient rehabilitation, these results were not statistically significant.

Future trials of multidisciplinary rehabilitation should aim to establish both effectiveness and cost effectiveness of multidisciplinary rehabilitation overall, rather than evaluate its components.

## **PLAIN LANGUAGE SUMMARY**

### **Multidisciplinary rehabilitation of older patients with hip fractures**

Hip fracture is a serious injury in older people and can contribute to their death or loss of independence. Normally surgery is performed and followed by care in a ward under the supervision of orthopaedic staff. Additional rehabilitation within the hospital is sometimes provided by a geriatrician and other health professionals. Sometimes, the emphasis is on early discharge from hospital with multidisciplinary rehabilitation provided to the patient at home. There is enormous variety in these rehabilitation programmes.

This review included 13 trials, which involved a total of 2498 older, usually female, patients who had undergone surgery for hip fracture. Generally the trials appeared well conducted, although some were at risk of bias that could affect the reliability of their results. For example, despite randomisation, in five trials there were some important differences in patient characteristics, such as age, at the start of the trial that could have influenced trial findings. The trial interventions were very varied but all compared multidisciplinary rehabilitation with usual care. In 11 trials, care was provided either totally or mainly in an inpatient or hospital setting. While there was a tendency for a better outcome after multidisciplinary rehabilitation, the results were not statistically significant and thus cannot be considered conclusive. However, the overall evidence indicates that multidisciplinary rehabilitation is not harmful. Additionally, there was some inconclusive evidence that multidisciplinary rehabilitation did not add to the burden of carers. In one trial that compared home-based multidisciplinary rehabilitation with usual inpatient care, carers reported significantly lower burden in the long term after multidisciplinary rehabilitation. Participants in the home-based rehabilitation group of this trial had shorter hospital stays, but longer periods of rehabilitation. One other trial found no significant effect from doubling the number of weekly contacts at the patient's home by a multidisciplinary rehabilitation team.

Overall, the results of this review suggest that multidisciplinary rehabilitation may help more older people recover after a hip fracture. However, the results are not conclusive and more research is needed.

## **BACKGROUND**

This new review, preceded by a protocol (published in Issue 2, 2008), replaces a previous review (Cameron 2001), the scope of which was confined to inpatient rehabilitation.

### **Description of the condition**

Fracture of the proximal femur, generally termed a 'hip fracture', occurs most often in frail older people and generally as a result of a simple fall from standing height or less (Norton 1997). In industrialised countries, the mean age of people sustaining a hip

fracture is around 80 years and around 80% are female. People sustaining a hip fracture frequently have many other medical and physical problems; these often hinder their recovery and add to the challenge of managing their fracture. Cognitive impairment and dementia are also major considerations, both in terms of risk of hip fracture and recovery from hip fracture. Despite generally successful surgical treatment, hip fracture poses a major threat to life, mobility and independence (Marottoli 1992). Between 12% and 37% of people sustaining a hip fracture die within the first year (Lyons 1997), often as a direct consequence of their fracture (Parker 1991). Most of the survivors fail to regain their former levels of mobility and activity, and many become more dependent (Magaziner 2000). About 10 to 20% of the survivors will require a change to a more dependent residential status (Rosell 2003).

The burden on society from hip fracture is immense and is increasing. An estimated 1.26 million hip fractures occurred in adults in 1990 with predictions of numbers rising to 7.3 to 21.3 million by the year 2050; the steepest increases being expected in Asia (Gullberg 1997). The resources required to provide the institutional and community care for those afflicted are already enormous (Schneider 1990). Recently, the direct costs of hip fracture were found to exceed acute myocardial infarction in Italy (Piscitelli 2007). There are data suggesting current trends in the incidence of hip fracture vary between countries, and also within countries. Overall there is an increase in incidence of hip fracture that is continuing in most countries (Cummings 2002). In developed countries, lifetime prevalence has been estimated to be one in six for Caucasian women and somewhat less than half of this for men (Cummings 2002). Inevitably the absolute disease burden will also increase with population aging, putting further pressure on already stretched health care systems. Additionally, very large costs are generated by the additional requirement for institutional care for many people.

## Description of the intervention

Most people with hip fracture have surgery (Handoll 2008), after which there are a wide range of treatments that are used to assist recovery (SIGN 2002). Some of these treatments have specific goals, such as restoring mobility (see Handoll 2007 for a systematic review of mobilisation strategies) and independence in other basic daily living functions, such as bathing, dressing and continence. The focus of this review is the delivery and provision of rehabilitation, specifically using a multidisciplinary approach.

For the purpose of this review, 'rehabilitation' is defined as services provided by a multidisciplinary team with the goal of reducing disability by improving task-oriented behaviour, for example, walking and dressing.

Care provision after hip fracture varies substantially across the world and may be determined by economic and cultural factors (personal or societal). Many strategies have been employed for the rehabilitation of hip fracture patients. Rehabilitation is generally

adapted to an individual's general health, disability status and living circumstances. For instance, a small percentage of hip fracture patients are fit enough to return directly home from the surgical ward albeit, where possible, with the support of services, including home-based rehabilitation. The majority though have, or require, additional institutionally-based treatment.

Inpatient rehabilitation can be provided in an orthopaedic ward, but it often takes place in a separate rehabilitation ward, to where the patients are transferred from the acute orthopaedic ward upon recovery from their operation. The rehabilitation ward is typically designed to improve functioning by encouraging mobility and self care through practice of these activities with the support of staff in a ward environment that provides equipment, for example mobility aids and adaptations in the bathroom, and appropriate routines, for example meals in a dining room. Multiple health disciplines, for example physiotherapists, occupational therapists, social workers, nurses and doctors, work with the person with hip fracture to provide a co-ordinated rehabilitation programme. This is facilitated by formal meetings, usually weekly, and regular discussion about goals of treatment with the patient and their family. In the UK, there are a number of rehabilitation units that accommodate only geriatric orthopaedic patients (Geriatric Orthopaedic Rehabilitation Units: GORU). The more common model internationally is the Mixed Assessment and Rehabilitation Unit (MARU), where older patients from mixed diagnostic groups are accommodated.

Ideally the patient is generally discharged from a hospital-based rehabilitation programme when able to live with acceptable risk in their usual accommodation or an alternative setting. Following discharge from hospital several different types of rehabilitation programme have been described. Early Supported Discharge (ESD) is a rehabilitation programme provided in the patient's home setting. In the USA, patients are often discharged to skilled nursing facilities for rehabilitation. Other types of non inpatient rehabilitation programme are provided in some circumstances; for example, in a day hospital or in an outpatient department.

Programmes of care and rehabilitation after hip fracture surgery have been developed in which a number of the components described above are combined and co-ordinated. One such programme is the Geriatric Hip Fracture Rehabilitation Programme. These programme components and models are described in more detail in the NHS Review (Cameron 2000).

In summary, the intervention tested in this review is specialised multidisciplinary rehabilitation supervised by a geriatrician or rehabilitation physician (a medical practitioner with skill and experience in rehabilitation) compared with usual care, for older people with hip fracture, in either an inpatient rehabilitation setting, an ambulatory rehabilitation setting or both. The comparator (control) intervention is usual care. In early studies this was care in the orthopaedic surgical ward. However, in some more recent studies it is an alternative rehabilitation programme that usually starts later, and is less intensive and co-ordinated (Halbert 2007). In

the ambulatory setting, the supervisory role may be performed by a specialist clinician in geriatrics or rehabilitation other than a physician.

### How the intervention might work

The primary goal for the person with hip fracture is a return to optimal level of functioning (WHO 2003). To achieve this, specific goals are set and the therapeutic input required to achieve these is dependent on the co-ordinated skills of multiple health professionals. While the process has been described and documented by the World Health Organisation, stroke researchers have most clearly described the components of rehabilitation that are potentially associated with effectiveness (Stroke Collaboration 1997; Stroke Collaboration 2007). However, in this review, we aim to establish the effectiveness and cost effectiveness of multidisciplinary rehabilitation overall, rather than attempt to evaluate its components.

### Why it is important to do this review

This review is required because of the very large number of older people who sustain hip fractures; and the substantial impact, both on the individual and to society, of these fractures. Given this, it is highly important that the effectiveness of the various strategies employed for the rehabilitation of people with these fractures is assessed. Since the previous review (Cameron 2001), which focused on co-ordinated multidisciplinary inpatient rehabilitation only, the international trend to reduction of acute hospital length of stay has intensified and encouraged the development of rehabilitation programmes that operate in a variety of settings. Thus as well as updating the evidence for the inpatient care, we have included outpatient or 'ambulatory' rehabilitation in different settings to ensure the continuing relevance.

## OBJECTIVES

This review aims to examine the effectiveness and cost effectiveness of specialised multidisciplinary rehabilitation supervised by a geriatrician or rehabilitation physician/clinician compared with usual care, for older people with hip fracture. This comparison encompasses both inpatient and post-hospital-discharge (including outpatient) care. Given the anticipated differences in types of service provision between the two settings (inpatient and ambulatory) and consequent problems in interpretation of the findings, we will pool data from both settings on an exploratory basis only. Our primary objectives are thus to examine the effectiveness and cost effectiveness of specialised multidisciplinary rehabilitation supervised by a geriatrician or rehabilitation physician compared with usual care, for older people with hip fracture, in:

- the inpatient rehabilitation setting;
- the ambulatory rehabilitation setting.

Unless predominantly ambulatory care, care spanning over both settings was intended to be included in the inpatient category.

## METHODS

### Criteria for considering studies for this review

#### Types of studies

Included were randomised trials of post-surgical care comparing specialised multidisciplinary rehabilitation with conventional care. Trials that used a quasi-randomisation technique (e.g. allocation by date of birth or days of the week) were also eligible for inclusion.

#### Types of participants

Older people with any type of fracture of the proximal femur which had been surgically fixed prior to entry into the care programme were included. Specific age limits were not defined but as anticipated most participants will be aged 65 years and over. Younger patients (< 65 years) were included as the number of these was relatively small and there appeared to be adequate randomisation with unbiased distribution of this younger population between the intervention and control groups. Reports of proximal femoral fracture confined to younger populations or to people with multiple trauma including hip fracture were excluded.

#### Types of interventions

For this review, the type of intervention under scrutiny is treatment in a multidisciplinary rehabilitation programme (where rehabilitation is delivered by a multidisciplinary team, supervised by a geriatrician or rehabilitation physician/clinician) as opposed to "usual" care (control group). Such a programme would aim to improve the functioning of the person with hip fracture. The programme will be provided in an inpatient or ambulatory setting (or both). Here ambulatory setting covers home, outpatient department and day hospital locations. "Usual" care for the control group will be usual orthopaedic or medical care, or potentially a rehabilitation programme of lesser intensity, or with different components, to the intervention under study.

## Types of outcome measures

The primary outcome is 'poor outcome', defined as death or deterioration of functional status leading to increased dependency in the community or admission to institutional care.

Information was sought on this and its constituent outcomes, and other outcomes as listed below. These were treated as secondary outcomes.

- All cause mortality;
- Morbidity, including postoperative complications and treatment of newly recognised but pre-existing disease or illness;
- Patients' postoperative functional status including cognitive functioning, mobility and ability to perform activities of daily living;
- Resource use including length of hospital stay and subsequent admission rates to, and days spent in, institutional care;
- The level of care and extent of support required or provided on discharge;
- The patients' perceived quality of life, and wellbeing, after discharge;
- Carer burden and stress;
- Direct, indirect, hidden and opportunity costs (the cost of treatments forgone);
- Adherence to rehabilitation under examination (including changes to other types of rehabilitation).

## Timing of outcome assessment

Results were collected for the final follow-up time for which these were available. We, however, took note where trial results may not have been representative of final functional status. This was based on a minimum follow-up of six months from time of injury.

## Search methods for identification of studies

### Electronic searches

We searched the Cochrane Bone, Joint and Muscle Trauma Group Specialised Register (April 2009), the Cochrane Central Register of Controlled Trials (*The Cochrane Library*, 2009, Issue 2), MEDLINE (1950 to April week 3 2009), and EMBASE (1980 to 2009 week 16). We also searched [Current Controlled Trials](#) (up to 29 April 2009) and the [WHO International Clinical Trials Registry Platform](#) (up to 23 April 2009) for ongoing and recently completed trials. No language restriction was applied.

The topic-specific MEDLINE search is combined with the first two stages of the Cochrane optimal trial search strategy ([Higgins 2006](#)). This and the Group's generic hip fracture EMBASE search strategy are shown in [Appendix 1](#).

## Searching other resources

As well as following up any reports of relevant trials presented at conferences attended in person (IC), we included the findings from handsearches of Orthopaedic Transactions, supplements of Acta Orthopaedica Scandinavica and the British Volume of the Journal of Bone and Joint Surgery (1996 to 2007). In addition, we handsearched the final programmes of SICOT (1996, 1999 and 2007), the British Orthopaedic Association Congress (2000, 2001, 2002, 2003, 2005 and 2006), and abstracts of the [American Orthopaedic Trauma Association annual meetings](#) (1996 to 2006). We scrutinised weekly downloads of "Fracture" articles in new issues of 15 journals (Acta Orthop Scand; Am J Orthop; Arch Orthop Trauma Surg; Clin J Sport Med; Clin Orthop; Foot Ankle Int; Injury; J Am Acad Orthop Surg; J Arthroplasty; J Bone Joint Surg Am; J Bone Joint Surg Br; J Foot Ankle Surg; J Orthop Trauma; J Trauma; Orthopedics) and of "Rehabilitation Medicine" in a further 10 journals (Am J Phys Med Rehabil; Arch Phys Med Rehabil; BMJ; Clin Rehabil; J Am Geriatr Soc; J Rehabil Med; JAMA; Lancet; Phys Ther; Scand J Rehabil Med) from [AMEDEO](#).

Results from a comprehensive search for trials (up to August 1998) for a non-Cochrane review on rehabilitation following fractures in older people ([Cameron 2000](#)) were screened as well as those from a more recent non-Cochrane review on multidisciplinary rehabilitation ([Halbert 2007](#)).

## Data collection and analysis

### Selection of studies

After initial screening by two authors (IC and HH), all four review authors independently assessed potentially eligible trials identified via the search for inclusion using a pre-piloted form. Any disagreement was resolved by discussion. It was not found necessary to seek further information about study methods and interventions from trialists to determine eligibility.

### Data extraction and management

Using a pre-piloted data extraction form, pairs of review authors independently extracted trial details and data for the five trials not included in [Cameron 2001](#); and four trials, one of which was the basis of the pilot, reviewed in [Cameron 2001](#). Data extraction for all of the trials was done by HH, who checked these against data extraction forms provided by the other authors and, where appropriate, data presented in [Cameron 2000](#), [Cameron 2001](#) and [Halbert 2007](#). Key additional information was sought from trialists, including the method of randomisation. There was no need to extract results from graphs in trial reports, although this would have been considered where data were not otherwise available. Decisions for data aggregation for calculating 'poor' outcome, the



designated primary outcome of this review, from mortality and functional data presented in individual trials were by consensus (IC and HH).

Results were collected for the final follow-up time for which these are available. We intended, however, to note instances where a marked and important difference between groups in the pattern of functional recovery had been found at an intermediate assessment.

### Assessment of risk of bias in included studies

Risk of bias was independently assessed, without masking of the source and authorship of the trial reports, by pairs of review authors for nine trials, and by HH alone for four other trials that had been assessed in previous reviews. Consistency in assessment was checked by HH at data entry. We used the tool outlined in the Cochrane Handbook for Systematic Reviews of Interventions (Higgins 2008). This tool incorporates assessment of randomisation (sequence generation and allocation concealment), blinding (of participants, treatment providers and outcome assessors), completeness of outcome data, selection of outcomes reported and other sources of bias. We considered 'hard' outcomes (death, admission to institutional care, readmission to hospital) and other outcomes (e.g. functional status) separately in our assessment of blinding and completeness of outcome data. We assessed three additional sources of bias: selection bias resulting from imbalances in key baseline characteristics (e.g. cognitive function, prior care); performance bias such as resulting from lack of comparability in the experience of care providers; and ascertainment bias such as differences in timing of follow-up assessment.

### Measures of treatment effect

Quantitative data reported in individual trial reports for outcomes listed in the inclusion criteria are presented in the text and in the analyses, using risk ratios with 95% confidence intervals for dichotomous outcomes, and mean differences with 95% confidence intervals for continuous outcomes. The primary analyses is based on the rehabilitation setting: inpatient or ambulant. We specified *a priori* that unless predominantly delivered in an ambulant setting, care spanning over both settings or combinations of inpatient and ambulant settings would be initially included in the inpatient category.

### Unit of analysis issues

Although we would have included cluster randomised trials, the unit of randomisation in the included trials was the individual patient.

### Dealing with missing data

Where appropriate, we have performed intention-to-treat analyses to include all people randomised to the intervention groups. As

planned, we have investigated the effect of drop outs and exclusions by conducting worst and best scenario analyses. We were alert to the potential mislabelling or non identification of standard errors and standard deviations. Unless missing standard deviations could be derived from confidence intervals or standard errors, we did not assume values in order to present these in the analyses.

### Assessment of heterogeneity

Heterogeneity was assessed by visual inspection of the forest plot (analysis) along with consideration of the  $\chi^2$  test for heterogeneity and the  $I^2$  statistic (Higgins 2003).

### Assessment of reporting biases

We considered there were insufficient data available to assess publication bias by preparing a funnel plot. Our search of 'grey literature' and pursuit of trials listed in clinical trial registers should have helped to avoid publication bias.

### Data synthesis

Results of comparable groups of trials were pooled, initially using the fixed-effect model and 95% confidence intervals. We also used the random-effects model, especially where there was unexplained heterogeneity. In the absence of cluster randomised trials, we found it unnecessary to use the generic inverse variance to pool data (Deeks 2005).

### Subgroup analysis and investigation of heterogeneity

Our primary analysis is by setting: inpatient and ambulant. As an exploratory analysis, we pooled the data from both settings. We stated *a priori* that unless predominantly ambulant care, care spanning over both settings or combinations of inpatient and outpatient settings would be initially included in the inpatient category. In our protocol we stated that our planned subgroup analyses, which could have been based on the primary setting or the combined setting data, would be by the stage of rehabilitation; patient cognitive function (one measure of this would be the ability versus inability to give individual consent); and pre-injury residence or dependency status (own home/independent; dependent/nursing home or institutional care). We also stated that presentation in separate subgroups would be considered where there is a fundamental difference in intervention, including types of service provision. In the event, our subgroup analysis was limited to categories of intervention. We used the test of interaction to establish whether the subgroups were statistically significantly different from one another (Altman 2003).

## Sensitivity analysis

Where possible, we planned to do sensitivity analyses examining various aspects of trial and review methodology. In the current review, these include the effects of missing data, and whether there was selection bias from imbalances in patient characteristics of the comparison groups. We used the test of interaction to establish whether the subgroups were statistically significantly different from one another (Altman 2003).

## RESULTS

### Description of studies

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

### Results of the search

Systematic searches, carried out for this review and for previous reviews (Cameron 2000; Cameron 2001; Halbert 2007), yielded a total of 37 eligible studies, of which 13 are included, 19 are excluded, two are ongoing, and three are Studies awaiting classification. All 13 included trials are reported in full and in English; exceptionally, Fordham 1986 is available only as a report. Multiple publications, often conference abstracts but also reports reporting longer term follow-up results, are available and included for several trials. Personal communications resulting in additional information and data have been recorded in the Included studies section of the references: some pertain to correspondence belonging to a previous review (Cameron 2001). Four other reports of Stenvall 2007a that focused on different aspects of the intervention were also available for this trial.

### Included studies

Individual trial details of the methods, participants, interventions and outcome measurement for the 13 included trials are presented in the [Characteristics of included studies](#).

### Design

All 13 trials randomised individual patients into one of two intervention groups.

### Sample sizes

The 13 included trials involved a total of 2498 randomised patients who had undergone surgery for hip fracture. Sample size at randomisation ranged from 66 patients recruited into Crotty 2003 to 378 into Galvard 1995.

### Setting

The 13 trials took place in one of six countries: Australia (3 trials); Canada (1); Spain (1); Sweden (2); Taiwan (1); and UK (4). Recruitment start dates ranged from 1984 (Galvard 1995) to 2001 (Shyu 2008); no information was available for Kennie 1988. Care was provided in an inpatient setting, thus before hospital discharge, for both intervention groups of 11 trials (Cameron 1993; Fordham 1986; Galvard 1995; Gilchrist 1988; Huusko 2002, Kennie 1988; Naglie 2002; Shyu 2008; Stenvall 2007a; Swanson 1998; Vidan 2005) but for the control intervention group only of Crotty 2003. Intervention group participants were discharged from acute care within 48 hours of randomisation in Crotty 2003. Only Ryan 2006 was set in the community, after hospital discharge.

### Participants

The mean age of trial participants ranged from 78 to 84 years. Most trials set a lower age limit for trial entry: this ranged from 50 in Cameron 1993 and 70 in Stenvall 2007a. Three trials (Fordham 1986; Gilchrist 1988; Kennie 1988) included only women. The proportion of men in the remaining trials varied from 17% in Cameron 1993 and 33% in Crotty 2003. Six trials (Crotty 2003; Fordham 1986; Galvard 1995; Ryan 2006; Shyu 2008; Swanson 1998) specifically excluded patients with dementia; however, a third of the participants of Shyu 2008 had mild cognitive impairment. Nearly half of the study population of Cameron 1993 had cognitive impairment; and just over half of the study population of Gilchrist 1988 had reduced cognitive scores. The proportion of the study population with dementia or severe cognitive impairment was a fifth in Huusko 2002, a third in Kennie 1988 and Stenvall 2007a, and a quarter in Vidan 2005. Only the proportion (26%) of the study population with mild impairment was reported by Naglie 2002. There was no selection based on pre-fracture residence in nine trials. Two of these (Kennie 1988; Stenvall 2007a) excluded patients whose fracture had occurred in hospital. The four other trials (Crotty 2003; Galvard 1995; Ryan 2006; Swanson 1998) only included people living in the community and, except for Swanson 1998, exclusively in their own home.

### Interventions

The nature of the intervention (multidisciplinary rehabilitation) varied considerably in the included trials. Similarly, there was variation in 'usual care', where described. In 11 trials, multidisciplinary rehabilitation was provided primarily in an inpatient setting. In

Crotty 2003, the intervention was primarily home-based compared with usual inpatient care. Ryan 2006 involved ambulatory care.

### Inpatient rehabilitation

In six trials, the intervention was based on the provision of rehabilitation, after patient transfer, in a GORU either in the same hospital (Gilchrist 1988; Naglie 2002; Stenvall 2007a) or in a hospital at a distance from the orthopaedic department (Fordham 1986; Galvard 1995; Kennie 1988). The control groups received the care that was usually provided by the orthopaedic teams. Patients were seen in consultation by geriatricians if required; some patients in Stenvall 2007a were transferred to a geriatric rehabilitation unit. Access to similar allied health staff was confirmed in Gilchrist 1988 and Kennie 1988; and probable but not confirmed in Fordham 1986 and Galvard 1995. Access to some staff (e.g. occupational therapist) was either greater or exclusive to the intervention group in Naglie 2002 and Stenvall 2007a. In four trials (Fordham 1986; Gilchrist 1988; Kennie 1988; Naglie 2002), the major component of the intervention was combined ward rounds with the geriatrician and orthopaedic surgeons, and multidisciplinary case conferences. Similarly, the close co-operation of geriatrician and orthopaedic surgeons in the medical care of patients and multidisciplinary teamwork were central to Stenvall 2007a. Galvard 1995 provided little detail of the intervention.

In Cameron 1993, Shyu 2008, Swanson 1998 and Vidan 2005, the intervention consisted of a more intensive rehabilitation programme while the control group received the level and type of rehabilitation usually provided in those hospitals. Usual care in Shyu 2008 was more minimal than in other trials, with most participants receiving just one session of physical therapy and no care provision after hospital discharge. The intervention in Swanson 1998 also involved early surgery and the use of regional anaesthesia and local analgesia where possible. Though the interventions were focused on hip fracture patients in essence Cameron 1993, and possibly some of the other trials including Huusko 2002, provided care in a MARU (mixed assessment and rehabilitation unit) setting. Care in Vidan 2005 was provided in shared orthopaedic wards. Huusko 2002 compared multidisciplinary rehabilitation in a geriatric ward in the same hospital where the surgery was undertaken, with care in local community hospitals supervised by general practitioners.

The intervention in Cameron 1993, Shyu 2008, Swanson 1998 and Vidan 2005 was early assessment by the rehabilitation physician or geriatrician (or geriatric nurse), emphasis on re-establishing physical independence and detailed discharge planning. Early mobilisation, early participation in self-care and individualised discharge planning were also part of the care provided in a GORU in Naglie 2002 and Stenvall 2007a. Assessment, intensive rehabilitation in a specialist geriatric ward, detailed discharge planning with follow-up home visits including physiotherapy were provided by

a multidisciplinary geriatric team for the intervention group in Huusko 2002.

### Ambulatory rehabilitation

Crotty 2003 compared accelerated discharge, within 48 hours of randomisation, and home-based interdisciplinary rehabilitation with usual care involving routine interdisciplinary hospital care and rehabilitation in hospital. The rehabilitation programme at the patient's home focused on early resumption of self care and domestic activities.

Ryan 2006 compared intensive (six visits per week) with less intensive (three or less visits per week) multidisciplinary rehabilitation in the patient's own home.

### Outcomes

Only 'poor outcome', the primary outcome, is considered here. This was defined *a priori* as death or deterioration of functional status leading to increased dependency in the community or admission to institutional care. Where data were available, 'poor outcome' was presented for long-term follow-up (between 4 and 12 months) and at hospital discharge. The definitions of deterioration in residential status varied. For some trials it was based on the requirement for institutional care, and in others the non-return to home or independent living. Using this definition, deterioration was not measured in those trials that included people from nursing homes who returned back to nursing homes.

### Excluded studies

The reasons for exclusion of 19 studies are listed in the [Characteristics of excluded studies](#). The primary reasons for exclusion were: treatment allocation not randomised (4 trials); not multidisciplinary rehabilitation (10 trials); mixed population without separate data for hip fracture patients (4 trials); and no comparative data available despite requests (Jette 1987). Reconsideration of the intervention in Sanchez Ferrin 1999, included in Cameron 2001, resulted in its exclusion.

### Ongoing studies

The details of two ongoing trials are presented in the [Characteristics of ongoing studies](#). Notably, Slevold 2009 aims to establish the use of an orthogeriatric unit (GORU), both pre- and postoperatively.

### Studies awaiting classification

Details of the three trials in this category are presented in the [Characteristics of studies awaiting classification](#). All trials were identified after the main study selection process. Jalovaara 2009

appears to have been registered retrospectively and only trial registration details have identified thus far. [Uy 2008](#), which was ended prematurely and included 11 participants only, is unique in its focus on rehabilitation of nursing home residents. Although a full report of this trial is available, a consensus was not reached on whether it should be included in the current review.

### **Risk of bias in included studies**

A summary of the review authors' assessments of the risk of bias associated with various aspects of study design/conduct is presented in [Figure 1](#).

**Figure 1. Summary of review author's assessments (+ = low; ? = unclear; - = high risk of bias) for aspects of study conduct for individual trials.**

	Adequate sequence generation?	Allocation concealment?	Blinding? (Death, residence, readmission)	Blinding? (Function, QOL)	Incomplete outcome data addressed? (Death, residence, readmission)	Incomplete outcome data addressed? (Function, QOL)	Free of selective reporting?	Free of selection bias (from imbalances at baseline)?	Free of performance bias (from non-trial differences in care provision)?	Free of detection bias (from e.g. differences in follow-up procedures)?
Cameron 1993	+	+	?	?	+	?	?	?	?	+
Crotty 2003	+	+	+	?	+	?	?	+	?	+
Fordham 1986	?	+	?	-	?	-	+	?	-	+
Galvard 1995	+	?	+	?	+	-	?	-	-	?
Gilchrist 1988	?	?	?	?	+	?	?	+	?	+
Huusko 2002	+	+	+	?	-	-	-	-	?	+
Kennie 1988	+	+	?	-	+	?	?	-	?	+
Naglie 2002	+	+	+	?	+	+	?	+	?	+
Ryan 2006	+	+	+	+	+	?	?	-	-	+
Shyu 2008	+	+	+	?	?	?	?	+	+	+
Stenvall 2007a	+	+	+	?	+	+	?	-	?	+
Swanson 1998	+	?	+	?	+	+	?	?	?	+
Vidan 2005	?	+	+	+	+	+	?	+	?	+

## Allocation

Eight trials were considered at low risk of bias for randomisation sequence generation and allocation concealment. Insufficient information on sequence generation prevented judgement of the aspect for three trials (Fordham 1986; Gilchrist 1988; Vidan 2005); and on allocation concealment for three trials (Galvard 1995; Gilchrist 1988; Swanson 1998). Overall, there were no serious concerns regarding risk of selection bias from inappropriate methods of treatment allocation.

## Blinding

Blinded assessment was reported for four trials (Crotty 2003; Naglie 2002; Ryan 2006; Vidan 2005). Despite lack of blinding, most other trials clearly applied systematic methods for data collection and often also had independent assessors. The outcomes of death, residence and readmission were considered to be less influenced by lack of blinding than other outcomes such as function and quality of life measures. Lack of blinding may have influenced assessment of these latter outcomes in Fordham 1986 and Kennie 1988.

## Incomplete outcome data

Although sometimes this was achieved after contacting trial investigators, participant flow information was available for all trials. There were some concerns for possible bias for mortality, residence and readmission data for three trials (Fordham 1986; Huusko 2002; Shyu 2008), but this was only considered high risk in Huusko 2002 where there were post-randomisation exclusions and data excluded from participants without initial mental scores. The results for functional outcomes were judged at high risk of bias in Fordham 1986 (missing data and imbalances in missing data between the two groups), Galvard 1995 (data collected for a subgroup, imbalance in numbers assessed) and Huusko 2002 (as above; data only for those participants with baseline measurements).

## Selective reporting

The study protocol was not available for any trial and so a cautious stance was taken in judging this item. The comprehensiveness of the reporting for Fordham 1986 made it very unlikely that selective reporting occurred in this trial. Huusko 2002 was the only trial judged to be at some risk of selective reporting bias.

## Other potential sources of bias

Other biases considered pertained to selection bias from major imbalances in baseline characteristics, performance bias in terms of important differences in care provision outside of that of the trial interventions, and detection bias resulting from difference in follow-up procedures. Contamination bias resulting from the inadvertent application of the intervention being evaluated to people

in the control group potentially leading to reduced differences between the intervention and control groups was also considered. Five trials (Galvard 1995; Huusko 2002; Kennie 1988; Ryan 2006; Stenvall 2007a) had major imbalances at baseline that could have influenced trial findings. In Kennie 1988 and Stenvall 2007a, this imbalance is likely to have favoured the intervention group, whereas the converse was likely in the other three trials.

Insufficient information on care programmes, especially in the control group and on clinician's experience, prevented judgement on the risks from performance bias. There was, however, a high risk of bias in three trials. In Fordham 1986, this was mainly in consequence of the location of the two study sites and arrangements for geriatrician cover. In Galvard 1995, the geriatric hospital had no prior experience with hip fracture patients; and in Ryan 2006, there was risk of compensatory care provision in the less intensive group. Contamination bias, resulting from shared care facilities and/or staff also seemed likely in Fordham 1986, Gilchrist 1988, Ryan 2006 and Vidan 2005.

Active and systematic methods of follow-up seemed to preclude detection bias in all trials except Galvard 1995, where imbalance in the follow-up of functional outcomes may have reflected different research environments at the two study sites.

## Effects of interventions

### Inpatient rehabilitation

#### Poor outcome

The outcomes of death or deterioration in residential status, generally the requirement for institutional care, were combined to give the overall outcome measure of 'poor outcome', which was assessed at the conclusion of follow-up and at hospital discharge. Data pooled from eight trials showed a non-statistically significant tendency in favour of the intervention (Analysis 1.1. Risk ratio 0.89, 95% confidence interval 0.78 to 1.01) at long-term follow-up. The removal of Kennie 1988, which was the only trial with a statistically significant result and the source of the statistical heterogeneity in Analysis 1.1 ( $I^2 = 22\%$ ), from the analysis reduced the size of the effect (Analysis 1.2. RR 0.93, 95% CI 0.82 to 1.07). A test for interaction ( $P = 0.076$ ) showed no difference between the results for trials subgrouped by type of intervention (specialist unit versus intensive rehabilitation): see Analysis 1.3. The removal of Kennie 1988, the source of the substantial heterogeneity ( $I^2 = 55\%$ ), from the specialist unit group again made the result non-statistically significant (RR 0.86, 95% CI 0.66 to 1.12; analysis not shown). A sensitivity analysis (Analysis 1.4) based on perceived risk of selection bias showed no statistically significant difference between the results for trials subgrouped by risk of selection bias (none versus unclear or high risk) from imbalances in patient characteristics (test for interaction:  $P = 0.316$ ). As reported above, the risk of selection bias in Kennie 1988 and Stenvall 2007a meant that the results were likely to favour the intervention group,



whereas the results were more likely to favour the control group in [Huusko 2002](#).

Poor outcome at hospital discharge was based on mortality in hospital and discharge location. This outcome is more vulnerable to bias, such as differences in discharge policies and supply of alternative accommodation (as in [Galvard 1995](#)). Pooled results from seven trials, three of which ([Fordham 1986](#); [Galvard 1995](#); [Gilchrist 1988](#)) did not provide data for long-term 'poor outcome', showed a marginally statistically significant result in favour of the intervention ([Analysis 1.5](#). RR 0.87, 95% CI 0.75 to 1.00). The removal of the results for [Kennie 1988](#) rendered the result non-significant (RR 0.90, 95% CI 0.78 to 1.04; analysis not shown).

### Mortality

Mortality data were reported for all 11 trials. [Analysis 1.6](#) shows no statistically significant difference in mortality at the scheduled end of follow-up (RR 0.90, 95% CI 0.76 to 1.07). There was no statistical heterogeneity in these results. The results for [Cameron 1993](#) were for the four month follow-up period, the main trial follow-up period. The inclusion of 12 month mortality figures for [Cameron 1993](#) did not alter the above finding ([Analysis 1.7](#). RR 0.88, 95% CI 0.74 to 1.04). The results for [Swanson 1998](#) are 12 month mortality data provided by the lead trialist, rather than the six months post-discharge figures extrapolated from the main trial report. There was also no statistically significant difference in hospital mortality ([Analysis 1.8](#). MD 0.77, 95% CI 0.56 to 1.06); only [Vidan 2005](#) found a significant difference in hospital mortality.

### Functional status

Measures of physical functioning varied between studies, and data pooling was either inappropriate or not possible. The numbers of participants for whom measurements were taken were unclear in several trials, and data were often incomplete. One trial ([Gilchrist 1988](#)) provided no data on function. The available data are shown in [Analysis 1.9](#) and [Analysis 1.10](#). Where possible the outcome for poor functional result in survivors combined with mortality data has been presented. This did not present an anomalous situation where the direction of effect differed between the result for the function in the survivors and the combined outcome. [Cameron 1993](#) found no significant difference between the two groups at four months for regain of participant's former level of independence (29% versus 26%). [Fordham 1986](#) also reported no difference in independent patient function based on several activities of daily living. [Galvard 1995](#) reported no difference in long-term mobility but this was measured in a subgroup of participants, with a greater proportion from the control group (38 versus 60). [Huusko 2002](#) found the intervention group recovered instrumental activities of daily living faster and [Swanson 1998](#) reported a statistically significant improvement at three months and discharge but there was no longer a significant difference at six months or final one year follow-up for this trial. Both [Kennie 1988](#) and [Stenvall 2007a](#)

found significantly fewer people in the intervention were more dependent, or either more dependent or dead at one-year follow-up. [Naglie 2002](#) found no difference in function either in terms of walking or transfers. Conversely, [Shyu 2008](#) found significantly fewer participants of the intervention group failed to recover their former mobility. The difference between the two groups in the Chinese Barthel Index scores did not reach statistical significance ([Analysis 1.9](#)). [Stenvall 2007a](#) also found that activities of daily living were better maintained in the intervention group. [Swanson 1998](#) reported a statistically significant difference at three months which was lost at final follow-up ([Analysis 1.9](#)). The difference between the two groups in the incomplete recovery of activities of daily living and mobility was not significant in [Vidan 2005](#).

### Morbidity

The available data for morbidity in terms of in hospital complications ([Swanson 1998](#); [Vidan 2005](#)) or complications during 12 months follow-up ([Huusko 2002](#)), are presented in [Analysis 1.11](#). Significantly fewer intervention group patients (6 versus 13) had chest infection, cardiac problems and/or bedsores in [Swanson 1998](#), but more had stroke or emboli (4 versus 1). [Vidan 2005](#) attributed the reduced overall incidence of medical complications in the intervention group (70 versus 100) to the early identification and daily patient care. The active prevention, identification and treatment of medical disorders was part of the intervention in [Stenvall 2007a](#), where significantly fewer participants of the intervention were reported with post-operative delirium (reported  $P = 0.003$ ), urinary tract infection ( $P = 0.005$ ) or pressure ulcers ( $P = 0.01$ ). Identification of new medical disorders was part of the intervention of [Gilchrist 1988](#), who from an inspection of post-discharge case records with full set of investigation results reported significantly fewer cases of untreated medical conditions in the intervention group (5/88 versus 33/69). [Kennie 1988](#) reported there was no difference between the two groups in the "number of overall illnesses post-discharge". There was no difference between the two groups in the numbers of people with "some type of" complication during follow-up in [Huusko 2002](#). [Cameron 1993](#) reported that there were no medical complications that could be directly attributed to the intervention.

### Length of stay in hospital and hospital readmission

The reported lengths of stay (all studies considered total length of stay) which included initial treatment in the orthopaedic unit and subsequent stay in the rehabilitation setting varied considerably. For example, the range of mean values in the control groups was from 10 days ([Shyu 2008](#)) to 56 days ([Kennie 1988](#)). Lengths of stay in [Huusko 2002](#) were calculated from the day of surgery to the day of discharge lasting at least two weeks. The lengths of stay were shorter in the intervention groups of seven trials ([Cameron 1993](#); [Gilchrist 1988](#); [Huusko 2002](#); [Kennie 1988](#); [Swanson 1998](#); [Stenvall 2007a](#); [Vidan 2005](#)), longer in three ([Fordham 1986](#); [Galvard 1995](#); [Naglie 2002](#)) and similar in one ([Shyu 2008](#)). Stan-

dard deviations were not available for [Fordham 1986](#) (mean length of stay GORU: 56 days; control: 44 days; no difference reported); and data were presented as medians in [Huusko 2002](#) (median 34 versus 42 days, reported  $P = 0.05$ ), and [Vidan 2005](#) (median 16 versus 18 days, reported  $P = 0.06$ ). The distribution of length of stay in [Fordham 1986](#) showed more participants of the intervention group (10 versus 4) stayed over 91 days. [Vidan 2005](#) reported two outliers whose stay was over 100 days. Where data were presented showing the distribution of lengths of stay, it was clear that they were not normally distributed but, for completeness, the graph of the available data from eight trials is presented in [Analysis 1.12](#). These data were not pooled given the clearly considerable heterogeneity. The total number of days in hospital per patient was 80 in each group of [Huusko 2002](#). There was also no difference in mean stay over six months in institutional care (either acute or rehabilitation hospital or nursing home) in [Naglie 2002](#), which was reported to be 110 days for both groups. Mean hospital stay in [Stenvall 2007a](#) over one year was less in the intervention group (37.0 versus 51.4 days; reported  $P = 0.051$ ).

Hospital readmissions, reported in six trials, did not significantly differ between intervention and control ([Analysis 1.13](#). RR 0.99, 95% CI 0.82 to 1.19). However, there was some heterogeneity between the trial results ( $I^2 = 28\%$ ). In [Galvard 1995](#) there was a marked and significant decrease in those readmitted in the intervention group whereas there were slightly more readmissions in the intervention group in four other trials ([Cameron 1993](#); [Shyu 2008](#); [Stenvall 2007a](#); [Swanson 1998](#)). There was no difference in [Vidan 2005](#). [Swanson 1998](#) reported that no readmission was related to the original admission. The disparity of direction effect between [Galvard 1995](#) and three trials ([Cameron 1993](#), [Stenvall 2007a](#); [Swanson 1998](#)) persisted when mortality data (these were deaths in hospital except for [Cameron 1993](#), where results were for mortality at four months) were added to readmission data. (One intervention group patient who remained in hospital was included in the numerator for [Swanson 1998](#).) The combined outcome favoured the intervention in [Vidan 2005](#) given the large difference in-hospital mortality (1 versus 9) for this trial. Overall, there was no statistically significant difference between the two groups for the pooled data ([Analysis 1.14](#). RR 0.93, 95% CI 0.79 to 1.09). For readmission data, it is notable that three trials with shorter lengths of stay in the intervention groups ([Cameron 1993](#); [Stenvall 2007a](#); [Swanson 1998](#)) tended to have more readmissions in this group. In contrast, there were fewer readmissions in the intervention group of [Galvard 1995](#), where average length of stay was 25 days more than in the control group.

### Carer burden

Two studies ([Cameron 1993](#); [Kennie 1988](#)) reported data relating to carer burden. A separate report of [Cameron 1993](#) found that carer burden prior to fracture was the strongest predictor of subsequent burden and that carers of patients in nursing homes and patients with greater cognitive and physical disability were

more burdened. In the 12 month follow-up of [Kennie 1988](#), no difference in carer burden was reported between the intervention and control groups. [Cameron 1993](#) found that the intervention did not significantly impact on carer burden. Without supporting data or indication of how outcome was assessed, [Huusko 2002](#) reported no differences between the two groups in home nursing, food service or help from others.

### Costs

Four trials reported the results from a cost analysis ([Cameron 1993](#); [Fordham 1986](#); [Galvard 1995](#); [Huusko 2002](#)). [Cameron 1993](#), who reported cost outcomes in a separate paper, found that costs (defined as cost per recovered patient) was significantly reduced in the intervention group (10,600 versus 12,800 Australian dollars). Costs assessed were direct costs due to treatment and aftercare up to four months after the fracture. [Fordham 1986](#) concluded that the cost of care per patient (£2714 versus £2618 at 1985 prices) was slightly greater in the intervention group due to costs generated by travel to the unit. [Galvard 1995](#) reported increased costs for the intervention group (84,537 versus 94,026 Swedish krona at 1989 prices). Though the total direct cost per patient during the first year in the intervention group was estimated at 2000 euros (1999 prices: 17,900 versus 15,900 euros) more, [Huusko 2002](#) considered that the costs did not differ remarkably and furthermore suggested that the costs in the control group were underestimated. [Gilchrist 1988](#) noted that no additional funding had been allocated for the intervention in their trial. An estimate of the cost benefit of the Orthopaedic-Geriatrician liaison unit responsible for delivering the intervention in [Swanson 1998](#) was given in the abstract of conference proceedings ([Day 1997](#)) but did not appear in the final report.

### Subgroup analysis by patient characteristics

Aside from [Huusko 2002](#), there were no data available from other trials that enabled meaningful subgroup analysis by patient characteristics. The initial report of [Huusko 2002](#) presented the results split by the Mini Mental State Examination scores of patients into four categories (normal, suspected mild dementia, suspected moderate dementia, suspected severe dementia) (*see Huusko 2000*). However, we have not presented these data here. Given the significant imbalance in the numbers with suspected dementia between the two groups in this trial, the relatively small numbers of patients in the various subgroups, some potential discrepancies in the baseline characteristics data and failure to carry out intention-to-treat analyses, presenting the subgroup analyses based on the available data in the trial report could be misleading.

### Ambulatory rehabilitation

The two trials in this category are considered in turn. [Crotty 2003](#) compared accelerated discharge, within 48 hours of randomisation, and home-based interdisciplinary rehabilitation with usual care consisting of routine interdisciplinary hospital care and rehabilitation in hospital in 66 people recovering from hip



fracture surgery. There were no statistically significant differences between the two groups in the incidence of 'poor outcome' (mortality or higher level of care at 12 months), mortality, higher level of care (move to hostel or nursing home) or inability to walk (see [Analysis 2.1](#)). Similarly, as shown in [Analysis 2.2](#), there were no statistically significant differences at 12 months in the SF-36 scores for the physical (MD 4.70, 95% CI -0.43 to 9.83) or mental (MD 1.50, 95% CI -2.88 to 5.88) components. At four months follow-up, [Crotty 2003](#) found statistically significant differences favouring the intervention group for the modified Barthel index (median 97.0 versus 94.0; clinical importance not established); and the Falls Efficacy Scale results, although there was no difference in the rate of falls. Participants in the home-based rehabilitation group had a shorter stay in hospital (see [Analysis 2.3](#): MD -6.50 days, 95% CI -11.30 to -1.70 days), but a longer period of rehabilitation (MD 14.00 days, 95% CI 7.84 to 20.16 days). Therapists visited participants of the home-based group an average of 13.6 times. There was no difference in the numbers of participants who were readmitted to hospital within four months (see [Analysis 2.4](#)). At four months, there were no statistically significant differences in carer time, GP visits or use of community services. However, the burden of care, as rated by the Caregiver Strain Index (CSI), was reported to be statistically and clinically significantly less for carers of participants of the home-based group at 12 months (median CSI 1.0 versus 4.0; reported P = 0.02).

[Ryan 2006](#) compared intensive with less intensive home-based multidisciplinary rehabilitation lasting a maximum of 12 weeks in 71 people who had been recently discharged from hospital after hip fracture surgery. There were no statistically significant differences between the two groups in any of the measured outcomes. The data for 'poor outcome' (mortality or institutional care at 12 months), mortality and institutional care are presented in [Analysis 3.1](#). Intensification of intervention was based on at least doubling the number of contacts made by the multidisciplinary team per week. This was not achieved although statistically significantly more visits were made to the intervention group (see [Analysis 3.2](#): mean difference 6.50 visits; 95% CI 3.01 to 9.99).

### Care in both settings

In our protocol we stated that we would consider an exploratory analysis where data were pooled from both settings. Inclusion of data from [Crotty 2003](#), where the care in the intervention group was predominantly at home, made very little difference (see [Analysis 1.3](#)) to the results for 'poor outcome' ([Analysis 4.1](#): RR 0.88, 95%CI 0.78 to 1.00).

## DISCUSSION

### Summary of main results

The characteristics of multidisciplinary rehabilitation and 'usual care' varied considerably in the 13 included trials, and all findings need to be viewed in the context of the clinical heterogeneity of the trial interventions, trial populations and outcome measurement. Multidisciplinary rehabilitation was provided totally or primarily in an inpatient setting in 11 trials. Pooled results showed no statistically significant difference between intervention and control groups for poor outcome (risk ratio 0.89; 95% confidence interval 0.78 to 1.01). Although this tends to favour multidisciplinary rehabilitation, this result is not robust and the potential effect is reduced by the removal of [Kennie 1988](#); a trial that may be biased because of imbalances in baseline characteristics. Removal of [Kennie 1988](#) halves the absolute risk reduction (from 4% to 2%). Subgroup analysis by type of intervention (specialised unit versus more intensive rehabilitation) did not show a significant difference. There was also no statistically significant difference for mortality (risk ratio 0.90, 95% confidence interval 0.76 to 1.07) or readmission to hospital, whether for fracture-related or unrelated conditions. Individual trials reported more favourable results in the intervention group for activities of daily living and mobility, although this was sometimes short term. No quality of life measures were reported. There was considerable heterogeneity in length of stay and cost data; the intervention was less costly in two of the four trials providing quantitative data for costs. The three trials reporting carer burden showed no evidence of a detrimental effect from the intervention.

The only trial ([Crotty 2003](#)) comparing primarily home-based multidisciplinary rehabilitation with usual inpatient care found a marginal improvement in participant functioning and a clinically significantly lower long-term burden for carers in the intervention group. Although the participants in the home-based rehabilitation group had shorter hospital stays, they had a longer period of rehabilitation. [Ryan 2006](#) found no significant effect from intensification aimed at doubling the number of weekly contacts at the patient's home from a multidisciplinary rehabilitation team.

### Overall completeness and applicability of evidence

Some trial reports did not provide complete data for the primary outcome at long-term follow-up. Although this was often remedied by contact with trial investigators, data from only eight of the 11 inpatient-setting trials were available for pooling. This meant that data from only 69% (1633/2361) of the participants recruited into the 11 trials contributed to this outcome; although data were available for poor outcome at discharge for the other three trials. This deficiency also hindered subgroup analyses, as did the general absence of data for different categories of patients ([Huusko 2002](#) being the exception for dementia status). Additionally, data for the various validated measures of function used in the trials were generally incomplete.

As emphasised above, the trials identified are a disparate group.

This, together with inadequate details of 'usual care', hampers considerations of the applicability of the findings of the review. Nonetheless, the majority of the 11 trials conducted in an inpatient setting investigated collaborative care between orthopaedic surgeons and geriatricians with involvement of a multidisciplinary team. Some basic grouping by intervention (specialist unit versus intensive rehabilitation) was also possible; but subgroup analysis was inconclusive. The interventions of trials evaluating more intensive rehabilitation were more complex and multicomponent than for trials comparing care in a GORU compared with conventional orthopaedic care. Although Stenvall et al (Stenvall 2007a) included interventions which were aimed at different outcomes (delirium, nutritional status, falls), separate publications (respectively: Lundstrom 2007; Olofsson 2007; Berggren 2008 and Stenvall 2007b) reporting these outcomes rightly set them in the context of the whole rehabilitation programme. This recognises that interaction with the selected components and other components of the intervention cannot be ruled out or quantified. Essentially, any application of the findings of this review needs to be levelled at the general concept of multidisciplinary rehabilitation. However, the evidence to date means that our review cannot conclusively answer the question of effectiveness of this approach, although observed trends favour the principle of co-ordinated multidisciplinary approaches to rehabilitation after hip fracture.

Another question is to whom these results apply aside from the target population. We cannot answer this question too specifically but note that there were marked differences in study populations and contexts. For example, usual care in Shyu 2008, which included one session of physiotherapy only and discharge home without aftercare after a relatively short stay in hospital, contrasts greatly with usual care in the other trials. Additionally, patients with dementia or from nursing homes are under-represented in the included trials and, as pointed out, our primary outcome does not measure a deterioration in those who are already in institutional care.

Potentially very pertinent to but not appraised in Shyu 2008 was carer burden. The three inpatient rehabilitation trials reporting carer burden did not show a negative effect on carers from the intervention. The only trial (Crotty 2003) evaluating primarily home-based multidisciplinary rehabilitation found a clinically significantly lower long-term burden for carers in the intervention group.

Costs were incompletely assessed in the included trials and the findings from four trials were mixed. A proxy for direct costs for inpatient rehabilitation is length of hospital stay; the direction of effect also varied between individual trials. Some trials aimed for early supported discharge from hospital. A finding from Crotty 2003 was that the actual length of rehabilitation can still take longer in home-based rehabilitation.

While the GORU is still relevant, in practice and as a research topic, the general relevance of this review needs to be monitored in the light of evolving and changing clinical practice. This is likely

to be complex, with much variation and so the heterogeneity of the already included trials and those in the future is likely to be an inevitable feature of this review.

Finally, the implementation of the intervention in Ryan 2006 was poor and, as acknowledged by the trial investigators, the lack of information on the actual care provided hampers interpretation of trial findings.

## Quality of the evidence

Although most trials appeared well designed, an impression usually enhanced upon gaining additional information from trial investigators, some were at risk of bias. In the above, we have drawn particular attention to imbalances in key patient characteristics (e.g. gender, mental health) that could have influenced trial results of five trials. It is notable that this selection bias occurred despite seemingly adequate randomisation methods and points to the need for larger study sizes. Lack of blinding and incomplete outcome data are more likely to have affected the reliability of functional assessment rather than mortality and other 'hard' outcomes. Nonetheless, some trial reports gave a confusing or an incomplete account of mortality and residence, which required clarification from trial authors. Sometimes this resulted in some small unaccountable discrepancies between published and unpublished findings. Some questions remained unanswered on conduct of some trials, including those relating to performance and contamination biases.

## Potential biases in the review process

Given the sustained effort over the years in searching for trials in this area, and the continued involvement of one author (IC) and his colleagues in primary research, we think it is unlikely that we have overlooked fully published trials in this slow-moving research area. It is hard to ascertain whether we have missed many unpublished trials but the identification due to post-registration of one trial that now awaits classification shows that it is possible. While we consider that we have included and excluded trials appropriately, it was sometimes hard to decide whether trials evaluated multidisciplinary rehabilitation or not. This is illustrated by the exclusion of Sanchez Ferrin 1999, formerly included in Cameron 2001 but actually a trial testing geriatric assessment.

Despite the clear heterogeneity of trial comparisons, populations, outcome assessment and some aspects of trial quality, we pooled data for several 'hard' outcomes provided by the trials comparing multidisciplinary rehabilitation with usual care in the inpatient setting. This activity in itself is not a potential bias in the review process but we acknowledge that it does hamper interpretation and considerations of external validity. It is notable that statistical heterogeneity in poor outcome in the long-term or at discharge was not substantial and could be alleviated by the removal of just one trial. Lack of data prevented the potential to examine the

effects of clinical heterogeneity through subgroup analysis.

The emphasis on the primary outcome and other 'hard' outcomes, for which most trials provided data, could have meant that important findings from the various measures of function and independence may have been given less attention than merited. However, data for the various validated measures of function in use were generally incomplete and, where complete, pooling was either not possible or appropriate.

A strength of the review is the often successful acquisition of extra data and details from trial investigators. This can add to the complexity of the data extraction and checking processes, and lead to disparities between the published and subsequently provided data. In this review, the process has reinforced the perception of the inherent dangers of reporting percentages without the data from which they are derived.

### Agreements and disagreements with other studies or reviews

This review stems from [Cameron 2001](#), of which the fourth update appeared in Issue 2, 2003 of *The Cochrane Library*. Despite the addition of four trials resulting in a total of nine trials in the final update, successive updates resulted no substantive change to the conclusions. Those in [Cameron 2001](#) were: "While this review concludes that there is no conclusive evidence of the effectiveness of co-ordinated post-surgical care typified by the GORU model following proximal femoral fracture, there is a trend towards effectiveness in all main outcomes." Our current review has been expanded in scope, with some re-evaluation of the inclusion criteria and updating of review methods. Despite the inclusion of three new trials in the inpatient category, and exclusion of a formerly included trial ([Sanchez Ferrin 1999](#)), our conclusions for this setting remain consistent with those in [Cameron 2001](#).

Three review authors also contributed to [Halbert 2007](#), which shared the increased scope of our review. [Halbert 2007](#) concluded that patients who received multidisciplinary rehabilitation were at lower risk of a poor outcome defined as death or admission to a nursing home at discharge from rehabilitation. Our review focused on long-term outcome, which ranged between four months and one year, but crucially was less susceptible to differences in discharge policies or practicalities between intervention groups in individual trials. [Halbert 2007](#) also pooled data from [Crotty 2003](#) with those from the inpatient rehabilitation trials. Feedback at the protocol stage for our review persuaded us that we should only pool from different settings in an exploratory way. [Halbert 2007](#) concluded that the result supported "the routine provision of organized care following hip fracture, as is current practice for patients after stroke." The findings from our review make such a conclusion more tentative.

## AUTHORS' CONCLUSIONS

### Implications for practice

While there is no conclusive evidence of the effectiveness of multidisciplinary inpatient rehabilitation following hip fracture surgery in older people, there is a trend towards effectiveness in all main outcomes. Importantly, no serious detriment (in terms of patient outcomes or crude cost comparisons) has been demonstrated as a result of this care. Therefore, although further evidence of the potential costs and benefits of multidisciplinary inpatient rehabilitation are required, there is some rationale to justify its adoption in the meantime. The optimal structure, setting and intensity of this care are not known. There is insufficient evidence to conclude for multidisciplinary rehabilitation in the ambulatory setting.

### Implications for research

The available studies are a heterogeneous group, and there is insufficient information to draw robust conclusions about effectiveness and cost-effectiveness of multidisciplinary rehabilitation in either the inpatient or ambulant setting. This is unsatisfactory, especially set in the context of the increasingly elderly population, with the numbers of hip fracture patients increasing at the same time as the pressures on health care funds are increasing. Societal changes, including the loss of traditional carers, also contribute to the urgency in finding the most optimal forms of affordable good quality provision of care for hip fracture patients.

Future research evaluating the effectiveness of specialised inpatient rehabilitation needs to pay attention to several factors. A modest effect size, such as a 10% reduction in poor outcome, would be an important result for this population. To obtain definitive evidence for such a reduction larger trials are needed. Measures of outcome should be standardised (particularly with regard to functional status) and assessed by a blinded observer. Trials should include careful monitoring of direct and indirect costs as well as carer burden and cost effectiveness. Components of the interventions used in trials should be carefully specified. Some account should be taken of other strategies including those of early discharge and a more community-based emphasis. Patient characteristics, including cognitive status, should be clearly presented and if subgroup analyses are considered, these should be a priori, sufficiently powered, and an analysis of the outcome of patients with or without evident dementia considered. With current knowledge, studies should aim to establish the effectiveness of multidisciplinary rehabilitation overall. An evaluation of its components should be delayed until more general evidence is available.

As well as trial registration and making the trial protocol publicly available, it is important that trial reports conform to the requirements of the nonpharmacologic CONSORT statement ([Boutron 2008](#)). Complying with this version of CONSORT should mean that adequate descriptions of the interventions under test will be available.

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Scottish Intercollegiate Guidelines Network. Guideline 56: Prevention and management of hip fracture in older people. A national clinical guideline 2002. Scottish Intercollegiate Guidelines Network: [www.sign.ac.uk/guidelines/published/support/guideline56/index.html](http://www.sign.ac.uk/guidelines/published/support/guideline56/index.html) (accessed 05 November 2007).

**Stenvall 2007b**

Stenvall M, Olofsson B, Lundstrom M, Englund U, Borssen B, Svensson, et al. A multidisciplinary, multifactorial intervention program reduces postoperative falls and injuries after femoral neck fracture. *Osteoporosis International* 2007;**18**(2):167–75. [MEDLINE: 17061151]

**Stroke Collaboration 1997**

Anonymous. How do stroke units improve patient outcomes? A collaborative systematic review of the randomized trials. Stroke Unit Trialists Collaboration. *Stroke* 1997;**28**(11):2139–44. [MEDLINE: 9368554]

**Stroke Collaboration 2007**

Stroke Unit Trialists' Collaboration. Organised inpatient (stroke unit) care for stroke. *Cochrane Database of Systematic Reviews* 2007, Issue 4. [DOI: 10.1002/14651858.CD000197.pub2]

**WHO 2003**

United Nations. World programme of action concerning disabled persons. Division for Social and Policy Development, United Nations 2003. [www.un.org/esa/socdev/enable/diswpa01.htm](http://www.un.org/esa/socdev/enable/diswpa01.htm) (accessed 5 November 2007).

\* Indicates the major publication for the study



## CHARACTERISTICS OF STUDIES

### Characteristics of included studies *[ordered by study ID]*

#### Cameron 1993

Methods	<p>Randomised trial: stratification (using sealed envelopes)</p> <p>Assessor blinding: no</p> <p>Length of follow-up: 4 months (12 months for some outcomes)</p>
Participants	<p>General hospital in Sydney, Australia.</p> <p>Conducted: January 1989 to October 1990.</p> <p>252 people with hip fracture; 17% male. Mean age 84 years. Cognitive status: 122 were cognitively impaired.</p> <p>Inclusion criteria: Aged &gt; 50 years, had undergone surgery within 7 days of injury for an uncomplicated hip fracture, resident in district, informed consent.</p> <p>Exclusion criteria: pathological fracture, other fractures, fracture sustained while a hospital inpatient, transferred to another hospital for surgery.</p> <p>Assigned: 127/125 [Accelerated care / Usual care]</p> <p>Assessed (4 and 12 months): 127/125</p> <p>Loss to follow-up: none lost to follow-up.</p>
Interventions	<p>Randomisation took place at time of surgery.</p> <p>(1) Accelerated rehabilitation and early discharge. Early assessment by physician experienced in rehabilitation and geriatric medicine, to identify and treat concurrent illness, and establish rehabilitation goals. Early commencement of rehabilitation (mobilisation and self-care), greater emphasis on retraining physical independence, closer family care giver support and more detailed discharge planning. Nursing home patients: contact by rehabilitation physician and arrangements made for mobilisation; supervised by nursing staff, visiting physiotherapist. Review of progress by rehabilitation physician; and orthopaedic review according to need. Other patients, mobilisation in hospital, supervised by physiotherapist, continued by nursing staff. Joint 3 or 4 times weekly review by orthopaedic surgeon and rehabilitation physician. Patients with limited disability direct discharge to home: seen by occupational therapist, input too from social worker and nutritionist. Additional / more intensive treatment for those with moderate to severe disability prior to fracture: transfer to rehabilitation ward with interdisciplinary rehabilitation programme and regular contact with nursing, medical, physiotherapy, occupational therapy and social workers. Additional support upon discharge, e.g. delivered meals, nursing help, physiotherapy, day hospital attendance.</p> <p>(2) Conventional care and rehabilitation (of these 56% had multidisciplinary rehabilitation) as per standard treatment provided at study hospital at time of trial. Patients living in nursing homes and those with limited disability were discharged when deemed orthopaedically appropriate. Patients likely to need additional assistance to return home were referred to Rehabilitation and Geriatric service several days post surgery. Many were transferred to a rehabilitation ward.</p> <p>The following summary is given: "In summary, accelerated rehabilitation differs from conventional care in its early assessment of rehabilitation goals, early commencement (usually within 24 hours of surgery), greater emphasis on retraining for physical independence, closer family care-giver contact and more detailed discharge planning."</p>

**Cameron 1993** (Continued)

Outcomes	'Poor outcome': mortality at discharge or discharge to institutional care; mortality at 12 months or institutional care. Other outcomes: mortality (4 and 12 months), ADL/functional status (Barthel Index), length of stay, place of residence, readmissions, carer burden, cost effectiveness.	
Notes	Data and clarifications received from Ian Cameron (including emails: 26/02/1997, 19/06/2000) on mortality at 4 and 12 months, institutional care, dependency, SDs for hospital stay.	
<b>Risk of bias</b>		
<b>Item</b>	<b>Authors' judgement</b>	<b>Description</b>
Adequate sequence generation?	Yes	"A stratified randomization procedure was used." Stratification by prior living arrangements and disability.
Allocation concealment?	Yes	"The surgeon was blind to the outcome of randomization, which was performed at the time of surgery." Use of sealed envelopes.
Blinding? Death, residence, readmission	Unclear	Not blinded but data collection unlikely to be affected for these outcomes.
Blinding? Function, QOL	Unclear	Clinical assessor was not blinded to group assignment. However, there was high correlation between the findings and that of an independent blind assessor who evaluated Barthel scores for 10 participants.
Incomplete outcome data addressed? Death, residence, readmission	Yes	"All 252 patients were successfully followed up until death or 4 months after injury." However, there were discrepancies in the mortality data between the reports and those provided by the author from his thesis.
Incomplete outcome data addressed? Function, QOL	Unclear	Despite reporting that all patients were followed up, discrepancies in mortality data (see above) meant that this was unclear.
Free of selective reporting?	Unclear	Incompletely reported but data provided subsequently. The study protocol is not available but it seems very likely that the published reports included all expected outcomes, including those that were pre-specified.

**Cameron 1993** (Continued)

Free of selection bias (from imbalances at baseline)?	Unclear	Intervention group was slightly younger (82.4 versus 85.4); this was reportedly due to “differences in the subgroup from nursing homes”.
Free of performance bias (from non-trial differences in care provision)?	Unclear	Insufficient details for the control group to judge this.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Follow-up procedures were the same in both groups and all participants were accounted for.

**Crotty 2003**

Methods	<p>Randomised trial: using computer-generated randomisation and sealed, opaque envelopes; independent pharmacist with no other involvement with study.</p> <p>Assessor blinding: yes</p> <p>Length of follow-up: 12 months</p>
Participants	<p>3 hospitals in Adelaide, Australia.</p> <p>Conducted: July 1998 to March 1999.</p> <p>66 people with hip fracture, 33% male. Mean age 82.5 years. Cognitive status: 14 had confusion; otherwise needed to be able to participate in rehabilitation programme.</p> <p>Inclusion criteria: Aged 65+ years, had undergone surgery for fall related hip fracture; medically stable; adequate medical and mental capacity to participate in a rehabilitation programme; expected to return home after hospital discharge; home environment suitable for rehabilitation; agreed to hospital admission should complications occur; written consent.</p> <p>Exclusion criteria: inadequate support in community; no telephone at home; or not resident in Adelaide’s Southern Metropolitan Region.</p> <p>Assigned: 34/32 [Accelerated care / Usual care]</p> <p>Assessed (12 months): 28/28</p> <p>Loss to follow-up: 3 lost to follow-up; 7 died</p>
Interventions	<p>Randomisation took place in acute care, after surgery</p> <p>(1) Accelerated discharge and home-based interdisciplinary rehabilitation. Initial assessment by study co-ordinator who visited patient’s home and organised modifications, equipment and assistive aids before patient’s discharge. GPs were contacted. Participants were discharged from acute care within 48 hours of randomisation and were visited by physiotherapists, occupational therapists, speech pathologists, social workers and therapy aides, who negotiated a set of realistic, short-term, and measurable treatment goals with both participants and their carers. Standard therapy services (e.g. podiatry, nursing care, assistance with light domestic tasks) provided as required. Weekly multidisciplinary case conference also attended by specialist in rehabilitation medicine or geriatrician. Decision to discharge from rehabilitation programme made in consultation with rehabilitation consultant. When necessary participants were referred to community agencies for ongoing care.</p> <p>(2) Usual or ‘conventional’ care, involving routine interdisciplinary hospital care and</p>

Crotty 2003 (Continued)

	rehabilitation in hospital. Inpatient services, care pathways and discharge planning.	
Outcomes	<p>'Poor outcome': mortality at 12 months or living in more dependent residence (hostel or nursing home).</p> <p>Other outcomes: mortality (4 and 12 months), quality of life (SF-36: physical component summary and mental component summary); ADL (Modified Barthel Index), Balance Confidence Scale, mobility (timed up and go test and inability to walk), length of stay in hospital/of rehabilitation (including at home), change in residence (indicating higher level of care), readmissions, home visits by therapists (as part of intervention), use of community services, GP visits, Caregiver Strain Index.</p>	
Notes	Data and clarifications received from Michelle Miller (17/12/2008): separate data by group for deaths, admission to higher level of care; inability to walk, readmission to hospital; SDs for SF36 data, and hospital stay.	
<b>Risk of bias</b>		
<b>Item</b>	<b>Authors' judgement</b>	<b>Description</b>
Adequate sequence generation?	Yes	"computer-generated allocation sequence"
Allocation concealment?	Yes	"Randomization was undertaken by the hospital pharmacy department, which maintained a computer-generated allocation sequence in sealed opaque envelopes." "Randomization ... was performed by a hospital pharmacist who was unaware of the medical status of the patient and who had no other involvement in the study."
Blinding? Death, residence, readmission	Yes	"Follow-up data were collected at 4 and 12 months ... by an assessor who was blinded to treatment allocation."
Blinding? Function, QOL	Unclear	"Follow-up data were collected at 4 and 12 months ... by an assessor who was blinded to treatment allocation." However, trial participants were not blinded.
Incomplete outcome data addressed? Death, residence, readmission	Yes	Loss to follow-up and death fully reported and data split by treatment group provided.
Incomplete outcome data addressed? Function, QOL	Unclear	Although denominators stated the 3 lost to follow-up were all in the accelerated care group.
Free of selective reporting?	Unclear	Incompletely reported but data provided subsequently. The study protocol is not

**Crotty 2003** (Continued)

		available but it seems very likely that the published reports included all expected outcomes, including those that were pre-specified.
Free of selection bias (from imbalances at baseline)?	Yes	Baseline characteristics, as well as prior care and timing seemed comparable.
Free of performance bias (from non-trial differences in care provision)?	Unclear	Not enough detail to confirm this.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Comparable follow-up, and set times.

**Fordham 1986**

Methods	Randomised trial: method unclear although independent of clinicians providing care Assessor blinding: no mention Length of follow-up: to discharge
Participants	General hospital in Huddersfield, UK; (intervention patients transferred to community hospital). Conducted: March 1985 to March 1985. 108 women with hip fracture (surgically treated). Mean age not stated. Cognitive status: none retained in trial with long standing dementia. Inclusion criteria: Women aged over 65 years with hip fracture. Exclusion criteria: (Post-randomisation) Patients transferred from other hospitals or health authorities. Non consent. Confusion and terminal illness. Assigned: 50/58 [Joint geriatric and orthopaedic care / Orthopaedic care] Assessed (mortality): 50/58 Loss to follow-up: none, but no data on 25 omitted (14 versus 11) post-randomisation.
Interventions	Randomisation took place immediately after the operation. (1) Joint geriatric and orthopaedic management involving early post-admission assessment by geriatrician on the orthopaedic ward, joint decision for transfer to geriatric-orthopaedic ward (6 beds in orthopaedic rehabilitation ward converted to geriatric-orthopaedic beds), joint clinical rounds once a week and liaison away from bed side, joint responsibility for rehabilitation programme, joint decisions about and facilitation of discharge or transfer, joint junior medical staff. (2) Orthopaedic management (geriatrician available for advice) including decision to transfer to orthopaedic rehabilitation unit.
Outcomes	'Poor outcome': mortality at discharge + discharge to nursing home (or remaining in hospital at 2 months) Other outcomes: mortality (discharge), patient function / activities of daily living (standing, getting in/out of bed, getting in/out of a chair; walking; dressing) assessed using 9 categories (1 = independent), length of stay, place of residence (at discharge or 2 months), transfer to community hospital, costs (staff and hospital stay)

**Fordham 1986** (Continued)

Notes		
<i>Risk of bias</i>		
Item	Authors' judgement	Description
Adequate sequence generation?	Unclear	"Randomly assigned" by research team within first few of hospital admission.
Allocation concealment?	Yes	Randomisation by research team - then appropriate clinicians were informed.
Blinding? Death, residence, readmission	Unclear	Unlikely that mortality data affected by lack of blinding. However, discharge location might have been.
Blinding? Function, QOL	No	The lack of blinding may have influenced these outcomes.
Incomplete outcome data addressed? Death, residence, readmission	Unclear	Post randomisation exclusions means that there is some risk of bias. Full data for death and residence at discharge provided for all those retained in trial.
Incomplete outcome data addressed? Function, QOL	No	Functional outcome data (standing, getting in/out of bed, getting in/out of a chair; walking; dressing) were incomplete with data missing for some participants; with some imbalances between groups in those not assessed.
Free of selective reporting?	Yes	Very comprehensive and planned analyses.
Free of selection bias (from imbalances at baseline)?	Unclear	No significant differences reported in population after post-randomisation exclusions (mainly not in catchment area) but larger numbers of subtrochanteric (12/50 versus 5/58) fractures and less hip replacement in the intervention group.
Free of performance bias (from non-trial differences in care provision)?	No	The three mile distance between the hospitals, with no clinics held in the acute hospital by the geriatricians, may have affected performance. Potential contamination as some clinicians were involved in patient management in both groups.

**Fordham 1986** (Continued)

Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Follow-up appeared thorough.
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**Galvard 1995**

Methods	Randomised trial: use of a random number generator Assessor blinding: no mention Length of follow-up: 12 months
Participants	General hospital in Malmo, Sweden. Conducted: November 1988 to December 1989. 378 people with hip fracture. Of 371: 26% male. Mean age 79 years, range 52 to 102 years. Cognitive status: no information but all independent. Inclusion criteria: People living in own home, perhaps receiving meals on meals or home help service, with hip fractures. Patient consent. Exclusion criteria: People resident in nursing home or waiting for a nursing home bed, or already in hospital. Second hip fracture. Assigned: 182/196 [Geriatric hospital / Orthopaedic hospital] Assessed (mortality): 179/192 Loss to follow-up varied according to outcome assessed: it was markedly incomplete for hip pain (38/60), walking ability and gait speed.
Interventions	Randomisation took place immediately after the operation. (1) Geriatric rehabilitation: transfer to geriatric hospital (usually on second post-operative day) and weekly visit by orthopaedic surgeon. (2) Usual orthopaedic care
Outcomes	'Poor outcome': mortality at discharge + discharge to nursing home (or other) Other outcomes: mortality (discharge and 1 year), patient function (subset at 1 year: hip pain and mobility), length of stay, place of residence (at discharge), readmissions (during 1 year), costs
Notes	Although 378 patients were entered into the study, 7 were excluded (protocol violations) : 4 with a second hip fracture wrongly included; 2 did not give consent, 1 did not live locally.

**Risk of bias**

Item	Authors' judgement	Description
Adequate sequence generation?	Yes	"Randomisation took place at the orthopaedic department immediately after the operation, using a random number generator."
Allocation concealment?	Unclear	No details.

**Galvard 1995** (Continued)

Blinding? Death, residence, readmission	Yes	All patient records, from a central data registration facility, reviewed by the trial investigators.
Blinding? Function, QOL	Unclear	No reference made to blinding but it was considered unlikely that the trial allocation influenced the physiotherapist assessing mobility at one year follow-up.
Incomplete outcome data addressed? Death, residence, readmission	Yes	Full data for death, residence at discharge and readmission provided.
Incomplete outcome data addressed? Function, QOL	No	Functional outcome data (mobility and pain) only provided for a subgroup of trial participants (38 versus 60) attending follow-up session at 1 year. Report noted that a potential contributing factor to the difference in numbers of attendees was "that patients treated at the orthopaedic department traditionally participate in trials and studies, while patients treated at the geriatric hospital had no such tradition at the time."
Free of selective reporting?	Unclear	The study protocol is not available but it is very likely that the published report includes all expected outcomes, including those that were pre-specified.
Free of selection bias (from imbalances at baseline)?	No	As reported by the trial authors, there is significant bias due a) the men in the geriatric hospital group being older (79.1 versus 73.6 years), and b) the higher proportion of patients with subtrochanteric fractures, which often require longer rehabilitation, in the same group (21/179 (12%) versus 8/192 (4%))
Free of performance bias (from non-trial differences in care provision)?	No	In contrast to the orthopaedic hospital, the geriatric hospital had no prior experience with hip fracture patients. It was noted that "the orthopaedic department had first priority to home help services, alterations in patient's flats, and convalescent homes." This and the fact that there was a lack of nursing home beds in Malmo was mooted as a factor contributing to the longer hospital stay in geriatric



**Galvard 1995** (Continued)

		hospital.
Free of detection bias (from e.g. differences in follow-up procedures)?	Unclear	While detection bias was unlikely for outcomes such as death, the imbalance in the follow-up of functional outcomes could have been related to the different research environments at the two trial sites (see above).

**Gilchrist 1988**

Methods	Method of randomisation: unclear, stratified by fracture type Assessor blinding: no mention Length of follow-up: 6 months after discharge (for mortality).
Participants	General hospital in Glasgow, UK. Conducted: October 1984 to July 1986 222 women with hip fracture. Mean age 81 years. Cognitive status: initial mental function assessed using mini object test (101 had scores > 19/30, would have had better mental function). Inclusion criteria: Women aged over 65 years with hip fractures, transferred to the hospital for rehabilitation. Exclusion criteria: None (6 patients had pathological fractures). Assigned: 97/125 [Orthopaedic-geriatric ward / Orthopaedic ward] Assessed (mortality): 97/125 Loss to follow-up: none.
Interventions	Randomisation took place at transfer, which occurred at a mean of 10 days after admission for hip fracture, from another hospital. (1) Combined geriatric-orthopaedic care in special designated unit. Weekly ward round with a geriatrician, an orthopaedic senior registrar and a senior ward nurse; followed by a case conference with in addition a physiotherapist, occupational therapist and a social worker. (2) Usual orthopaedic care in orthopaedic ward. Referral to geriatrician (different person from the one attending the geriatric-orthopaedic ward) by letter. All patients had similar nursing cover, access to similar paramedical services and remained under the care of the orthopaedic surgical staff.
Outcomes	'Poor outcome': mortality at discharge and discharge to nursing home (or institution) Other outcomes: mortality (discharge, 3 and 6 months post discharge), length of stay, place of residence (at discharge), medical conditions (recognised; untreated), length of time patient seen by geriatrician
Notes	Although both groups were reported to have had home visits at 3 and 6 months after discharge, there were no data pertaining follow-up at these times aside from mortality.
<b>Risk of bias</b>	

**Gilchrist 1988** (Continued)

Item	Authors' judgement	Description
Adequate sequence generation?	Unclear	"Randomised". Method not stated. However, "stratified on the basis of the site of the fracture (intracapsular;extracapsular)" and a 5 to 4 bias introduced in favour of the orthopaedic wards due to a larger number of beds.
Allocation concealment?	Unclear	No details.
Blinding? Death, residence, readmission	Unclear	Not blinded, nor was blinding possible, but prospective and active follow-up for death. Unclear if discharge placement was influenced by knowledge of group allocation.
Blinding? Function, QOL	Unclear	No blinding. No function or QOL data but, while some standardisation was evident, assessment of morbidity (untreated conditions) may have been influenced by knowledge of group allocation.
Incomplete outcome data addressed? Death, residence, readmission	Yes	No lost to follow-up or post-randomisation exclusions; no transfer between wards. However, there was an incomplete account of placement of participants who had not been living in the community before their fracture.
Incomplete outcome data addressed? Function, QOL	Unclear	Not clear if function assessed.
Free of selective reporting?	Unclear	The study protocol is not available but it is very likely that the choice of main outcomes was pre-specified.
Free of selection bias (from imbalances at baseline)?	Yes	No important differences in age, gender (all female), type of fracture, dementia (mini object score) and time to transfer
Free of performance bias (from non-trial differences in care provision)?	Unclear	None detected, however, possible contamination with geriatric intervention 'spilling over' to the control group. A greater interest was taken by the orthopaedic surgeons in the medical and social aspects of care in the control group such that patient management (e.g. more comprehensive discharge notes and fewer drugs) had "considerably

**Gilchrist 1988** (Continued)

		changed". This may have been influenced by the rotation of junior staff during training.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Follow-up appeared active and systematic.

**Huusko 2002**

Methods	Randomised using opaque sealed envelopes; independently computer generated allocation sequence. Assessor blinding: no Length of follow-up: 12 months.
Participants	Specialist district hospital in Jyväskylä, Finland. Conducted: October 1994 to December 1998. 260 people with hip fracture. Of 243: 28% male. Mean age 80 years, range 66 to 97 years. Cognitive status: 52 had dementia. Inclusion criteria: community dwelling, > 64 years with surgically treated hip fracture, informed consent, independent ambulation. Exclusion criteria: pathological fracture, multiple fractures, terminally ill, serious early complication, receiving calcitonin, unable to communicate. Assigned: 130/130 [Intensive care/ Usual care]; 120/123 (after post-randomisation exclusions) Assessed (12 months): 120/123 (mortality); 119/119 (residence); 95/98 (ADL)
Interventions	Patient consent obtained on first post-operative day; assume randomisation followed this. All patients mobilised on first post-operative day. (1) Intensive geriatric rehabilitation within hospital: Multidisciplinary geriatric team (geriatrician, specially trained GP and nurses, occupational therapist, physiotherapists, social worker, neuropsychologist) working with other specialists (in physical medicine, neurology, psychiatry) providing intensive rehabilitation programme (assessment; twice daily physiotherapy; ADL practice; daily schedule; counselling; information; discharge plan; home visits, treatment at home after discharge) based in geriatric ward in same hospital as surgery. During first two months after discharge, patients discharged to independent living were visited 10 times by physiotherapist. (2) Discharge to local community hospitals, treatment by general practitioners; physiotherapist usually available. Transfer 2 to 5 days after surgery. Around half of the patients on the long term wards are long-term patients. (No details given of actual non-specialist care.)
Outcomes	'Poor outcome': mortality at discharge or non-return to independent living; transfer to more dependent residence mortality or institutional care or hospital at 12 months. Other outcomes: mortality (in hospital and 12 months), functional status (ADL and instrumental ADL), morbidity (any complication during 12 months), length of stay, discharge location (incomplete data); place of residence (12 months), any complication during follow-up, costs, physiotherapy sessions, home services.

**Huusko 2002** (Continued)

Notes	<p>Patients were also randomised to nasal calcitonin or placebo for 3 months (full report in 2002). The distribution of patients taking calcitonin was claimed to be similar in the two groups considered here.</p> <p>A pre-planned sub-group analysis by cognitive impairment (based on the mini mental state examination) was performed and reported before (2000) the report of the full population (2002).</p> <p>Data from Huusko on return to independent living at discharge; on independence in ADL; and overall length of hospital stay over 1 year follow-up.</p>
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***Risk of bias***

Item	Authors' judgement	Description
Adequate sequence generation?	Yes	"Allocation sequence was computer-generated"
Allocation concealment?	Yes	"Allocation sequence was computer-generated and sealed in numbered, opaque envelopes in Helsinki, Finland, by the information technology department of Novartis before the study was started. The envelopes were stored on the orthopaedic ward by the head nurse until patients were randomised."
Blinding? Death, residence, readmission	Yes	Not blinded, but unlikely to affect results
Blinding? Function, QOL	Unclear	"We could not blind the staff doing the interventions or assessments". Geriatrician in charge of study didn't take part in geriatric rounds or team meetings.
Incomplete outcome data addressed? Death, residence, readmission	No	Baseline data were not presented for 17 participants (10 versus 7): 11 'violation of randomization criteria'; 3 'withdrawal of consent'; 3 'protocol violation'. Residence data also not available for 5 participants without initial mental state scores.
Incomplete outcome data addressed? Function, QOL	No	As well as post-randomisation exclusions, additional losses for functional outcomes (ADL and instrumental ADL) because results provided only for those with data measured at baseline.
Free of selective reporting?	No	No protocol available and incomplete reporting.

**Huusko 2002** (Continued)

Free of selection bias (from imbalances at baseline)?	No	Imbalance in patient characteristics. For example, intervention group had a greater number with dementia (32/120 versus 20/123); fewer were functionally independent in ADL before hip fracture (41 versus 66).
Free of performance bias (from non-trial differences in care provision)?	Unclear	No details of staff training or motivation. Hard to determine for complex packages.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Appeared systematic and thorough.

**Kennie 1988**

Methods	Randomised using random numbers and sealed envelopes Assessor blinding: not done Length of follow-up: 12 months
Participants	District hospital in Stirling, UK. Conducted: over an 18 month period 108 women with hip fracture. Median ages of the 2 groups: 79 and 84 years, range 65 to 94. Cognitive status: initial mental function assessed using short portable mental status questionnaire (56 with at least mild cognitive impairment; 35 had moderate or severe impairment). Inclusion criteria: Women aged over 65, fractured proximal femur requiring operative fixation Exclusion criteria: not fit enough for enrolment into trial; pathological fracture; likely to be discharged within 7 days of trial entry; remaining unfit for transfer, e.g. prolonged traction. Assigned: 54/54 [Multidisciplinary care / Orthopaedic care] Assessed (12 months): 53/54 Loss to follow-up: one patient, who was known to be alive.
Interventions	Randomisation when orthopaedic surgeon judged the patient was fit enough to go to a rehabilitation ward. (1) Multidisciplinary care (general practitioner (GP), geriatrician and advice when needed from orthopaedic specialist) in orthopaedic beds at peripheral hospital. Transfer 0 to 7 days after trial entry. Day to day care by GP. Each week, geriatrician attended 2 ward rounds and one multidisciplinary team conference. Orthopaedic surgeon gave advice on demand. (2) Routine orthopaedic care in orthopaedic admission ward (a few moved to other short stay wards). Allied health care (physiotherapy, occupational therapy; orthotic and other services) received by both groups. Attempt to standardise these overall.

**Kennie 1988** (Continued)

Outcomes	'Poor outcome': mortality or in nursing care at 1 year (or mortality or physically dependent at 1 year) Other outcomes: mortality (discharge, 1 year), functional status (mobility; physical independence), length of stay, place of residence, life satisfaction index, carer burden (at 12 months); medical conditions (no difference post discharge).	
Notes	A few patients from the control group (not stated a number) was moved into other short stay wards at the discretion of the consultant orthopaedic surgeon, and were 'encouraged early discharge'. Data for mortality and residence at one year are taken from the Reid 1989 paper. There were still discrepancies with those provided in Burns 1992, which was also internally inconsistent and thus not included here. The data trends were the same.	
<b>Risk of bias</b>		
<b>Item</b>	<b>Authors' judgement</b>	<b>Description</b>
Adequate sequence generation?	Yes	Randomised: "random sequence determined before the start of the study after the method of Tukey".
Allocation concealment?	Yes	"The allocation was in sealed envelopes held by a departmental secretary."
Blinding? Death, residence, readmission	Unclear	"No attempt was made to blind either staff or patients". Assessments of independence in the activities of daily living were made jointly by an occupational therapist and a physiotherapist, and all other assessments were made by a doctor. Blinding at 1 year assessment was not stated, but seems unlikely.
Blinding? Function, QOL	No	"No attempt was made to blind either staff or patients". Assessments of independence in the activities of daily living were made jointly by an occupational therapist and a physiotherapist. Meetings were held to ensure consistent assessment throughout the study. Blinding at 1 year assessment was not stated, but seems unlikely.
Incomplete outcome data addressed? Death, residence, readmission	Yes	"No patient was excluded on grounds of non-compliance." One patient lost to follow-up was confirmed to be alive.

**Kennie 1988** (Continued)

Incomplete outcome data addressed? Function, QOL	Unclear	As above, but some discrepancies between the Reid and the Burns papers in the numbers followed up.
Free of selective reporting?	Unclear	The study protocol is not available but it is very likely that the choice of main outcomes was pre-specified.
Free of selection bias (from imbalances at baseline)?	No	The treatment group were younger (median ages 79 versus 84) and had higher cognitive function (P = 0.06).
Free of performance bias (from non-trial differences in care provision)?	Unclear	“the consultant geriatrician ... may have ..used community resources more fully, and been more willing than the orthopaedic surgeon to take calculated risks when discharging frail elderly patients back to the community.” In the control group: “the demand for beds encouraged their early discharge”. Both treatment and control groups received physiotherapy, occupational therapy, and orthotic and other services. However, the intervention group may have received more physiotherapy and nursing care. it is unclear whether there “was a greater ability of the nurses to conduct functionally orientated care when working outside the acute orthopaedic ward with its emphasis on caring for highly dependent patients”.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Follow-up appeared active and systematic.

**Naglie 2002**

Methods	Randomised trial: using computer-generated randomisation (block size of 4), stratified by age and place of residence, sequentially numbered, sealed, opaque envelopes. Assessor blinding: yes Length of follow-up: 6 months
Participants	Teaching hospital in Toronto, Canada. Conducted: June 1993 to March 1997. 280 people with hip fracture. Of 279: 20% male. Mean age 84 years. Cognitive status: 74 had mild to moderate mental impairment; unknown numbers with no impairment or severe impairment. Inclusion criteria: Aged 70+ years, had undergone surgery for hip fracture; resident in community or nursing home.

**Naglie 2002** (Continued)

	<p>Exclusion criteria: fracture occurred in acute care setting or hospital, pathological fracture, multiple trauma, previous surgery of fractured hip, expected survival &lt; 6 months, nursing home and dependent for ambulation before fracture, technical failure of surgery, no bed available on interdisciplinary unit.</p> <p>Assigned: 141/139 [Multidisciplinary care / Usual care]</p> <p>Assessed (6 months): 141/138</p> <p>Loss to follow-up: one withdrawal from usual care group</p>	
Interventions	<p>Randomisation took place in post-anaesthetic care unit when the patient was considered stable enough for transfer to a ward.</p> <p>(1) Interdisciplinary care consisting of routine postoperative surgical care plus daily medical care by internist-geriatrician, and regular care by physiotherapist, occupational therapist, social worker and clinical nurse specialist. Twice weekly interdisciplinary rounds to set goals and monitor patient's progress. (Emphasis on preventing medical problems, such as pressure sores; early mobilisation; early participation in self-care; individualised discharge planning; priority assessment &amp; specialised training.)</p> <p>(2) Usual care on orthopaedic units (same hospital but different care teams) which included access to geriatric consultation and allied health care professionals if requested; access to occupational therapists and clinical nurse specialists was limited.</p> <p>There was no specialist follow-up care.</p>	
Outcomes	<p>'Poor outcome': mortality at discharge or transfer to more dependent residence (e.g. nursing home or rehabilitation hospital); mortality at 6 months or living in more dependent residence (nursing home etc); mortality or decline in ambulation; or mortality or decline in ambulation or chair or bed transfers and place of residence.</p> <p>Other outcomes: mortality (at discharge, 3 and 6 months), ADL (Barthel Index), mobility (transfers and walking), length of stay, days in hospitals over 6 months, change in residence (indicating deterioration), health care utilisation.</p>	
Notes	<p>The discrepancies between the full report of the trial, published in 2002, and an abstract (1999) in the numbers randomised resulted from the exclusion of 18 participants in the abstract report. These comprised one patient who withdrew and 17 who were deemed ineligible after randomisation. The latter 17 were included in an intention-to-treat analysis in the full report.</p>	
<b>Risk of bias</b>		
<b>Item</b>	<b>Authors' judgement</b>	<b>Description</b>
Adequate sequence generation?	Yes	"Stratified, computer-generated randomization scheme with a block size of 4." Stratification by age (70 to 79; 80+ years) and place of residence at admission (private residence; retirement home; nursing home).
Allocation concealment?	Yes	"Orthopaedic residents, who were blinded to block size, assigned the patients to treatment group according to sequentially numbered, sealed opaque envelopes that were



**Naglie 2002** (Continued)

		colour coded by stratum.”
Blinding? Death, residence, readmission	Yes	“All follow-up assessments were conducted by research assistants blinded to group assignments.”
Blinding? Function, QOL	Unclear	“All follow-up assessments were conducted by research assistants blinded to group assignments.” Providers and participants were not blinded.
Incomplete outcome data addressed? Death, residence, readmission	Yes	Intention-to-treat analysis. Data missing only for one participant who withdrew from the trial. Flow diagram presented. (At review, 17 patients were deemed ineligible (8 versus 9) but there were included in the full report.)
Incomplete outcome data addressed? Function, QOL	Yes	As above.
Free of selective reporting?	Unclear	The study protocol is not available but it is very likely that the choice of main outcomes was pre-specified.
Free of selection bias (from imbalances at baseline)?	Yes	None of the differences between treatment groups was statistically significant.
Free of performance bias (from non-trial differences in care provision)?	Unclear	“To minimise the risk of contamination bias, interdisciplinary care and usual care were provided by different staff on different wards.” Insufficient information - there was no specialist follow-up care.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Systematic and active follow-up.

**Ryan 2006**

Methods	Randomised trial: random numbers table, sealed opaque envelopes Assessor blinding: yes Length of follow-up: 12 months.
Participants	In the community, Sheffield, UK Conducted: July 2000 to June 2002 71 people with hip fracture. 24% male. Mean age 81 years. Cognitive status: None had dementia Inclusion criteria: Aged 65 years or over recovering from (stroke or) hip fracture, recently

	<p>discharged from hospital to their own home. Contact within 5 days of notification of patient eligibility.</p> <p>Exclusion criteria: Concomitant disease (e.g. Parkinson's disease or dementia).</p> <p>Assigned: 37/34 [intensive / less intensive]</p> <p>Assessed (12 months): 30/28</p> <p>Loss to follow-up = 7 versus 6 (withdrew, deceased, unavailable, in hospital)</p>
Interventions	<p>Randomisation within 5 working days of referral to community rehabilitation team. Maximum duration of treatment period was 12 weeks. All treatment (and assessment) undertaken in patient's own home.</p> <p>(1) Intensive: 6 or more face-to-face contacts per week from members of a multidisciplinary rehabilitation team</p> <p>(2) Less intensive: 3 or less face-to-face contacts per week from members of a multidisciplinary rehabilitation team</p> <p>The service (both augmented and routine) was provided to both groups of patients by a local multidisciplinary team (physiotherapist, occupational therapist, speech and language therapist or therapy assistant). There were 4 teams, divided by geographical area, but belonging to the same city-wide service managed by the same Primary Care Trust.</p>
Outcomes	<p>'Poor outcome': mortality at 12 months and admission to institutional care</p> <p>Other outcomes: mortality (3 and 12 months), function (Barthel Index; Frenchay activities index), admission to institutional care, days spent in hospital (*stroke and hip fracture), use of home care services readmissions (*), number of contacts during treatment period (*), Euroqol (no data), therapy outcome measure (no data).</p>
Notes	<p>Some data for hip fracture patients received from Dr Ryan on 5 February 2009: gender and co-residing carer; death and institutional care at 3 and 12 months follow-up.</p>

***Risk of bias***

Item	Authors' judgement	Description
Adequate sequence generation?	Yes	"Randomization was carried out using a random number table (blocks of 10) and sealed opaque envelopes. Randomization was stratified on the basis of the diagnosis of hip fracture or stroke and locality team."
Allocation concealment?	Yes	"sealed opaque envelopes. .... An administrator based within each of the teams carried out the allocation procedure. The result was recorded on each patient file .. and was immediately communicated to the research team."

**Ryan 2006** (Continued)

Blinding? Death, residence, readmission	Yes	“Blinded follow-up assessments were carried out”
Blinding? Function, QOL	Yes	“Blinded follow-up assessments were carried out” “Blinding was achieved by follow-up being undertaken by trained therapy assistants who were not involved in the provision of service to participating patients and who worked in another part of the city to that of the patient.”
Incomplete outcome data addressed? Death, residence, readmission	Yes	Participant flow diagram provided.
Incomplete outcome data addressed? Function, QOL	Unclear	“A method of imputation, using the median for each group, was used for missing data.”
Free of selective reporting?	Unclear	The study protocol is not available but it is very likely that the published report includes all expected outcomes, including those that were pre-specified.
Free of selection bias (from imbalances at baseline)?	No	There was more males (12/37 versus 5/34) and significantly more patients with a residing carer in the intensive group.
Free of performance bias (from non-trial differences in care provision)?	No	The same multidisciplinary team provided care to both groups. In this pragmatic trial, therapists were requested to provide a more intensive service to those allocated to the intervention group. However, what occurred within those additional face-to-face contacts was not prescribed, neither was it known.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Active and systematic follow-up

**Shyu 2008**

Methods	Randomised trial: coin tossed by a third party Assessor blinding: no, but independent data collectors Length of follow-up: 12 months.
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Participants	<p>Teaching hospital in Taiwan          Conducted: September 2001 to November 2003          162 people with hip fracture. 31.5% male. Mean age 78 years. Cognitive status: About a third of participants had mild cognitive impairment; severe impairment not included.          Inclusion criteria: Aged 60 years or over, admitted for acute hip fracture surgery, unilateral hip fracture, resided in Northern Taiwan, prefracture score Chinese Barthel index &gt; 70; able to perform full range of motion against gravity and resistance. Approval sought for data collection.          Exclusion criteria: Severe cognitive impairment; completely unable to follow instructions (&lt; 10 on Chinese mini-mental state examination); or terminally ill.          Assigned: 72/87 [Multidisciplinary care / Usual care]          Assessed (12 months): 60/62          Loss to follow-up (patient withdrew) = 16 versus 14</p>	
Interventions	<p>Randomisation took place soon after admission to emergency department.          (1) Interdisciplinary programme of geriatric consultation, continuous rehabilitation and discharge planning. Geriatrician and geriatric nurses provided geriatric assessment/consultation; physiotherapist, geriatric nurses, and rehabilitation physician were responsible for rehabilitation programme; geriatric nurse co-ordinated discharge planning service. Early mobilisation, home visit and follow-up services provided. In addition to routine care, 1 physical therapy session a day from geriatric nurse (total 4 times, ~ 30 minutes each), 2 assessments by a physical therapist (each 20 minutes), one visit from rehabilitation physician (20 minutes). Post hospital discharge: 4 home visits during first month (once a week for ~ 30 minutes) and 4 home visits during second and third month (~30 minutes each) from a geriatric nurse.          (2) Usual care on trauma or orthopaedic ward. Some consultations of other disciplines (e.g. internal medicine) occasionally made depending on patient's condition. Exercises taught by nurses in first 2 to 3 days. Number of physical therapy sessions varied because insurance policy: 18 received 3 sessions, the rest only 1 session. Discharge approximately 7 days from surgery with no care provision subsequently.</p>	
Outcomes	<p>'Poor outcome': mortality at 12 months and admission to institutional care; mortality + functional deterioration at 12 months (non recovery of walking function).          Other outcomes: mortality (1, 3, 6 and 12 months), function (mobility), function (modified Barthel Index), depressive symptoms, Chinese Barthel Index, admission to institutional care, length of hospital stay, readmissions (emergency room visit), falls.</p>	
Notes	<p>Confirmation that the study populations of the 2005 and 2008 reports were the same received from Dr Shyu. Additional data for 12 months follow-up also received on 9 December 2008.</p>	
<b>Risk of bias</b>		
<b>Item</b>	<b>Authors' judgement</b>	<b>Description</b>
Adequate sequence generation?	Yes	"randomly assigned to an experimental or control group by the flip of a coin"

**Shyu 2008** (Continued)

Allocation concealment?	Yes	“a neutral third party not involved in delivering the intervention or assessing outcomes flipped the coin”. Adherence assumed.
Blinding? Death, residence, readmission	Yes	Insufficient information but data collection was probably independent.
Blinding? Function, QOL	Unclear	“participants were blinded but evaluators were not”. However, different nurses (data collectors) from those who delivered the intervention conducted the follow-up assessments at participant’s home.
Incomplete outcome data addressed? Death, residence, readmission	Unclear	Participant flow obtained. Some discrepancies in data (e.g. mortality)(between papers and data from author) although not an imbalance in numbers of participants who withdrew.
Incomplete outcome data addressed? Function, QOL	Unclear	As above.
Free of selective reporting?	Unclear	The study protocol is not available but it is very likely that the published report includes all expected outcomes, including those that were pre-specified.
Free of selection bias (from imbalances at baseline)?	Yes	No serious imbalance.
Free of performance bias (from non-trial differences in care provision)?	Yes	Insurance policy determined number of physiotherapy sessions in control group. However, the difference was between one or three sessions. Notably, the active follow-up of the control group can be considered additional to usual care.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Sufficiently similar and independent follow-up procedures

**Stenvall 2007a**

Methods	Randomised trial: sequentially numbered opaque sealed envelopes Assessor blinding: no, but independent data collectors Length of follow-up: 12 months.	
Participants	Teaching hospital in Sweden Conducted: May 2000 to December 2002 199 people with hip fracture. 26% male. Mean age 82 years. Cognitive status: 64 had dementia. Inclusion criteria: Aged 70 years or over, admitted for acute hip fracture surgery, oral and written consent by patient or carer. Exclusion criteria: Severe rheumatoid arthritis or hip osteoarthritis; pathological fracture; severe renal failure; bedridden; fracture sustained in hospital. Assigned: 102/97 [Multidisciplinary care / Usual care] Assessed (12 months): 84/76 (survivors) Loss to follow-up (patient withdrew) = 2 versus 2	
Interventions	Randomisation envelope received in the emergency room but envelope not opened until immediately before surgery. (1) Geriatric unit specialising in geriatric orthopaedic care. Multidisciplinary team providing comprehensive geriatric assessments and rehabilitation. Active prevention, detection and treatment of post-operative complications, such as falls, delirium, pain, pressure sores and malnutrition. Early mobilisation, with daily training, provided by physiotherapists (2 full time), occupational therapists (2 full time) and care staff, including dietician (0.2) during hospital stay. Staffing ratio 1.07 nurses or aides per bed. A geriatric team, including a physician, assessed patients at 4 months including for further rehabilitation needs. (2) Usual care on specialist orthopaedic ward. Care included that by physiotherapists (2 full time), occupational therapist (0.5 full time) and care staff, not including dietician during hospital stay. Staffing ratio 1.01 nurses or aides per bed in orthopaedic ward; 1.07 in geriatric control ward used for longer stays. No 4 month assessment.	
Outcomes	'Poor outcome': mortality at discharge and not discharged to previous residence; mortality + not in same residence at 12 months; or mortality and reduced activity of daily living. Other outcomes: mortality (in hospital; 4 and 12 months), function (mobility), function (Katz ADL), independent living, complications including falls, length of hospital stay (initial episode; over 1 year), return to previous residence, readmissions, falls.	
Notes	Separate publications produced by trial group reporting outcomes (delirium, nutritional status, and falls) in association with selected components of the intervention but set in the context of the whole programme, are not considered in this review.	
<b><i>Risk of bias</i></b>		
<b>Item</b>	<b>Authors' judgement</b>	<b>Description</b>
Adequate sequence generation?	Yes	Mention of 'lots' in the envelopes; stratification by methods of surgery.

**Stenvall 2007a** (Continued)

Allocation concealment?	Yes	Randomised “using opaque sealed envelopes”. “The lots in the envelopes were numbered sequentially.” “The selection procedures were carried out by people not involved in the study.”
Blinding? Death, residence, readmission	Yes	Systematic data collection. “A geriatrician, who was unaware of the study-group allocation, analysed all assessments and documentations after the study was finished.”
Blinding? Function, QOL	Unclear	Not blinded but independent assessors.
Incomplete outcome data addressed? Death, residence, readmission	Yes	Participant flow diagram provided.
Incomplete outcome data addressed? Function, QOL	Yes	Data provided.
Free of selective reporting?	Unclear	The main report of the trial that covers the whole intervention is probably protocol driven. This doesn't apply to the subsidiary publications, see Notes.
Free of selection bias (from imbalances at baseline)?	No	Significant differences between the two groups in the numbers of patients with diagnosed depression (33 versus 45) and on antidepressants (29 versus 45).
Free of performance bias (from non-trial differences in care provision)?	Unclear	Outpatient rehabilitation was not standardised.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	No apparent problems

**Swanson 1998**

Methods	Randomised trial: use of a computer-generated randomisation list Assessor blinding: no Length of follow-up: 12 months post hospital discharge.
Participants	Teaching hospital in Brisbane, Australia. Conducted: October 1994 to July 1995 71 people with hip fracture. 22% male. Mean age 78 years. Cognitive status: none had dementia. Inclusion criteria: Aged 55 years or over, admitted for acute hip fracture surgery, resided at home or in a hostel, independently mobile, accessible for follow-up, public-health

Swanson 1998 (Continued)

	<p>patient, informed consent.          Exclusion criteria: Dementia, pathological fracture, inadequate English; residing in a nursing home.          Assigned: 38/33 [Multidisciplinary care / Usual care]          Assessed: 38/33          No loss to follow-up.</p>
Interventions	<p>Randomisation took place before surgery. Eligible patients admitted to Accident and Emergency or to the Orthopaedic department were identified to the trial co-ordinator who then sought patient consent.</p> <p>(1) Accelerated rehabilitation programme involving multidisciplinary team (orthopaedic surgeon, geriatrician, nurse-coordinator, physiotherapist, occupational therapist, social worker) based in orthopaedic ward. Early surgery, minimal narcotic analgesia, early mobilisation, intense twice-daily physiotherapy, daily visits by occupational therapist and social worker, early review by geriatrician, two additional ward rounds attended by all staff, weekly case conference, home assessment visit before discharge, community services referrals arranged, follow-up at 1 and 6 months post discharge.</p> <p>(2) Standard orthopaedic management including daily visits from physiotherapist. Social worker or occupational therapist by request, geriatrician on referral, weekly discharge planning meeting. Other care including home visit as requested / needed.</p>
Outcomes	<p>'Poor outcome': mortality at discharge + discharge to nursing home or hostel; mortality + functional deterioration at 12 months [or institutional care].</p> <p>Other outcomes: mortality (discharge, 6 and 12 months post discharge), functional decline (mobility), function (modified Barthel Index), ADL, mobility, complications, dynamic balance, length of stay, place of residence (institutional care at discharge and 12 months), social support (dependency), readmissions (all for "unrelated causes"), analgesia, costs.</p>
Notes	<p>Six months post-discharge results were presented in the main trial report (Swanson 1998). A further report (Day 2001) reported phone and postal follow-up at 12 months post-discharge.</p> <p>Clarification of method of randomisation, mortality and length of data results and data for readmissions, functional decline and institutional care received from Cheryl Swanson on 5 December 2001.</p>

**Risk of bias**

Item	Authors' judgement	Description
Adequate sequence generation?	Yes	"computer-generated randomisation list"
Allocation concealment?	Unclear	".. trial co-ordinator, who, having sole possession of the treatment codes, obtained consent then randomised each patient to one of the treatment arms." However, trial co-ordinator also co-ordinated care in the intervention group.



**Swanson 1998** (Continued)

Blinding? Death, residence, readmission	Yes	Specific discharge criteria were established to eliminate bias. Data obtained from medical records.
Blinding? Function, QOL	Unclear	Intervention group patients followed-up at six months while attending an outpatient clinic. Usual care were followed up at outpatient clinic or phoned up by trial co-ordinator.
Incomplete outcome data addressed? Death, residence, readmission	Yes	No loss to follow-up and intention-to-treat analysis implied.
Incomplete outcome data addressed? Function, QOL	Yes	No loss to follow-up and intention-to-treat analysis implied.
Free of selective reporting?	Unclear	The study protocol is not available but it is very likely that the published report includes all expected outcomes, including those that were pre-specified.
Free of selection bias (from imbalances at baseline)?	Unclear	Slight imbalance in male/female composition of groups (Males: 11/38 versus 5/33).
Free of performance bias (from non-trial differences in care provision)?	Unclear	Given the comprehensive nature of the intervention group hard to tell. No hint of contamination of the control group.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Sufficiently similar follow-up procedures.

**Vidan 2005**

Methods	Randomised trial: closed envelopes in a secure box, administered by one person; stratified by pre-fracture ADL level Assessor blinding: yes, for patient interviews Length of follow-up: 12 months
Participants	General hospital in Madrid, Spain. Conducted: February 1997 to December 1997. 321 people with hip fracture. Of 319: 18.5% male. Mean age 82 years, (SD 7.7). Cognitive status: 24.5% had dementia. Inclusion criteria: Previously ambulant, aged 65 years or over, admitted for acute hip fracture surgery. Informed consent for patient or proxy. Exclusion criteria: Unable to walk before fracture; dependency in all ADL; pathological hip fracture; known terminal illness; life expectancy < 12 months. Assigned: 157/196 [Multidisciplinary care / Usual care] Assessed (mortality): 148/164

Vidan 2005 (Continued)

	Loss to follow-up included 2 in Multidisciplinary care group that did not have surgery.
Interventions	<p>Randomisation took place after baseline assessment, within 48 hours of admission. All participants had an orthopaedic surgeon and a nurse assigned on hospital admission. All shared same orthopaedic wards and used same hospital support services, including physical therapy and social work. Orthopaedic surgeon determined discharge in both groups.</p> <p>(1) Multidisciplinary care: Geriatric team (geriatrician, rehabilitation specialist, specific social worker); geriatric assessment; daily visits from geriatrician who was responsible for medical care; rehabilitation specialist planned schedule, duration and intensity of physical therapy; social worker assessed social environment and advised where necessary. Within 72 hours of admission, an interdisciplinary meeting, including orthopaedic and geriatric teams, to formulate a comprehensive treatment plan. Meeting repeated weekly to monitor compliance with plan.</p> <p>(2) Usual orthopaedic care: Management by orthopaedic surgeon and nurses. Counselling from different specialists as required.</p>
Outcomes	<p>'Poor outcome': mortality at 12 months + admission to long-term care institution; mortality in hospital + discharge to rehabilitation facility (incomplete data)</p> <p>Other outcomes: mortality (in hospital and 1 year), patient function (mobility; recovery in ADL at 3, 6 and 12 months), length of stay, discharge to rehabilitation facility; admission to long-term care institution, readmissions (during 1 year); morbidity, medical and surgical complications; receipt and duration of physical therapy.</p>
Notes	Details of method of randomisation and data received from trial investigator: Maria Vidan.

**Risk of bias**

Item	Authors' judgement	Description
Adequate sequence generation?	Unclear	No details: stratified by pre-fracture ADL level (independent in 4 or more ADLs or < 4 ADLs)
Allocation concealment?	Yes	"We used a system of closed envelopes in a secure place (box), administrated by only one person." (personal communication from trialist)
Blinding? Death, residence, readmission	Yes	Data obtained from medical records.
Blinding? Function, QOL	Yes	"A research assistant who was not involved in patient care and was blinded to the allocation group conducted all interviews."
Incomplete outcome data addressed? Death, residence, readmission	Yes	Full data for death, residence at discharge and readmission provided.

**Vidan 2005** (Continued)

Incomplete outcome data addressed? Function, QOL	Yes	Data on ADL and mobility provided.
Free of selective reporting?	Unclear	The study protocol is not available but it is very likely that the published report includes all expected outcomes, including those that were pre-specified.
Free of selection bias (from imbalances at baseline)?	Yes	No major differences in baseline characteristics.
Free of performance bias (from non-trial differences in care provision)?	Unclear	Trial authors noted that there was a shorter length of stay in the control group than in previous years that they thought may be explained by the Hawthorne effect (that the improvement resulted because the physicians were being observed). There were, however, some delays in the discharge of patients with dementia and ADL dependency "because home care services or rehabilitation centre facilities could not be arranged in a timely manner." Potential for contamination bias given both groups had same location and staff.
Free of detection bias (from e.g. differences in follow-up procedures)?	Yes	Identical follow-up procedures.

Allocation concealment: A = yes, B = unclear, C = no

ADL: activities of daily living

GP: General Practitioner

PFF: proximal femoral fracture

vs: versus

**Characteristics of excluded studies** [ordered by study ID]

Abe 2001	Trial of a four (short) versus eight (long) weeks rehabilitation training programme in surgically treated hip fracture patients. A translation from Japanese revealed that the two intervention groups were not concurrent (historical control).
Allegrente 2001	Multicomponent as opposed to multidisciplinary rehabilitation.
Bai 2003	Complete care package rather than multidisciplinary care.
Choong 2000	Randomised evaluation of a clinical care pathway. Not in scope of review.

(Continued)

Cuncliffe 2004	Only 28% of participants had fractures: additionally, the percentage with hip fracture is not reported.
Fordham 1995	Not randomised: observational study of a geriatric orthopaedic programme at one hospital and conventional orthopaedic care at another. Study produced equivalent groups at both hospitals.
Hempsall 1990	Not randomised: allocation to GORU by area of residence.
Houldin 1995	Pilot study evaluating a psychological intervention in eight hip fracture patients. Not in scope of review.
Huang 2005	Not multidisciplinary rehabilitation: discharge planning led by nurse only (and then extra care as requested).
Jette 1987	Quasi-randomised (based on on-call roster) study involving 75 participants with proximal femoral fracture patients. Intensive rehabilitation (standard care plus education sessions, a geriatric team evaluation, weekly team meetings, home visit post discharge, monthly follow-up by phone) compared with standard orthopaedic care. Main outcome measures: mortality, functional status, length of stay, place of residence. No comparative data available: requests for information and comparative data sent with no response.
Krichbaum 2007	While nurse co-ordinator, who could not provide direct care, arranged multidisciplinary care, this is not multidisciplinary rehabilitation.
Kuisma 1994	Unidisciplinary intervention (physiotherapy) only.
Marcantonio 2001	Intervention comprised a geriatric consultation aimed at reducing delirium. Excluded as not multidisciplinary rehabilitation and thus not in scope of review.
Pearson 1988	Specialised nursing unit rather than GORU. Mixed population of CVA, amputation and hip fracture patients (40%). Only limited outcome data for hip fracture patients.
Richards 1998	Location (evaluating early supported discharge and hospital at home) rather than multidisciplinary care. Mixed population: 31% hip fracture.
Roder 2003	Not a randomised trial, although treatment was left to chance.
Rubenstein 1984	No extractable data for fracture patients (23% of trial population): unclear how many had hip fracture.
Sanchez Ferrin 1999	Functional geriatric unit staff involved in initial geriatric assessment and monitoring of uptake of recommendations versus usual orthopaedic care. Assessment not multidisciplinary rehabilitation.
Siu 1996	Mixed population: < 5% (17) had hip fracture.

GORU: geriatric-orthopaedic rehabilitation unit  
CVA : cerebral vascular accident (stroke)

## Characteristics of studies awaiting assessment *[ordered by study ID]*

### Jalovaara 2009

Methods	Randomised trial
Participants	Included: both males and females, aged 50 years or over. Patients who were living in their own home or in sheltered housing (comparable to a home of their own but controlled by a warden and with some assistance available) at the time of sustaining the fracture. Excluded: patients who were institutionalised; or who had a pathological fracture.
Interventions	1. Physiatric-oriented rehabilitation 2. Geriatric-oriented rehabilitation 3. Control group: Routine rehabilitation in health centre hospitals
Outcomes	The following were assessed at 4 and 12 months: 1. Residential status 2. Walking ability 3. Use of walking aids 4. Activities of daily living (ADL) functions 5. Mortality
Notes	Trial is completed (2000); retrospective application for trial registration (March 2009)

### Uy 2008

Methods	Randomised trial: use of “numbered opaque envelopes, with the randomised sequence generated from a random number table”. Blind assessment of outcomes at 4 months.
Participants	11 participants (10 followed up). Included: Female, living in a nursing home in the hospital catchment area, ambulant prior to hip fracture, able to follow commands at time of seeking informed consent. Excluded: Non consent or not fulfilling the inclusion criteria.
Interventions	1. Inpatient multidisciplinary rehabilitation programme involving accelerated rehabilitation (as in <a href="#">Cameron 1993</a> ). 2. Usual care: discharge back to the nursing home soon after hip fracture surgery.
Outcomes	The following were assessed at 1 and 4 months: 1. Barthel index 2. Gait velocity 3. Mortality
Notes	The trial, a full report for which is available, was ended prematurely after recruiting only 11 participants because of a change in Australian Government regulations. Of the 10 participants followed up, only 3 were in the intervention group. A consensus for inclusion of this trial was not achieved due to questions over applicability and its small sample size. Its inclusion will be considered for the next update. Its inclusion will be more likely should another trial on this previously overlooked patient group appear.

**Ziden 2008**

Methods	Randomised trial (see Notes)
Participants	212 community-dwelling patients, aged 65 years or more, with surgery for hip fracture.
Interventions	1. A geriatric, multiprofessional home rehabilitation programme focused on supported discharge, independence in daily activities, and enhancing physical activity and confidence in performing daily activities. 2. Conventional care in which no structured rehabilitation after discharge was included.
Outcomes	The following were assessed at 1 month: 1. Falls 2. Degree of dependency and frequency in daily activities 3. Habitual physical activity 4. Basic functional performance
Notes	Randomisation and allocation occurred at emergency unit without patient consent. Subsequent exclusion of 99 patients, before patients invited to participate. Eleven declined consent, resulting in 102 study participants.

## Characteristics of ongoing studies *[ordered by study ID]*

### Gustafson 2009

Trial name or title	Home rehabilitation for older people with hip fracture - a randomised controlled trial
Methods	Randomised controlled trial
Participants	200 patients aged 70 years and above who have had hip fracture surgery. Live in the municipality of Umeå (both independent living and from institutions).
Interventions	<p>1. Multidisciplinary home rehabilitation; these patients will have home rehabilitation as long as necessary but not longer than 10 weeks.</p> <p>2. Conventional multidisciplinary rehabilitation at hospital; these patients will be at hospital as long as necessary.</p> <p>Both groups will have comprehensive geriatric assessments, management and rehabilitation with active prevention, detection and treatment of postoperative complications such as falls, delirium, pain and infections, for example. Those randomised to home rehabilitation will receive some of these assessments and rehabilitation at home.</p>
Outcomes	<p>Follow-up at 12 weeks after randomisation and 12 months after surgery.</p> <p>Primary outcomes:</p> <ol style="list-style-type: none"> <li>1. In-patient days and re-admissions</li> <li>2. Complications, for example falls and injuries</li> <li>3. Depression, assessed by the Geriatric Depression Scale (GDS)</li> <li>4. Delirium, assessed by the Organic Brain Syndrome Scale</li> <li>5. Functional performance regarding activities of daily living (ADL), assessed by the Katz ADL index and Barthel ADL index</li> <li>6. Walking ability and use of walking aids, assessed by the Swedish version of the Clinical Outcome Variable Scale (S-COVS).</li> </ol> <p>Secondary outcomes</p> <ol style="list-style-type: none"> <li>1. Balance, assessed by the Bergs Balance Scale and Fear of Falling</li> <li>2. Quality of life, assessed by EQ-5D</li> <li>3. Nutritional status, assessed by the mini nutritional assessment (MNA)</li> <li>4. Costs</li> <li>5. Self-perceived health, assessed by the 36-item Short Form (SF-36) health survey</li> <li>6. Pain, assessed by a visual analogue scale (VAS)</li> <li>7. Subjective well-being, assessed by the Philadelphia Geriatric Morale Scale (PGCM)</li> </ol>
Starting date	01/08/2008 to 01/08/2010
Contact information	<p>Professor Yngve Gustafson          Umeå University          Geriatric Medicine          Umeå          SE-901 87          Sweden</p>
Notes	

**Sletvold 2009**

Trial name or title	A new clinical pathway for patients with fractured neck of femur - the Orthopaedics operate, the Geriatricians manage the medical work-up
Methods	Randomised, single-blind (outcomes assessor) controlled trial
Participants	400 patients, aged 70 years or above, with hip fracture. Live in nearby municipalities, able to walk 10 m before fracture. Excluded: nursing home residents, expected to die within 3 months, pathological fractures.
Interventions	1. Pre- and postoperative treatment in an orthogeriatric unit (geriatric evaluation and management) 2. Treatment on ordinary orthopaedic unit No specific follow-up after discharge from hospital
Outcomes	Follow-up: 1, 4 and 12 months post-surgery 1. Primary outcome: mobility 2. Secondary outcome: site of residence
Starting date	April 2008 to April 2013
Contact information	Olav Sletvold or Pernille Thingstad St Olav University Hospital Trondheim Norway, 7040
Notes	



## DATA AND ANALYSES

### Comparison 1. Multidisciplinary inpatient rehabilitation versus usual care

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 'Poor outcome' (long-term follow-up)	8	1633	Risk Ratio (M-H, Fixed, 95% CI)	0.89 [0.78, 1.01]
2 'Poor outcome' (long-term follow-up) - Kennie 1988	7	1525	Risk Ratio (M-H, Fixed, 95% CI)	0.93 [0.82, 1.07]
3 'Poor outcome' (long-term follow-up) by intervention type	8	1633	Risk Ratio (M-H, Fixed, 95% CI)	0.89 [0.78, 1.01]
3.1 Specialist unit	3	586	Risk Ratio (M-H, Fixed, 95% CI)	0.75 [0.60, 0.94]
3.2 Intensive inpatient rehabilitation programme	5	1047	Risk Ratio (M-H, Fixed, 95% CI)	0.97 [0.83, 1.13]
4 'Poor outcome' (long-term follow-up) by selection bias	8	1633	Risk Ratio (M-H, Fixed, 95% CI)	0.89 [0.78, 1.01]
4.1 No selection bias	3	760	Risk Ratio (M-H, Fixed, 95% CI)	0.97 [0.78, 1.21]
4.2 Unclear or high risk of selection bias	5	873	Risk Ratio (M-H, Fixed, 95% CI)	0.84 [0.71, 0.98]
5 'Poor outcome' (at discharge)	7	1331	Risk Ratio (M-H, Fixed, 95% CI)	0.87 [0.75, 1.00]
6 Mortality (end of scheduled follow-up)	11	2334	Risk Ratio (M-H, Fixed, 95% CI)	0.90 [0.76, 1.07]
7 Mortality (end of scheduled follow-up) - with 12 month data for Cameron 1993	11	2334	Risk Ratio (M-H, Fixed, 95% CI)	0.88 [0.74, 1.04]
8 Mortality (at discharge)	9	1920	Risk Ratio (M-H, Fixed, 95% CI)	0.77 [0.56, 1.06]
9 Functional outcomes	5		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected
9.1 More dependent (based on Katz index) at 1 year	2		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
9.2 More dependent or dead at 1 year	2		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
9.3 Non-recovery/decline in walking at long-term follow-up	2		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
9.4 Non-recovery in walking or dead at long-term follow-up	2		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
9.5 Decline in transfers (bed to chair etc) at long-term follow-up	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
9.6 Decline in transfers or dead at long-term follow-up	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
9.7 Non-recovery in activities of daily living (ADL) at 1 year	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
9.8 Non-recovery in ADL or dead at 1 year	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
9.9 Incomplete recovery of ADL and mobility at 1 year	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable

9.10 Incomplete recovery of ADL and mobility, or dead at 1 year	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
10 Functional outcomes: Barthel scores at long-term follow-up	2		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
10.1 Chinese Barthel Index at 6 months	1		Mean Difference (IV, Fixed, 95% CI)	Not estimable
10.2 Modified Barthel Index at 6 months	1		Mean Difference (IV, Fixed, 95% CI)	Not estimable
11 Complications	3		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected
11.1 pressure sores	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
11.2 heart failure	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
11.3 pneumonia	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
11.4 confusion	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
11.5 chest infection, cardiac problem, bedsore	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
11.6 stroke, emboli	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
11.7 any complication	2		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
12 Length of hospital stay	8		Mean Difference (IV, Random, 95% CI)	Totals not selected
13 Readmitted to hospital during follow-up	6	1269	Risk Ratio (M-H, Fixed, 95% CI)	0.99 [0.82, 1.19]
14 Dead or readmitted to hospital during follow-up	5	1221	Risk Ratio (M-H, Fixed, 95% CI)	0.93 [0.79, 1.09]

## Comparison 2. Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 'Poor outcome', mortality, institutional care and unable to walk (12 months)	1		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected
1.1 Poor outcome (dead or moved to higher level of care)	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
1.2 Mortality	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
1.3 Moved to higher level of care	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
1.4 Unable to walk	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
2 SF-36 scores at 12 months (0: worst to 100: best)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
2.1 Physical component summary scores	1		Mean Difference (IV, Fixed, 95% CI)	Not estimable
2.2 Mental component summary scores	1		Mean Difference (IV, Fixed, 95% CI)	Not estimable
3 Lengths of hospital or rehabilitation stays (days)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
3.1 Length of hospital stay	1		Mean Difference (IV, Fixed, 95% CI)	Not estimable

3.2 Length of rehabilitation (hospital + home)	1		Mean Difference (IV, Fixed, 95% CI)	Not estimable
4 Readmission to hospital during 4 months follow-up	1		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected

### Comparison 3. Intensive versus less intensive community rehabilitation

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 'Poor outcome', mortality and institutional care (12 months)	1		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected
1.1 Poor outcome (dead or in institutional care)	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
1.2 Mortality	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
1.3 Institutional care	1		Risk Ratio (M-H, Fixed, 95% CI)	Not estimable
2 Number of contacts over 12 weeks (for participants with hip fracture or stroke)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected

### Comparison 4. Exploratory analysis: all settings

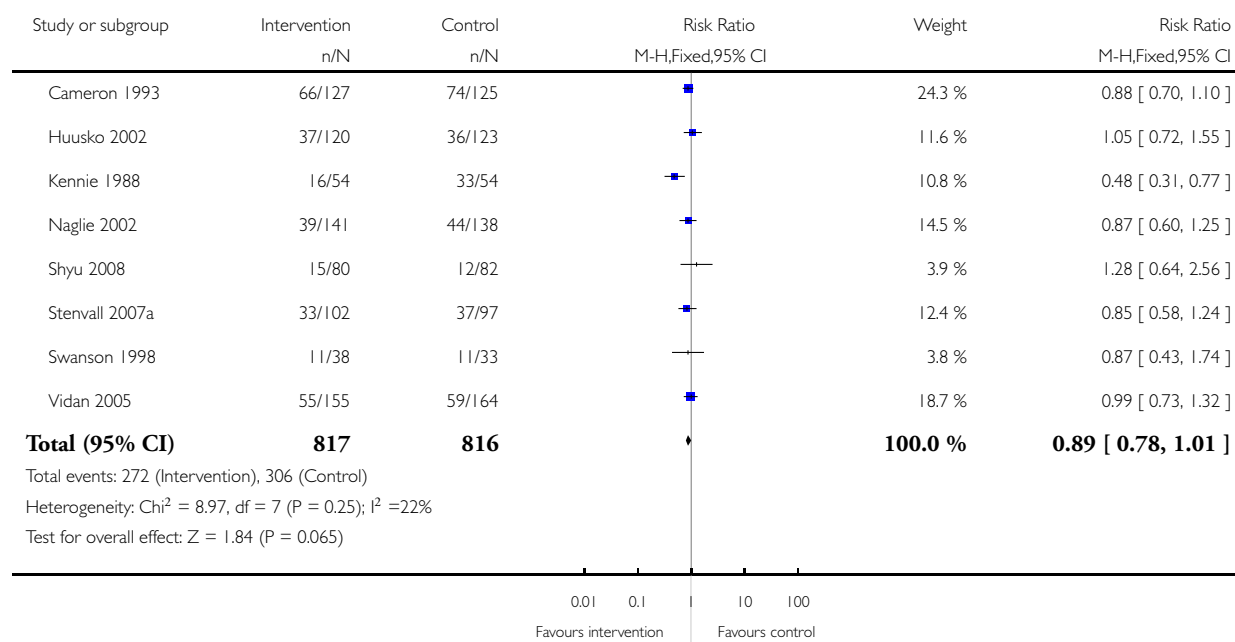
Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 'Poor outcome' (long-term follow-up) by intervention type + Crotty 2003	9	1699	Risk Ratio (M-H, Fixed, 95% CI)	0.88 [0.78, 1.00]
1.1 Specialist unit	3	586	Risk Ratio (M-H, Fixed, 95% CI)	0.75 [0.60, 0.94]
1.2 Intensive rehabilitation programme	6	1113	Risk Ratio (M-H, Fixed, 95% CI)	0.96 [0.82, 1.12]

### Analysis 1.1. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 1 'Poor outcome' (long-term follow-up).

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 1 'Poor outcome' (long-term follow-up)

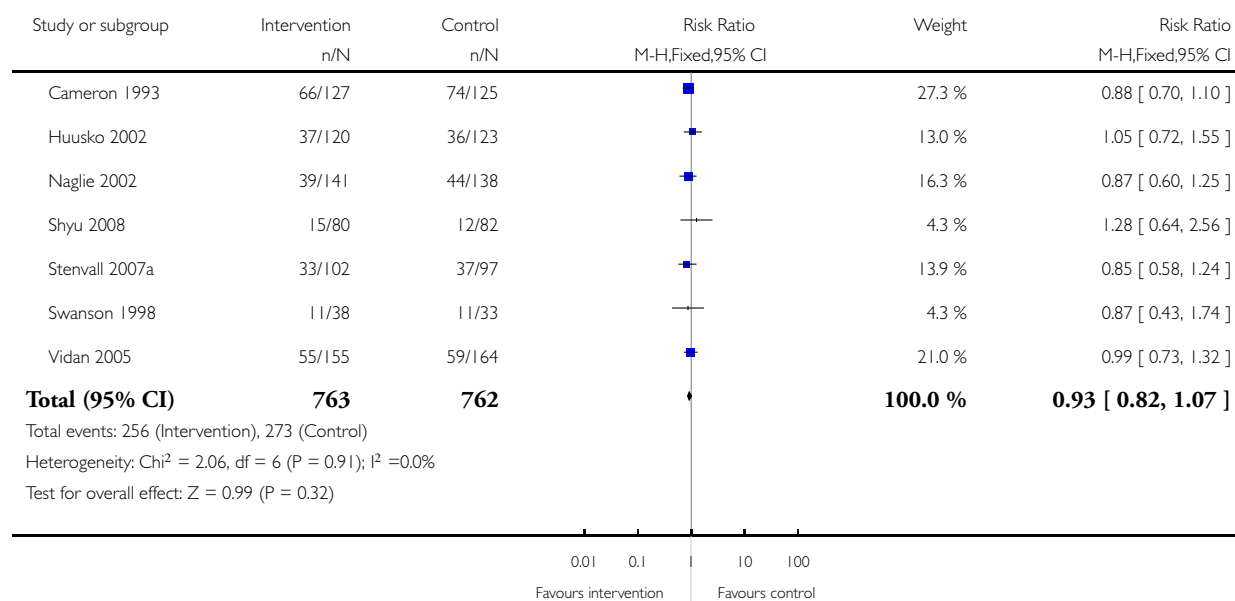


### Analysis 1.2. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 2 'Poor outcome' (long-term follow-up) - Kennie 1988.

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 2 'Poor outcome' (long-term follow-up) - Kennie 1988

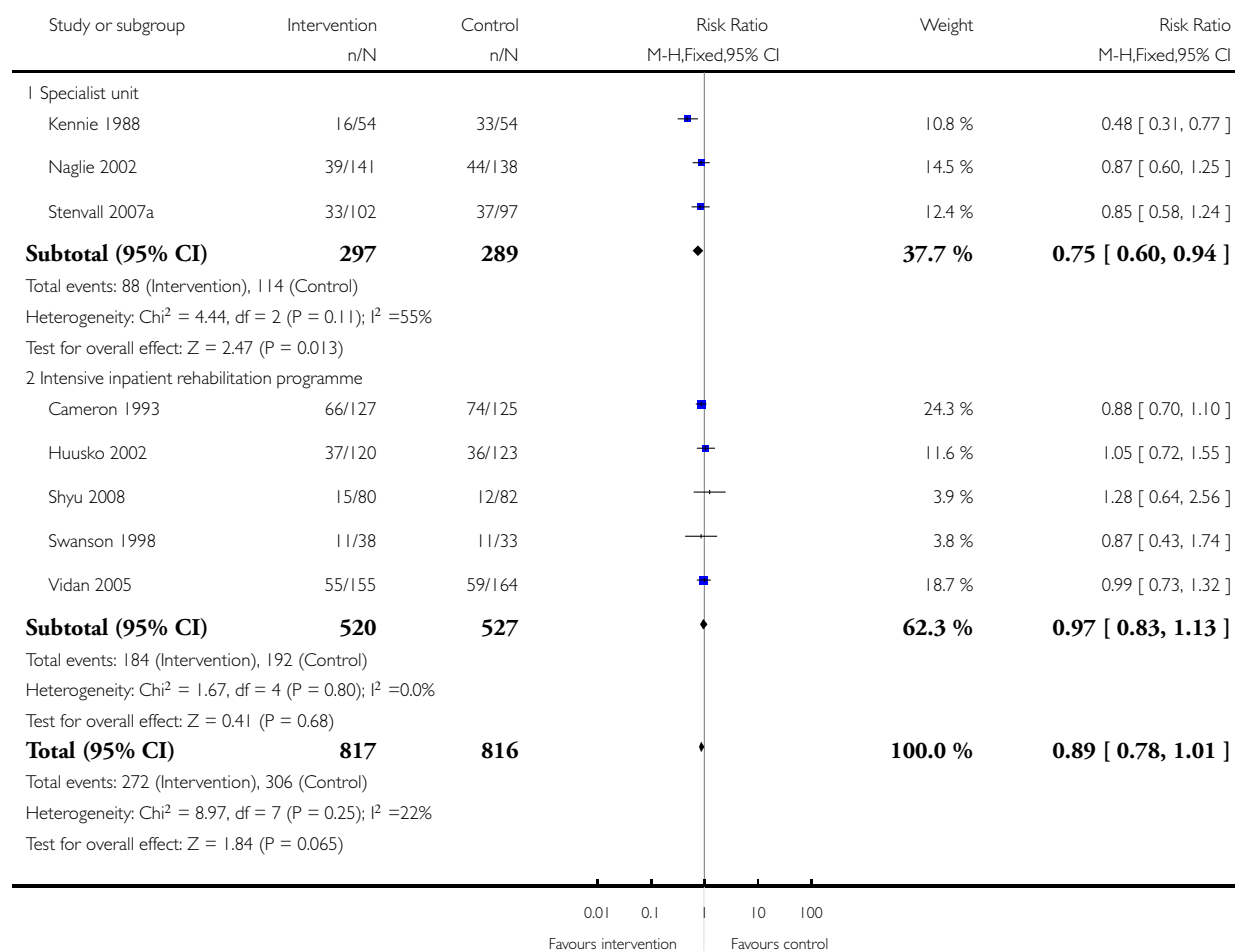


### Analysis 1.3. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 3 'Poor outcome' (long-term follow-up) by intervention type.

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 3 'Poor outcome' (long-term follow-up) by intervention type

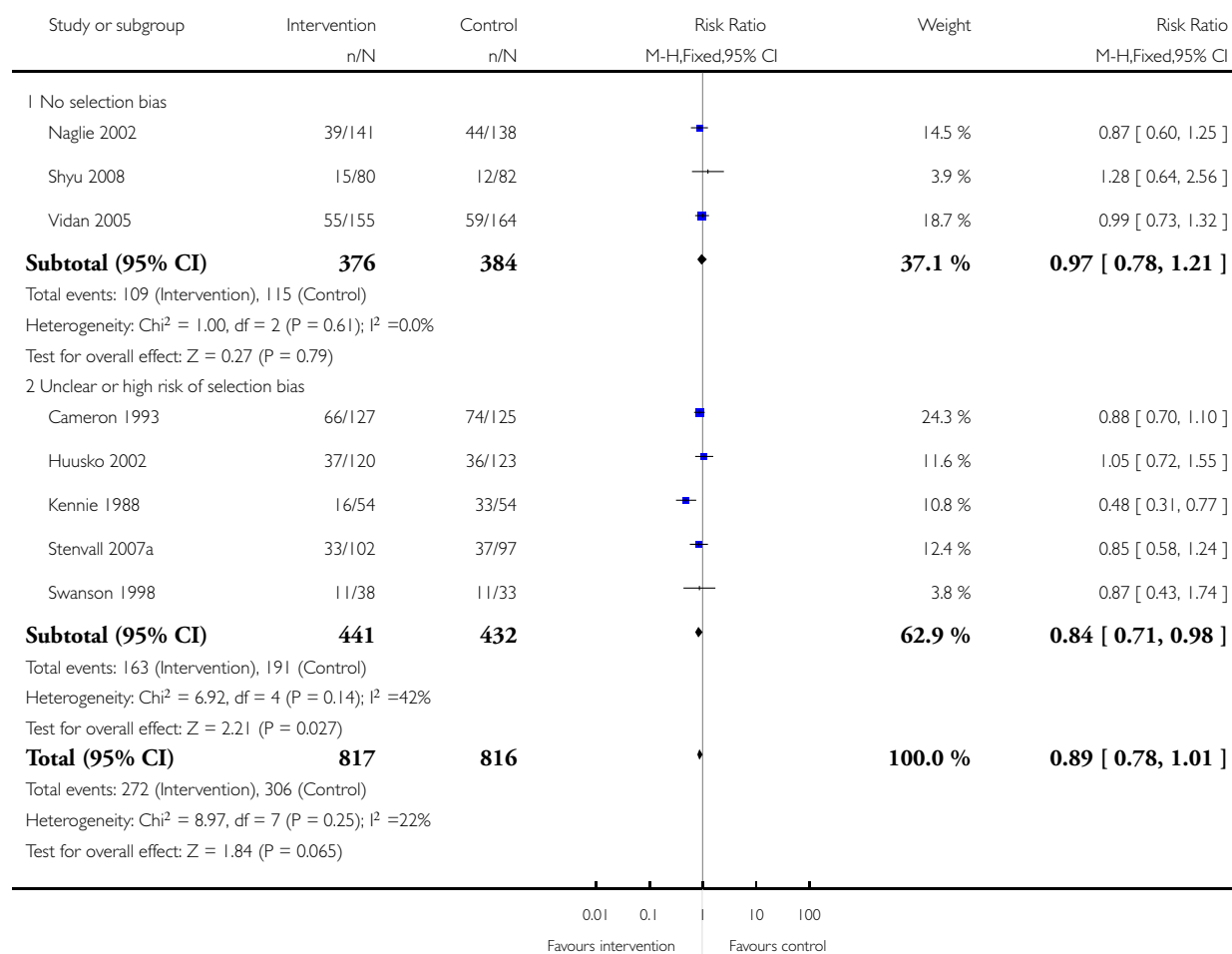


### Analysis 1.4. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 4 'Poor outcome' (long-term follow-up) by selection bias.

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 4 'Poor outcome' (long-term follow-up) by selection bias

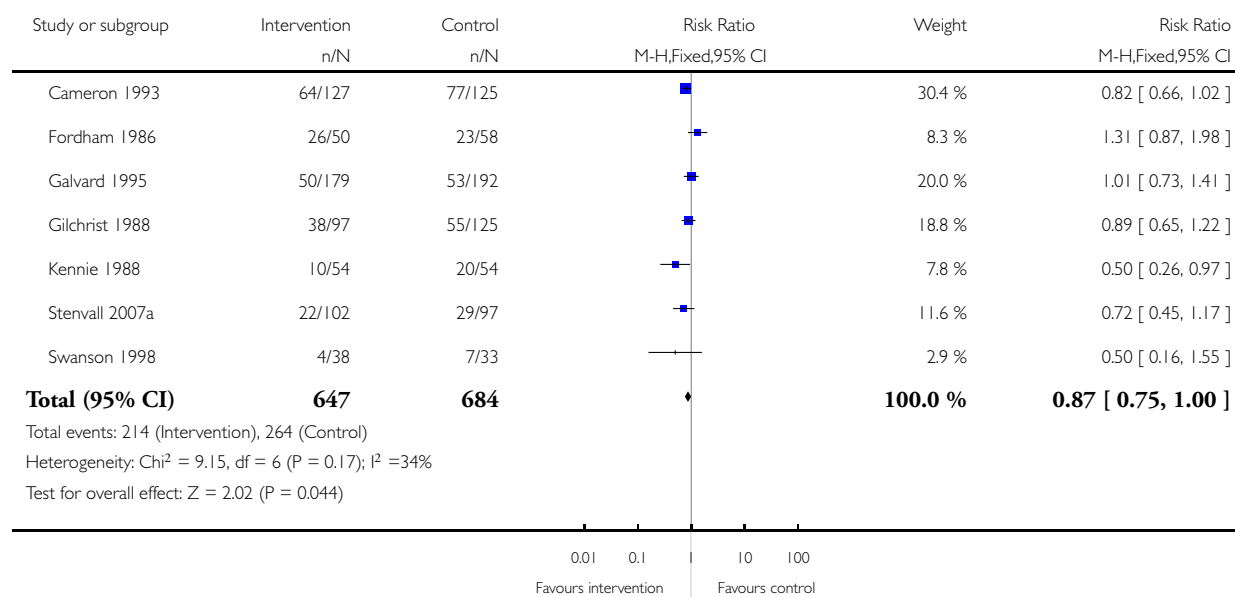


### Analysis 1.5. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 5 'Poor outcome' (at discharge).

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 5 'Poor outcome' (at discharge)



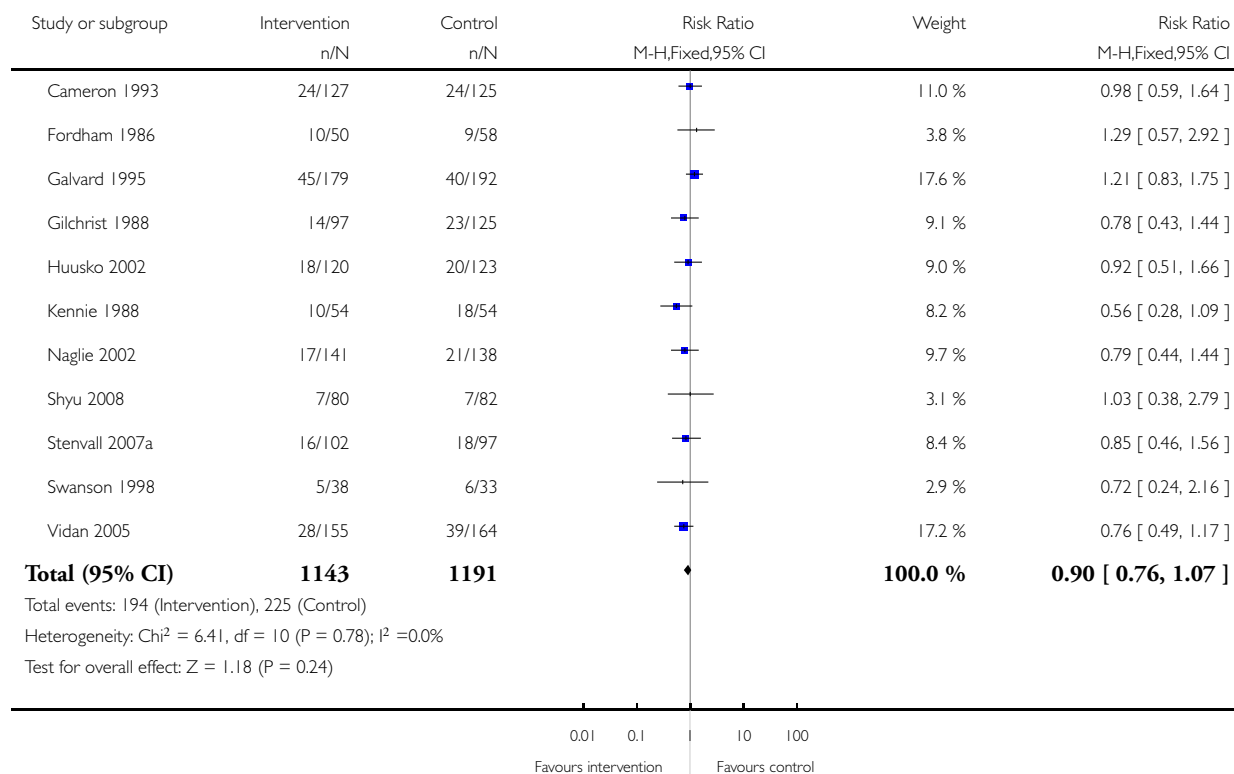


### Analysis 1.6. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 6 Mortality (end of scheduled follow-up).

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 6 Mortality (end of scheduled follow-up)

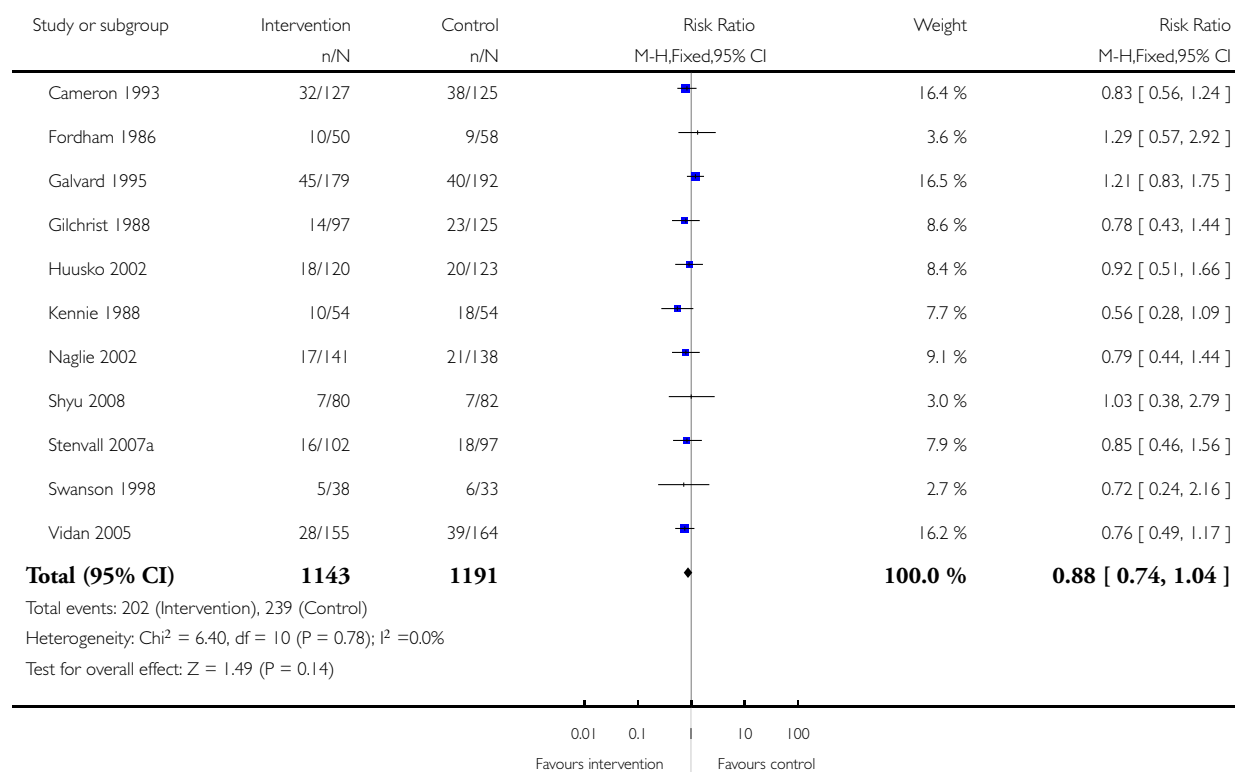


### Analysis 1.7. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 7 Mortality (end of scheduled follow-up) - with 12 month data for Cameron 1993.

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 7 Mortality (end of scheduled follow-up) - with 12 month data for Cameron 1993

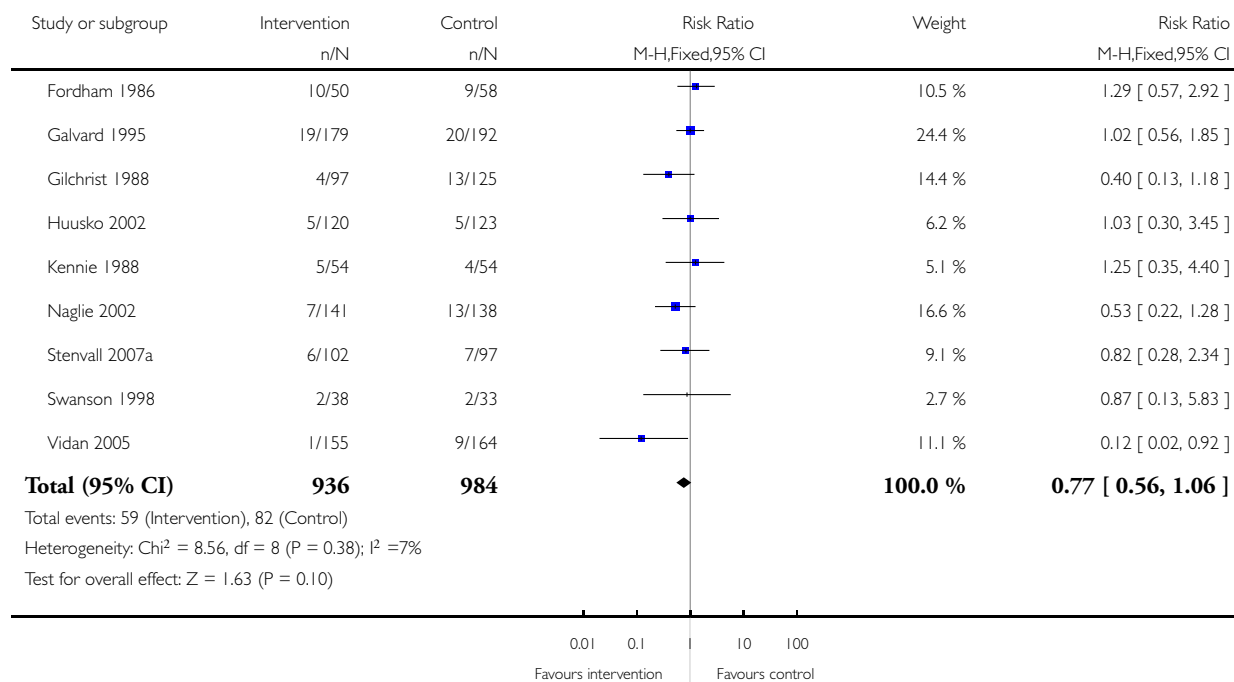


### Analysis 1.8. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 8 Mortality (at discharge).

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 8 Mortality (at discharge)

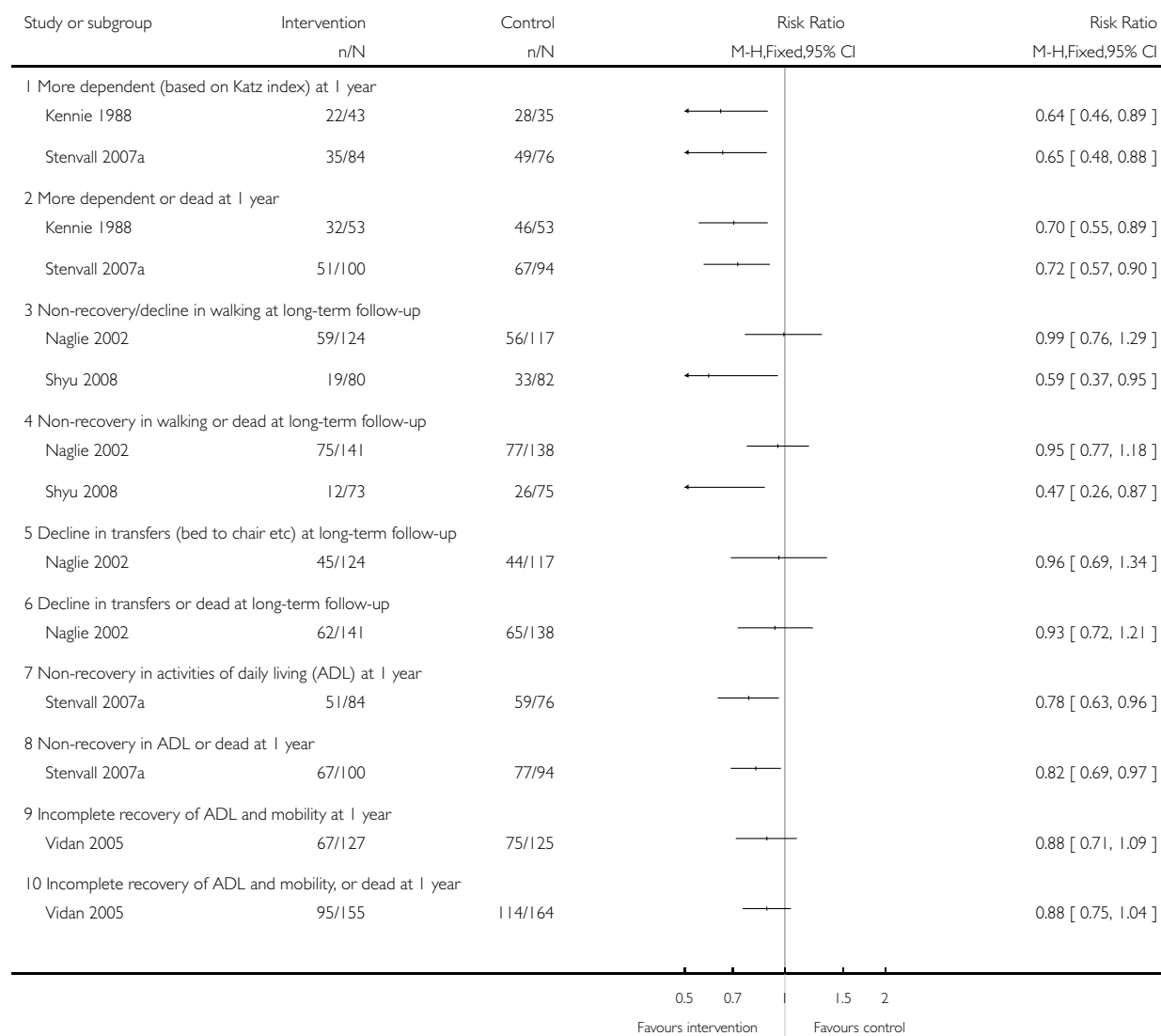


### Analysis 1.9. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 9 Functional outcomes.

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 9 Functional outcomes

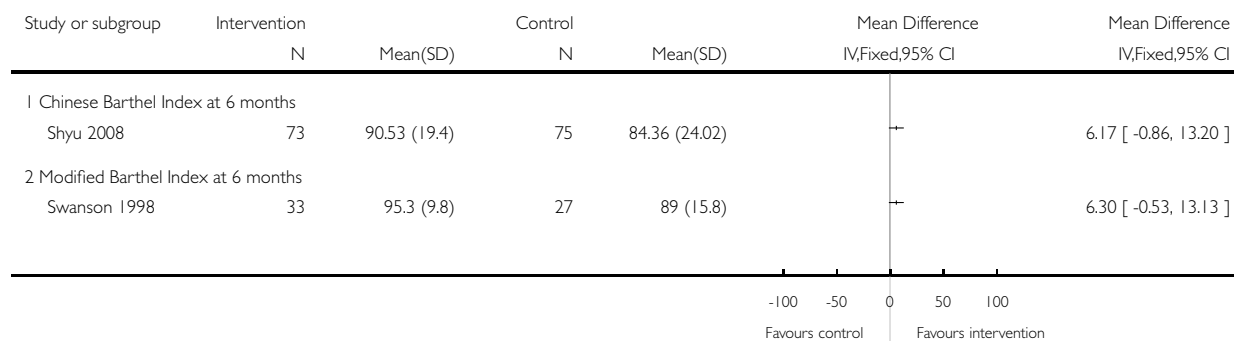


**Analysis 1.10. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 10 Functional outcomes: Barthel scores at long-term follow-up.**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 10 Functional outcomes: Barthel scores at long-term follow-up

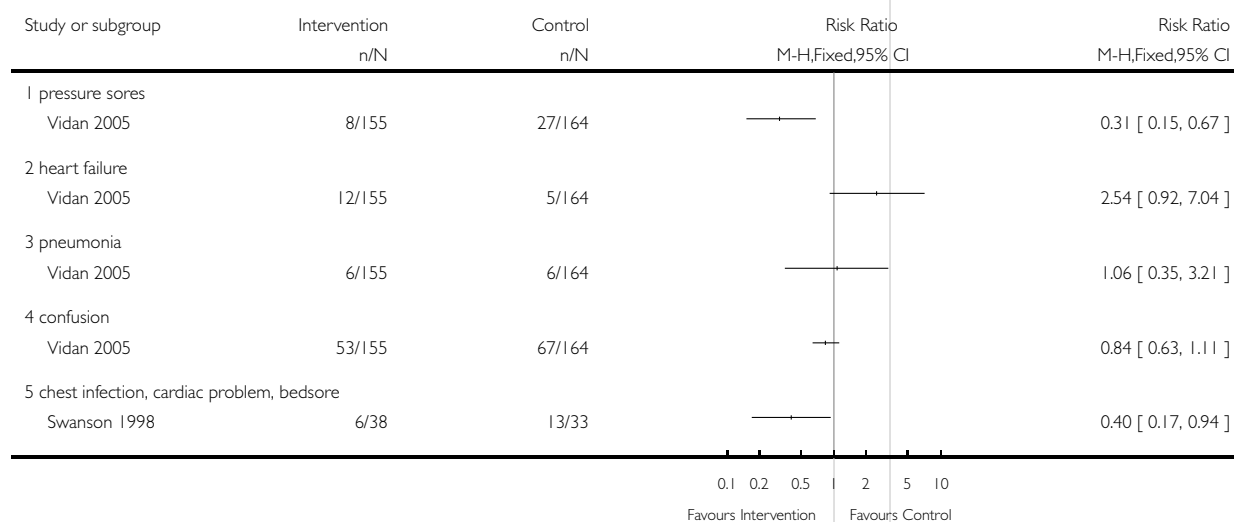


**Analysis 1.11. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 11 Complications.**

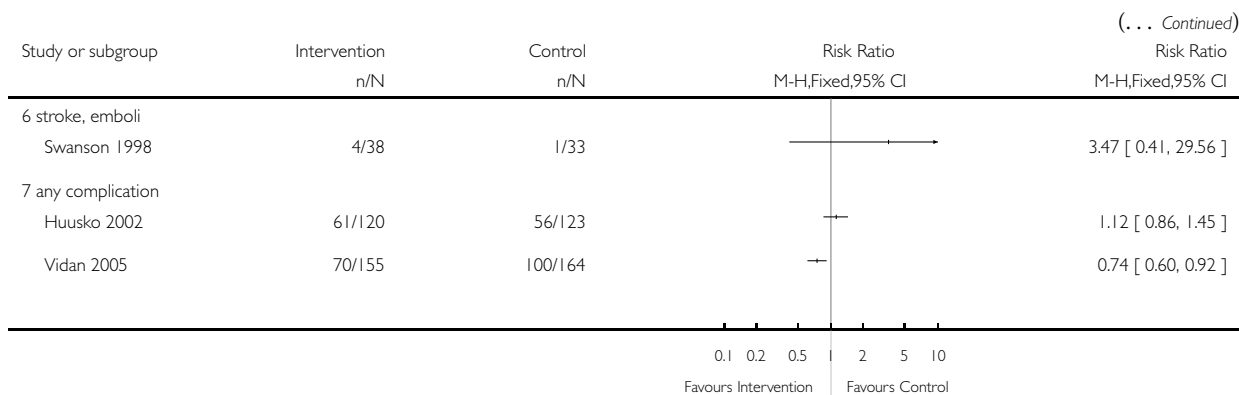
Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 11 Complications



(Continued ...)

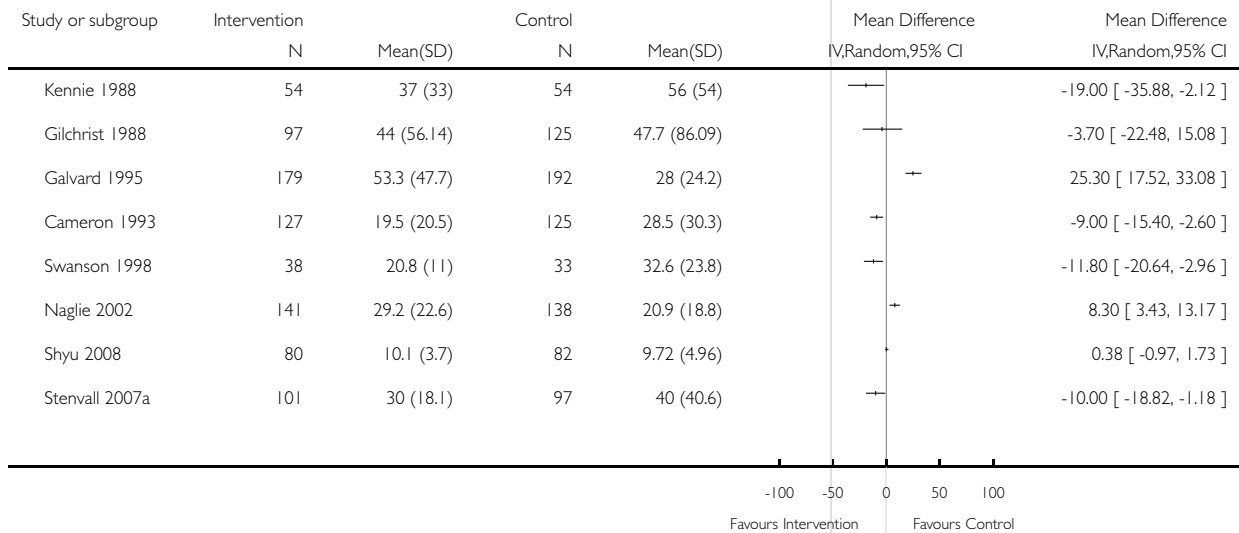


**Analysis 1.12. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 12 Length of hospital stay.**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 12 Length of hospital stay

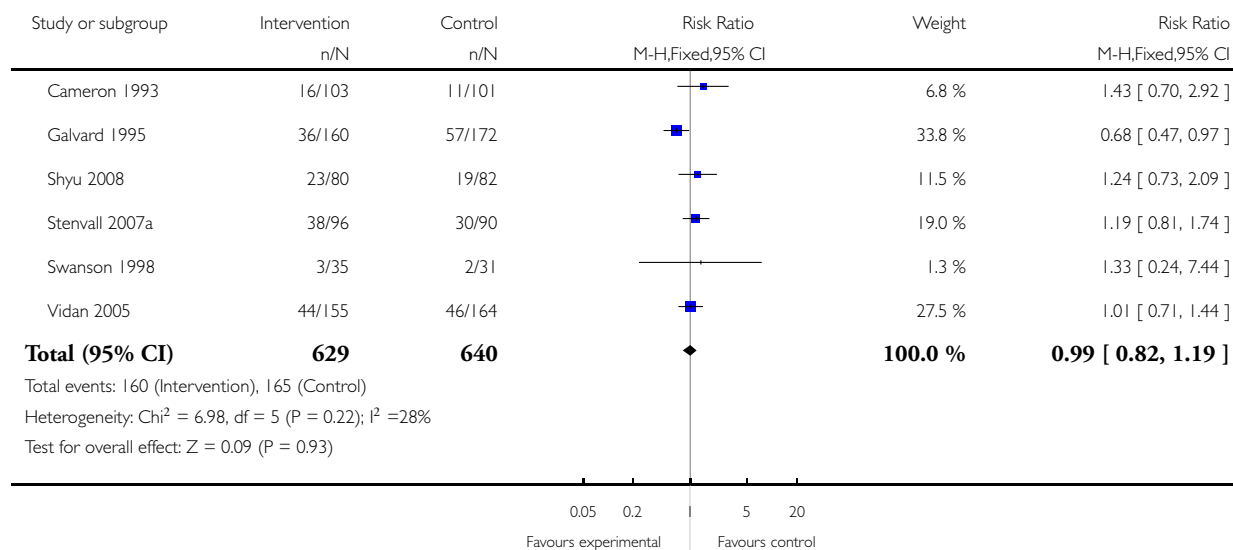


### Analysis 1.13. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 13 Readmitted to hospital during follow-up.

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 13 Readmitted to hospital during follow-up

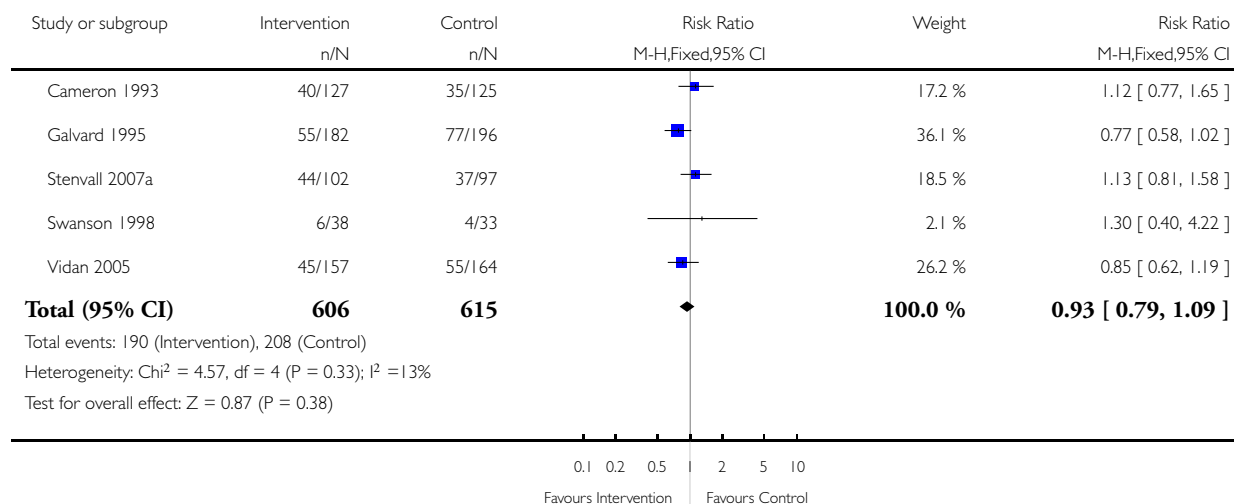


### Analysis 1.14. Comparison 1 Multidisciplinary inpatient rehabilitation versus usual care, Outcome 14 Dead or readmitted to hospital during follow-up.

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 1 Multidisciplinary inpatient rehabilitation versus usual care

Outcome: 14 Dead or readmitted to hospital during follow-up



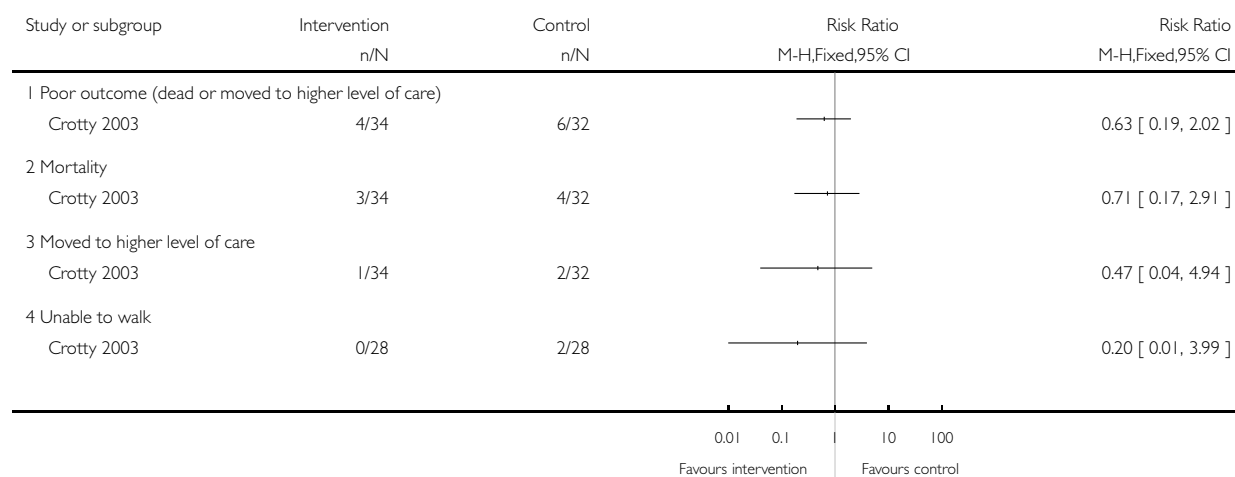


**Analysis 2.1. Comparison 2 Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation, Outcome 1 'Poor outcome', mortality, institutional care and unable to walk (12 months).**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 2 Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation

Outcome: 1 'Poor outcome', mortality, institutional care and unable to walk (12 months)

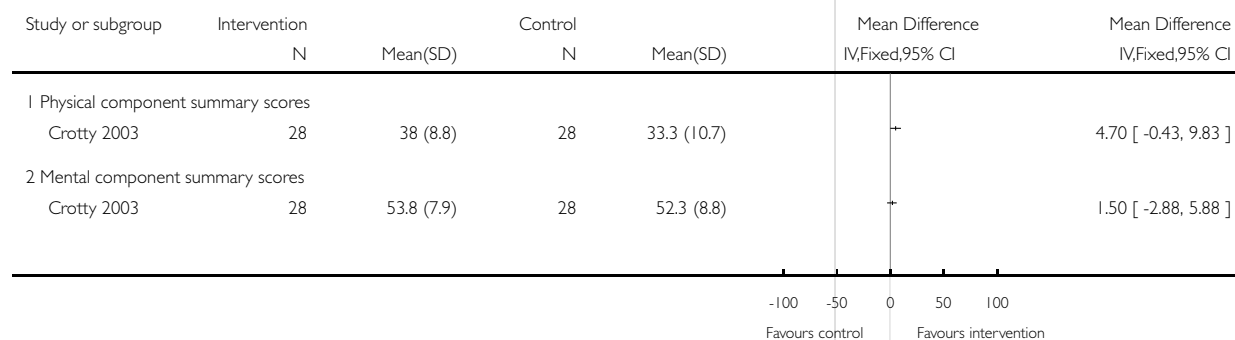


**Analysis 2.2. Comparison 2 Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation, Outcome 2 SF-36 scores at 12 months (0: worst to 100: best).**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 2 Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation

Outcome: 2 SF-36 scores at 12 months (0: worst to 100: best)



**Analysis 2.3. Comparison 2 Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation, Outcome 3 Lengths of hospital or rehabilitation stays (days).**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 2 Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation

Outcome: 3 Lengths of hospital or rehabilitation stays (days)

Study or subgroup	Intervention		Control		Mean Difference IV,Fixed,95% CI	Mean Difference IV,Fixed,95% CI
	N	Mean(SD)	N	Mean(SD)		
<b>1 Length of hospital stay</b>						
Crotty 2003	32	7.8 (9.3)	34	14.3 (10.6)	-6.50	[-11.30, -1.70]
<b>2 Length of rehabilitation (hospital + home)</b>						
Crotty 2003	32	28.3 (14.5)	34	14.3 (10.6)	14.00	[7.84, 20.16]

**Analysis 2.4. Comparison 2 Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation, Outcome 4 Readmission to hospital during 4 months follow-up.**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 2 Accelerated discharge and multidisciplinary home-based rehabilitation versus usual inpatient rehabilitation

Outcome: 4 Readmission to hospital during 4 months follow-up

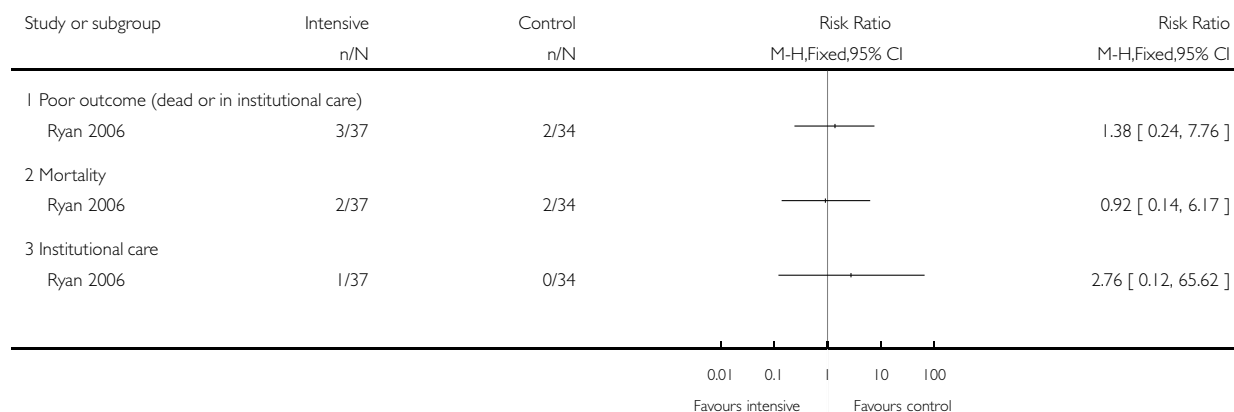
Study or subgroup	Intervention	Control	Risk Ratio M-H,Fixed,95% CI	Risk Ratio M-H,Fixed,95% CI
	n/N	n/N		
Crotty 2003	8/34	7/32	1.08	[0.44, 2.62]

**Analysis 3.1. Comparison 3 Intensive versus less intensive community rehabilitation, Outcome 1 'Poor outcome', mortality and institutional care (12 months).**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 3 Intensive versus less intensive community rehabilitation

Outcome: 1 'Poor outcome', mortality and institutional care (12 months)

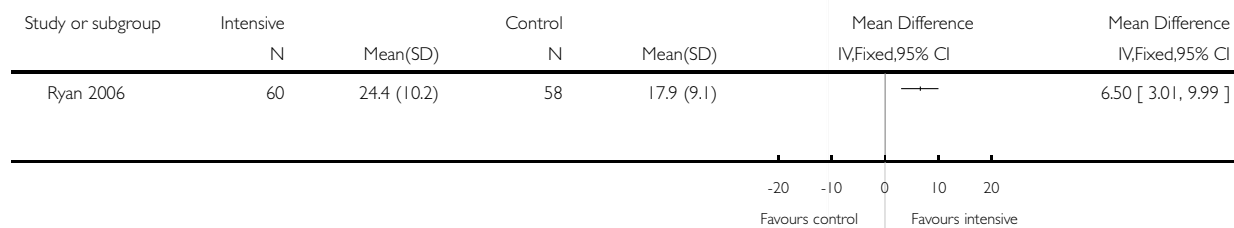


**Analysis 3.2. Comparison 3 Intensive versus less intensive community rehabilitation, Outcome 2 Number of contacts over 12 weeks (for participants with hip fracture or stroke).**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 3 Intensive versus less intensive community rehabilitation

Outcome: 2 Number of contacts over 12 weeks (for participants with hip fracture or stroke)

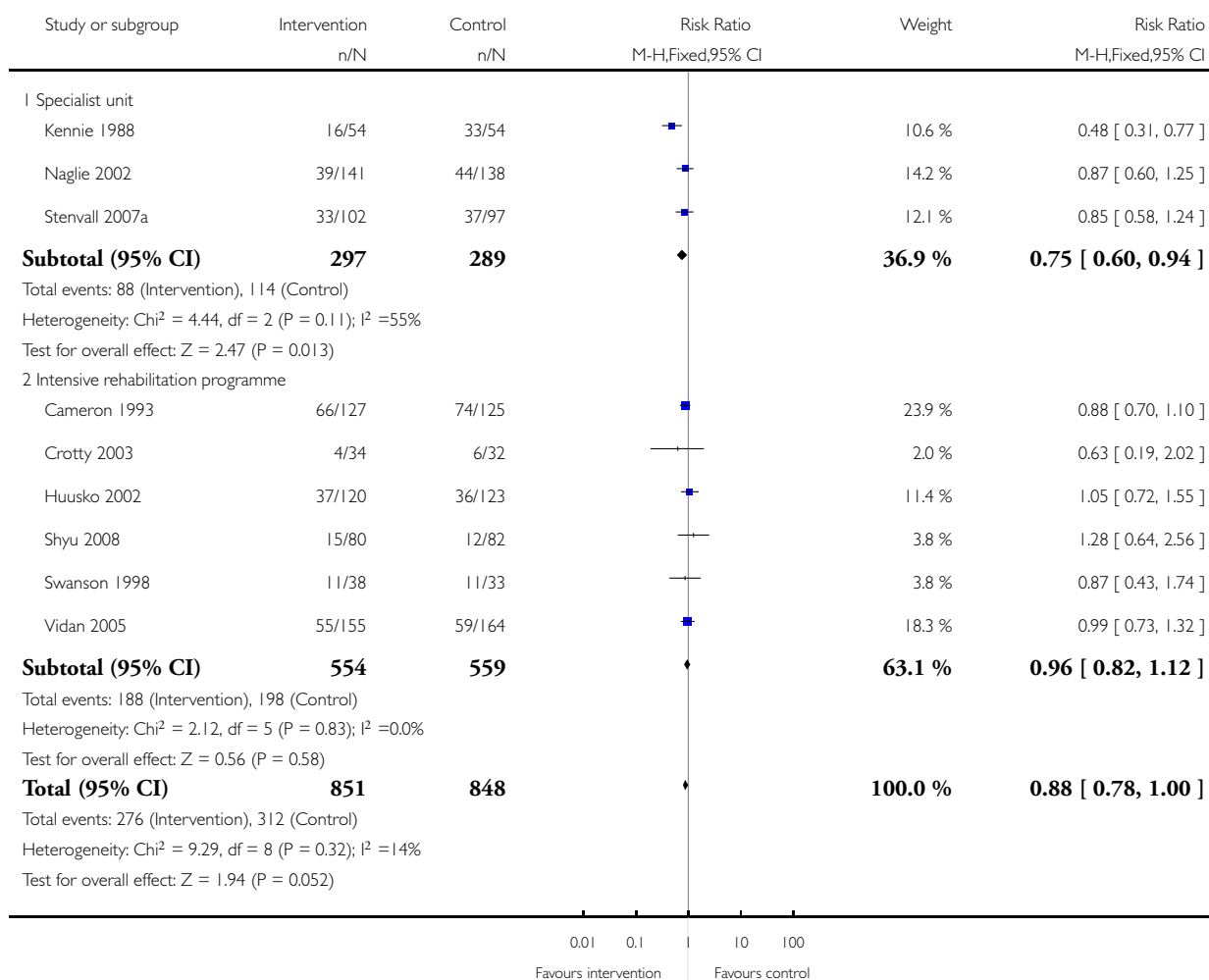


**Analysis 4.1. Comparison 4 Exploratory analysis: all settings, Outcome 1 'Poor outcome' (long-term follow-up) by intervention type + Crotty 2003.**

Review: Multidisciplinary rehabilitation for older people with hip fractures

Comparison: 4 Exploratory analysis: all settings

Outcome: 1 'Poor outcome' (long-term follow-up) by intervention type + Crotty 2003



## APPENDICES

### Appendix I. Search strategies

#### MEDLINE (Ovid interface) (1950 to April Week 3 2009)

1. exp Femoral Fractures/(3691)
2. ((hip\$ or ((femur\$ or femoral\$) adj3 (neck or proximal))) adj4 fracture\$.tw. (2817)
3. or/1-2 (4577)
4. Physical Therapy Modalities/or rehabilitation/or activities of daily living/ or early ambulation/or occupational therapy/ or Critical Pathways/ or "Physical Therapy (Specialty)"/(15382)
5. Rehabilitation Nursing/ (219)
6. (rehab\$ or (early adj1 (mobil\$ or discharg\$ or ambulat\$)) or occupational therap\$ or physiotherap\$ or physical therap\$ or multi-disciplin\$.tw. (27577)
7. (geriatr\$ or geriatr\$-orthop\$ or orthop?edic-geriatr\$ or ortho\$-geriatr\$ or orthogeriatr\$.tw (4138)
8. hospital at home.tw. (45)
9. exp Hospitalization/ (24901)
10. or/4-9 (63878)
11. and/3,10 (642)
12. exp Aged/ (338899)
13. and/11-12 (513)
14. randomized controlled trial.pt. (64865)
15. controlled clinical trial.pt. (9265)
16. Randomized Controlled Trials/ (22270)
17. Random Allocation/ (10520)
18. Double Blind Method/ (18830)
19. Single Blind Method/ (3730)
20. or/14-19 (108232)
21. Animals/not Humans/ (414752)
22. 20 not 21 (96954)
23. clinical trial.pt. (44586)
24. exp Clinical Trials as topic/ (47885)
25. (clinic\$ adj25 trial\$.tw. (49616)
26. ((singl\$ or doubl\$ or trebl\$ or tripl\$) adj25 (blind\$ or mask\$)).tw. (19461)
27. Placebos/ (3336)
28. placebo\$.tw. (25335)
29. random\$.tw. (125956)
30. Research Design/ (14640)
31. or/23-30 (230596)
32. 31 not 21 (212297)
33. 32 not 22 (131263)
34. or/22,33 (228217)
35. and/13,34 (92)

#### EMBASE (Ovid interface) (1980 to 2009 week 16)

1. exp Hip Fracture/ (2144)
2. ((hip\$ or ((femur\$ or femoral\$) adj3 (neck or proximal))) adj4 fracture\$.tw. (1487)
3. or/1-2 (2438)
4. exp Randomized Controlled trial/ (29184)
5. exp Double Blind Procedure/(8887)
6. exp Single Blind Procedure/(1679)
7. exp Crossover Procedure/(2802)
8. Controlled Study/ (491098)

9. or/4-8 (497624)
10. ((clinical or controlled or comparative or placebo or prospective\$ or randomi#ed) adj3 (trial or study)).tw. (51634)
11. (random\$ adj7 (allocat\$ or allot\$ or assign\$ or basis\$ or divid\$ or order\$)).tw. (14498)
12. ((singl\$ or doubl\$ or trebl\$ or tripl\$) adj7 (blind\$ or mask\$)).tw. (10097)
13. (cross?over\$ or (cross adj1 over\$)).tw. (4313)
14. ((allocat\$ or allot\$ or assign\$ or divid\$) adj3 (condition\$ or experiment\$ or intervention\$ or treatment\$ or therap\$ or control\$ or group\$)).tw. (17007)
15. or/10-14 (76261)
16. or/9,15 (518123)
17. limit 16 to human (331869)
18. and/3,17 (921)

## HISTORY

Protocol first published: Issue 2, 2008

Review first published: Issue 4, 2009

7 July 2008	Amended	Converted to new review format
19 February 2008	Amended	This protocol, published in Issue 2, 2008, precedes the update and scope expansion of a former review ("Co-ordinated multidisciplinary approaches for inpatient rehabilitation of older patients with proximal femoral fractures"; last updated Issue 2, 2003), which included inpatient rehabilitation only.

## CONTRIBUTIONS OF AUTHORS

The contributions of the authors to the former review ("Co-ordinated multidisciplinary approaches for rehabilitation of older patients with proximal femoral fractures"), from which this review is derived, were described in that document. Ian Cameron and Helen Handoll prepared the initial and subsequent drafts of the protocol for this review, which incorporated feedback and suggestions for rewrites from the other two co-authors, Terry Finnegan and Peter Langhorne.

Helen Handoll co-ordinated the production of the review, performed searches, located studies and contacted trialists. All four authors participated in study selection, and assessed risk of bias and extracted data from trial reports. Helen Handoll wrote the first draft and performed the primary data analyses and, with some contribution from Jenson Mak, entered trial data into RevMan. All reviewers critically reviewed subsequent drafts and commented on any re-analyses and revisions at all stages. Helen Handoll and Ian Cameron are the guarantors of the review.

## DECLARATIONS OF INTEREST

As Ian Cameron was the principal investigator of one of the included trials, this trial was assessed independently by other review authors.

## SOURCES OF SUPPORT

### Internal sources

- University of Teesside, Middlesbrough, UK.
- Rehabilitation Studies Unit, Faculty of Medicine, University of Sydney, Australia.

### External sources

- The Cochrane Collaboration, Prioritisation Project Fund, Not specified.  
Funding for completion of priority review

## DIFFERENCES BETWEEN PROTOCOL AND REVIEW

We renamed 'outpatient' rehabilitation as 'ambulatory' rehabilitation because the latter term better covers the various setting: home, outpatient department or day hospital.

We clarified that supervision of multidisciplinary care, particularly in the ambulatory setting, could be by a specialist clinician other than a physician.

We assessed risk of bias using the tool outlined in the Cochrane Handbook for Systematic Reviews of Interventions.

## NOTES

The title of the former review ([Cameron 2001](#)) was: "Co-ordinated multidisciplinary approaches for inpatient rehabilitation of older patients with proximal femoral fractures". The protocol for [Cameron 2001](#) was published in Issue 2, 1995; and the first version of the review appeared in Issue 2, 1997. This was followed by two substantive updates published in Issue 4, 2000 and Issue 3, 2001 respectively, and then by two minor updates published in Issue 2, 2002 and Issue 2, 2003 respectively.