

# Home-care 're-ablement' services for maintaining and improving older adults' functional independence (Protocol)

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[Intervention Protocol]

# Home-care 're-ablement' services for maintaining and improving older adults' functional independence

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## ABSTRACT

This is the protocol for a review and there is no abstract. The objectives are as follows:

To assess the effects of home-care 're-ablement' services compared to usual care, or to a wait list control group, in terms of maintaining and improving the functional independence of older adults.

## BACKGROUND

### Description of the condition

As the population ages and people live longer, the proportion of dependent older people is likely to increase (Brodsky 2003; Wittenberg 2004). As a result, the cost of long-term care for people aged over 65 years living in Organisation for Economic Co-operation and Development (OECD) countries is expected to double or even triple by 2050 (Oliveira Martins 2006). Therefore, many high-income countries have actively promoted a shift from residential to home-based care as a potentially more effective and financially sustainable approach to meeting the health and social care needs of older adults (Rostgaard 2011). Importantly, most older people prefer to 'age in place' (Wiles 2012) and, therefore, to

remain in their own homes for as long as possible, provided they have appropriate levels of support to meet their (changing) needs (Cutchin 2009).

Government policies in various countries reflect the need to reconfigure health and social service provision in order to meet the current and future requirements of an ageing population. In England, for example, the Department of Health has articulated a vision for the integration of health and social care services, with a greater focus on individualised preventative services to delay the need for more costly forms of care (Xie 2012). Similar themes of early intervention, person-centred care and restoration of function have emerged in Australia (Cartwright 2009), Sweden (Löfqvist 2012) and New Zealand (King 2011) mainly with a view to reducing pressure on the system. However, despite these changes, little is known about the effectiveness and cost-effectiveness of models of

care provision across different geographical and socio-economic contexts.

## Description of the intervention

In recent years, there has been increasing international interest in 're-ablement' (also known as 'restorative' care in Australia and the USA); an innovative approach to improving home-care services for older adults in need of care and support or at risk of functional decline (Francis 2011). There is a lack of clarity regarding the boundaries between 're-ablement' and other related interventions in health and social care (including intermediate care, occupational therapy and traditional domiciliary care) (Wood 2012). While 're-ablement' shares features with other interventions, it is distinguished by a re-orientation of home care away from treating disease and creating dependency to maximising independence, by offering intensive (i.e. multiple visits) and time-limited (typically 6 to 12 weeks duration), multidisciplinary, person-centred, and goal-directed home-care services (Ryburn 2009). It is important to note that 're-ablement' is not designed to resolve specific health care issues (for example Crotty 2010), but may help an older person to regain confidence and functional abilities after recovering from an illness or a period of hospitalisation. Therefore, a 're-ablement' programme typically includes a range of targeted components designed to optimise functioning in the performance of activities of daily living. These may include exercise and training supporting behavioural change, education about self-management and healthy ageing, environmental adjustments, provision of equipment, and use of local resources (Kent 2000; Tinetti 2002; Lewin 2010). So, for example, rather than providing a meals-on-wheels service, a 're-ablement' approach would enable an older person to develop the confidence and skills to prepare lunch through task analysis and redesign, the use of assistive technology and physical exercises (Glendinning 2010).

'Re-ablement', therefore, contrasts with usual home care/domiciliary care which tends to focus on doing things for older people rather than enabling/re-abling them to do things for themselves. Indeed, traditional models of home care have been shown to increase dependency, with an associated loss of function (Parsons 2013). Furthermore, the assumption underpinning usual home-care services is that they will continue indefinitely (Montgomery 2008), whereas 're-ablement' is specifically time-limited and aims to reduce the need for home care into the future (Ryburn 2009; King 2011). 'Re-ablement', therefore, is particularly valued for its potential to decrease demand on home-care services and to reduce the attendant costs of ongoing care (Jones 2009). Nevertheless, this form of care provision may have considerable resource implications in terms of re-training staff and effecting organisational change (Francis 2011).

The 're-ablement' approach has become increasingly popular and has been implemented widely in the UK (Department of Health 2010) and adopted in a number of other countries (e.g., New

Zealand (King 2011; Parsons 2013), Australia (Ryburn 2009), USA (Tinetti 2002)). The provision of 're-ablement' reflects a wider change agenda that promotes person-centred care through individually tailored services that permit greater choice and control for consumers (Xie 2012). Additionally, the growth in this type of approach is in line with the increasing demands of people as they age; older consumers are becoming increasingly likely to demand greater choice, more personalised services and better quality home-care support in the future (Rostgaard 2011).

## How the intervention might work

The 're-ablement' approach emphasises the active participation of an older person in working towards agreed goals that are designed to maximise independence and confidence. For example, these goals might include re-gaining confidence in self-care management and improving mobility. The content of the intervention may encompass graduated practice in completing tasks, environmental adjustments, and adaptive equipment, or enabling an older person to build up social networks (Ryburn 2009). Improved outcomes across similar domains, including self-care, mobility and quality of life have been reported (Kent 2000; Tinetti 2002). Furthermore, the ability to function effectively in the home may reduce the need for unscheduled hospital admission, and postpone or prevent admission to residential care (Tinetti 2002). A reduction in the care hours required following the intervention is frequently used as a measure of success (Kent 2000; Lewin 2010) although this may not always be a desired or possible outcome for some older people, particularly those who are socially isolated or in failing health (Francis 2011). Arguably, therefore, a decrease in hours of care with regard to older people with high dependency needs may not be an appropriate outcome measure. Importantly, additional outcomes that are valued by older people themselves as indicators of effective services should be measured (Clark 2001).

The route and threshold for entry into a 're-ablement' based service varies. Some hospital discharge support schemes select only older people who are most likely to benefit from the approach (i.e. people with relatively low levels of ongoing need), whereas a 're-ablement' service that takes referrals directly from the community may adopt a more flexible approach and screen out only those people who are terminally ill or who have advanced dementia (Glendinning 2010). Nevertheless, it seems likely that outcomes will vary depending on the route of entry and also on the functional abilities of the older person on entry to the service. For instance, those with a high level of need may not benefit as much as those with lower support requirements (Francis 2011). Indeed, 're-ablement' represents only one end of the continuum of care and may not be suitable for people with chronic or relatively intractable problems such as dementia, who may require a different type of longer term service model (CSED 2007).

## Why it is important to do this review

In recent years, there has been strong international interest in developing effective and cost-effective interventions to support older people living in their own homes and, in turn, to reduce the demand on acute hospital services and residential care provision. It has been argued that a lack of (or poorly developed) rehabilitation services has contributed to increasing pressure on acute hospital beds, delayed discharge, more frequent re-admissions to hospital and increased use of costly residential and nursing home care ([Audit Commission 2000](#)). One approach to 'freeing up' hospital beds is to support early discharge by providing acute care at home. For example, a Cochrane review ([Shepperd 2011](#)) of 'hospital at home' services found that older patients with a mix of conditions were less likely to need residential care at follow-up after receiving these services, although only a small proportion of older people were deemed to be eligible or were willing to take part.

There is currently limited evidence as to which setting or model(s) of care may be most effective for the rehabilitation and maintenance of older adults' independence ([Huss 2008](#); [Ward 2009](#)). This appears to be due, in large part, to the challenges involved in comparing different interventions containing multiple components across a range of settings. For example, [Beswick 2008](#) and [Huss 2008](#) reviewed a range of heterogeneous studies ( $n = 89$  and  $n = 21$  respectively) such as community-based nursing care following discharge from hospital, falls prevention, group education and annual health assessments. The reviews concluded that while multidimensional home-based programmes had the potential to reduce the burden of disability among older adults, it was not possible, on the basis of the available evidence, to identify which one of the various models/types of care provision was the most effective. There is a need to undertake a focused systematic review in order to assess the comparative effectiveness and disentangle the effects of each type of intervention and their potentially active ingredients or components.

Whilst a number of previous Cochrane and non-Cochrane reviews have examined a range of rehabilitation and home-visiting programmes, there has not, as yet, been a systematic review that has focused specifically on the effects of 're-ablement'-based interventions. Important questions about the effectiveness and cost-effectiveness of these types of interventions remain unanswered. For example, does 're-ablement' reduce health service utilisation (such as hospital re-admissions), do specific subgroups benefit more than others (e.g., younger populations, and those with lower levels of need), and is there evidence to support personalisation of the service? Thus, this review will address an important gap in our knowledge.

## OBJECTIVES

To assess the effects of home-care 're-ablement' services compared

to usual care, or to a wait list control group, in terms of maintaining and improving the functional independence of older adults.

## METHODS

### Criteria for considering studies for this review

#### Types of studies

Randomised controlled trials, cluster randomised trials and quasi-randomised controlled trials of 're-ablement' compared to 'usual care' (i.e., home-care support, which may include unpaid informal care) or wait list.

The inclusion of cluster randomised and quasi-randomised trials is deemed necessary in order to consider trials where individual random assignment may have been impractical due to the nature of the intervention (e.g., only the 're-ablement' intervention is available in one geographical area or there may be restrictions in terms of the availability of care staff to deliver either 're-ablement' or usual care).

We will also include studies examining the costs or cost-effectiveness of the intervention versus usual care which have been conducted alongside, or subsequent to, trials that meet the eligibility criteria ([Shemilt 2011](#)).

#### Types of participants

Older adults aged 65 years or older living in their own home who require assistance to perform tasks of daily living and to participate in normal activities due to poor physical or mental health. We will exclude trials involving older adults living outside their own homes (e.g. in nursing homes).

We may encounter trials with mixed populations in which case we will include trials with more than 80% of older adults ( $\geq 65$  years) in the overall sample. We will include trials if data about older adults can be disaggregated for analysis. Study authors will be contacted, if necessary, for additional information.

#### Types of interventions

'Re-ablement' interventions will be compared with groups receiving usual home-care services or with a wait list control group. Studies will be required to meet all of the following criteria:

1. participants must have an identified need for formal care and support or are at risk of functional decline ([Francis 2011](#))
2. the intervention must be time-limited (typically 6 to 12 weeks) and intensive (e.g., multiple home visits) ([Ryburn 2009](#));
3. the intervention must be delivered in the older person's own home ([Glendinning 2010](#));

4. the intervention must focus on maximising independence; and
5. the intervention must be person-centred and goal-directed (Parsons 2013).

We will exclude trials that focus on the provision of acute care (e.g., nursing care in the home), and those describing interventions outside of existing home-care services.

The control group will be in receipt of, or awaiting, usual home-care services, defined as ongoing assistance with completion of household activities and/or personal care by an outside agency (i.e., paid support) and/or informal (unpaid) care, with or without professional input (e.g., nurses, occupational therapists). The control group may also include those waiting for the intervention (wait list).

### Types of outcome measures

We recognise the possibility that specific outcomes may be measured using different tools across trials. If this is the case we will select the primary outcome identified by the publication authors. Where no primary outcome has been identified, we will select the one specified in the sample size calculation. If there are no sample size calculations, we will rank the effect estimates and select the median effect estimate.

We will only include studies which have measured functional outcomes (e.g., activities of daily living).

### Primary outcomes

1. Functional status including independent living, and ability to complete activities of daily living (measured using scales, such as Barthel Index of Daily Living or Lawton & Brody Scale; we will also consider studies which use unvalidated measures).
2. Adverse events including mortality, hospital (re)admission.

### Secondary outcomes

- Quality of life. We will evaluate studies that assess health and/or social care-related quality of life (HRQoL; SCRQoL) using validated uni- or multi-dimensional questionnaires. Examples of generic HRQoL questionnaires include the SF-36 and EQ-5D; SCRQoL measures include ASCOT (Adult Social Care Outcome Toolkit; Netten 2011).

- User satisfaction
- Service outcomes, including level of ongoing home care service (e.g. care hours) or use of external health services.
- Living arrangements (i.e., in own home or other setting).
- Cost effectiveness (as measured by comparing the costs of the intervention versus 'usual care'; and health service utilisation). Incremental Cost-Effectiveness Ratios (ICER) are a central component of full economic evaluations. However, full economic evaluations of 're-ablement' interventions may be relatively rare (e.g., Pilkington 2011) and as such, we will also

search for studies that include only costs data as long as these have been conducted alongside, or subsequent to, trials that meet the eligibility criteria.

### Timing of outcome assessment

All outcomes will be measured at baseline and on 'discharge' from the 're-ablement' service (typically 6 to 12 weeks). Follow-ups of 6 and 12 months will also be analysed when such data are available.

### Main outcomes for 'Summary of findings' table

We will prepare a 'Summary of findings' table to present the results of the meta-analysis, based on the methods described in the *Cochrane Handbook* (Schünemann 2011). We will present the results of meta-analysis for the major comparisons of the review for the following main outcomes:

- functional status
- adverse events
- quality of life
- level of ongoing service use
- living arrangements (i.e., living in own home or elsewhere)
- cost-effectiveness

### Search methods for identification of studies

We will identify studies through key word and text word searches of relevant electronic databases and government and non-government agencies, as well as searching grey literature (including conference papers, unpublished theses, reference lists of other rehabilitation reviews) and personal communications with experts in the field.

### Electronic searches

We will search the following electronic databases:

- Cochrane Central Register of Controlled Trials (CENTRAL, *The Cochrane Library*)
- MEDLINE (Ovid SP)
- EMBASE (Ovid SP)
- PsycINFO (Ovid SP)
- ERIC
- Sociological Abstracts
- SCOPUS
- Proquest Dissertations and Theses database
- CINAHL (EBSCOhost)
- SIGLE (System for Information on Grey Literature in Europe)
- Campbell Collaboration's Social, Psychological, Educational and Criminological Trials Register
- AgeLine
- Social Care Online (Social Care Institute for Excellence)

- WHO International Clinical Trial Registry
- ClinicalTrials.gov

The MEDLINE (OvidSP) strategy is shown in [Appendix 1](#). This will be modified as necessary for use with other databases. No date restrictions will be applied to the searches.

### Searching other resources

We will contact key experts in the field and first authors of included studies for advice as to other relevant published, unpublished and ongoing studies (e.g. conference papers, unpublished dissertations, working papers or government reports) that should be included. We will also search reference lists of included studies and relevant reviews to identify further relevant studies.

### Data collection and analysis

#### Selection of studies

One review author (AC) will read the titles and abstracts of the identified references and eliminate any studies that are deemed to be immediately irrelevant. Two review authors (AC and MF) will discuss any abstracts about which the first author is uncertain. We will then obtain the full text of the remaining studies and, based on the [Criteria for considering studies for this review](#), two review authors (AC and MF) will independently rank these as 'includes' or 'excludes'. In the event of a disagreement, we will seek consensus through discussion and involving a third review author (SMcG) if necessary. We will contact study authors for further information if the eligibility of the study for inclusion is unclear. We will also provide citation details and any available information about ongoing studies, and collate and report details of duplicate publications, so that each study (rather than each report) is the unit of interest in the review. We will report the screening and selection process in an adapted PRISMA flow chart ([Liberati 2009](#)).

#### Data extraction and management

Two review authors (AC and MF) will independently extract data from the original reports using a data extraction form adapted from the Data Extraction Template provided by the Cochrane Consumers and Communication Review Group. We will pilot the adapted form on a small sample of studies (n = 5) before finalising the design. Any disagreement between the review authors in relation to data extraction will be resolved by discussion and consensus, and following discussion with a third review author if necessary.

Data to be extracted will include the following items:

- General: author, year of publication, title, journal, country and language of publication; funding source and declaration of interest

- Trial: study design (RCT, Cluster-RCT, quasi-RCT)
- Participant: diagnosis/health status, age, gender, ethnicity, living situation (e.g., alone, with other); sample size; method of recruitment (post-hospital discharge or from the community); inclusion and exclusion criteria
  - Intervention: components of intervention (e.g., exercise, task redesign; assistive technology; education); assessment and care planning; involvement of older people in goal setting; care planning process; composition of 're-ablement' team and training; health professionals involved; other services used; length of intervention; frequency of contact; length of follow-up; implementation fidelity
  - Control: assessment and care planning; composition of team and training; health professionals involved; other services used; length of intervention; frequency of contact
    - Methodological quality elements for [Assessment of risk of bias in included studies](#)
    - Outcomes: functional status (tools used), adverse event; where living; level of service need; health service utilisation including attendance at emergency department, (re-)hospitalisation, family doctor visits; nursing home placement; satisfaction; timing of outcome assessment(s).

All extracted data will be entered into RevMan by one review author (AC) and will be checked for accuracy against the data extraction sheets by a second review author (MF) working independently.

#### Assessment of risk of bias in included studies

We will assess and report on the methodological risk of bias of included studies in accordance with the *Cochrane Handbook* ([Higgins 2011](#)) and the guidelines of the Cochrane Consumers and Communication Review Group ([Ryan 2011](#)). These recommend the explicit reporting of the following elements for RCTs: random sequence generation; allocation sequence concealment; blinding (participants and personnel); blinding (outcome assessment); completeness of outcome data (including data on attrition); selective outcome reporting; and other sources of bias (e.g., baseline comparability; reporting biases (see [Assessment of reporting biases](#))). For cluster-RCTs we will also assess and report the risk of bias associated with selective recruitment of cluster participants and potential contamination between intervention and control group. In addition, we will assess and report quasi-RCTs as being at a high risk of bias on the random sequence generation item of the 'Risk of bias' tool.

We will judge each item and outcome separately as being at high, low or unclear risk of bias as set out in the criteria provided by [Higgins 2011](#) and we will provide a quote from the study report and a justification for our judgement for each item in the 'Risk of bias' table. In all cases, two authors (AC and MF) will independently assess the risk of bias of included studies, with any disagreements resolved by discussion and consensus. We will contact study

authors for additional information about the included studies, or for clarification of the study methods as required. We will incorporate the results of the 'Risk of bias' assessment into the review through standard tables, and systematic narrative description and commentary about each of the elements. All of this information will be used to inform the overall assessment of the risk of bias of included studies and a judgement about the internal validity of the review results.

With regard to the cost-effectiveness analysis, the 'Drummond checklist' will be used, in conjunction with the NHS EED structured abstract, where available, to critically appraise the methodological quality of included health economic studies (Shemilt 2011).

## Measures of treatment effect

### Dichotomous data

For dichotomous variables (e.g., living at home versus other location), we will calculate relative risks (RR) and their 95% confidence intervals (CI) comparing the intervention to the control group for each included study.

### Continuous data

Continuous data (e.g., quality of life scores) will be analysed if means and standard deviations are available, or if there is some other way to calculate effect size (e.g. from t-tests, F-tests, or exact P values). If reports have insufficient data, additional information will be requested from the authors.

Where scales measure the same clinical outcomes (e.g. functional status) in different ways, standardised mean differences (SMD) or Cohen's d will be estimated using RevMan's formula for SMD; this is based on Hedges' g which includes an adjustment for small sample bias. A mean difference will be used where studies employ the same measures. Confidence intervals of 95% will be used for individual study data and pooled estimates throughout. Studies that provide both dichotomous and continuous measures of the same construct will be analysed separately.

### Time-to-event data

For time-to-event (e.g., transfer to nursing home) data, we will extract the log of the hazard ratio (log(HR)) and its standard error from trial reports. If these are not reported, we will attempt to estimate the log(HR) and its standard error using published methods (Parmar 1998; Tierney 2007). HRs will be pooled using the generic inverse-variance method of RevMan 5.2 (Deeks 2011).

## Economic data

The characteristics of any health economic studies to be included in the review will be tabulated by subgroups (i.e. full economic evaluations, partial economic evaluations, and analyses reporting more limited information). These studies will be assessed for risk of bias using the Drummond checklist before a decision is made to pool any studies, particularly in relation to whether the metric in question has equivalent meaning across studies (Shemilt 2011). In circumstances where there is evidence of little variation in resource or cost use between studies, it may be regarded as legitimate to present a pooled estimate. Otherwise we will clearly present the distribution of costs (Shemilt 2011).

If a decision is made to conduct meta-analyses of resource use or cost data, this will be supported, firstly, by a thorough critical appraisal of the methods used to derive such estimates within the corresponding health economics studies and, secondly, by the use of 95% confidence intervals and statistical methods to assess between-study heterogeneity (e.g.  $I^2$  statistic,  $\text{Chi}^2$  test, random-effects models). Cost estimates collected from multiple studies will be adjusted to a common currency and price year before these data are pooled. Careful consideration will be given to the jurisdiction, analytic perspective and time horizon for both costs and effects. If meta-analyses of resource use or cost data are conducted, a narrative summary will be included in the Results section to comment on the direction and magnitude of results and their precision. Similarly, if two or more health economics studies are included in a review, but a decision is taken not to pool (in a meta-analysis) resource use and/or cost data, this will be stated in the Methods section (Shemilt 2011). If we are not able to pool data we will include a narrative summary of included studies including the design and analytical viewpoints adopted, the primary outcome measure used for the evaluation, resource use and unit cost data, and the generalisability of the conclusions drawn for other jurisdictions (Drummond 1996).

## Unit of analysis issues

Trials may include results from more than one time-point (e.g. end of intervention at 6 to 12 weeks, and follow-up at 6 months, and 1 year). In such instances, separate analyses will be conducted, based on the different time frames and relevant outcomes. Any relevant cluster-RCTs that are identified will be analysed using the methods described in the *Cochrane Handbook* (Higgins 2011). If cluster-RCTs are included we will check for unit-of analysis errors. If errors are found and sufficient information is available, we will re-analyses the data using the appropriate unit of analysis by taking account of the intracluster co-efficient (ICC). We will contact study authors of included studies to obtain ICC estimates if these are not clearly available from the trial reports, or impute them using estimates from external sources (i.e., from a study of a similar population). If ICCs from other sources are used, sensitivity analyses to investigate the effect of variation in the ICC will be



conducted and reported. If it is not possible to obtain sufficient information to reanalyse the data we will report the effect estimate and annotate 'unit-of-analysis error'.

### Dealing with missing data

If data are missing from the relevant comparisons, we will contact the study authors to obtain the information. Missing data and drop outs/attrition will be assessed for each study and reported in a 'Risk of bias' table. Numbers, reasons and characteristics of drop outs will be assessed and reported. Any meta-analysis will use data from all originally randomised participants where possible. If missing data are not available, a sensitivity analysis will be conducted by excluding studies with 20% or more of data missing for one of the primary outcomes, to assess potential bias in the analysis. The extent to which the results might be biased by missing data will also be discussed.

### Assessment of heterogeneity

Where studies are considered sufficiently similar (for example based on considerations of population, intervention duration and intensity) to allow pooling of data using meta-analysis, we will assess the degree of heterogeneity by the visual inspection of forest plots and by examining the Chi<sup>2</sup> test for heterogeneity. Heterogeneity will be quantified by using the I<sup>2</sup> statistic (Higgins 2011). An I<sup>2</sup> value of 50% or more will be considered to represent substantial heterogeneity, but this value will be interpreted in light of the size and direction of effects and the strength of the evidence of heterogeneity based on the P value from Chi<sup>2</sup> test (Higgins 2011). If there is evidence of heterogeneity, authors will discuss possible reasons and conduct subgroup analyses accordingly; the issue of sample size and power in each study will be considered in the interpretation and reporting of the results.

Where we detect substantial clinical, methodological or statistical heterogeneity across included studies we will not report pooled results from meta-analysis but will instead use a narrative approach to data synthesis. In this event we will attempt to explore possible clinical or methodological reasons for this variation by grouping studies that are similar in terms of features of the intervention (e.g., duration) and methodological features (e.g., study design) to explore differences in intervention effects.

### Assessment of reporting biases

We will assess reporting bias qualitatively based on the characteristics of the included studies (e.g., if only small studies that indicate positive findings are identified for inclusion), and if information that we obtain from contacting experts and authors or studies suggests that there are relevant unpublished studies. If we identify sufficient studies (at least 10) for inclusion we will construct funnel plots to investigate any relationship between effect size and standard error. Such a relationship could be due to publication or

related biases, or due to systematic differences between small and large studies. Where such a relationship is identified, the methodological diversity of the studies will be further examined as a possible explanation (Egger 1997). Findings will be incorporated into the 'Risk of bias' tables under 'Other sources of bias'.

### Data synthesis

We will decide whether to meta-analyse data based on whether the included trials are sufficiently similar enough in terms of participants, interventions, comparisons and outcome measures to ensure meaningful conclusions from a statistically pooled result. We will only combine cluster RCTs with individual RCTs, if any unit-of-analysis errors have been addressed. Due to the anticipated variability in the intervention and participants of included studies we will use a random-effects model for meta-analysis. Data synthesis will be conducted using Review Manager (RevMan) 5.2, the latest version of the Cochrane Collaboration's meta-analysis software.

If we are unable to pool the data statistically using meta-analysis we will present a narrative summary of the findings. Depending on the number of studies these will be organised into categories or clusters (e.g., study design) that best explore the heterogeneity of the studies. We will also explore the main comparisons of the review: intervention versus usual care; intervention versus no intervention/wait list. The findings will be presented in a 'Summary of findings' table format so that it will be easier to identify any patterns in the results.

### Subgroup analysis and investigation of heterogeneity

Further investigations of the causes of heterogeneity may be conducted using subgroup analysis; this will be based on the following subgroup parameters (where available) that have emerged from the literature:

- Context of recruitment to intervention (i.e. after hospital discharge or from within the community): we anticipate that the participants who have been recently discharged from hospital may have a higher level of need and/or be at greater risk of re-admission than those recruited from the community, and thus some differences in outcomes may emerge (Francis 2011).
- Mean age of participants: there is some indication that younger participants (i.e., under 75 years) may gain greater benefit from 're-ablement'. We therefore will examine two groups: (1) people aged 65 to 75, and (2) people aged over 75, to explore this effect (Glendinning 2010).
- Living circumstances (i.e. alone or with others): isolated older people may experience the service differently from those with a higher level of social support (Francis 2011).
- Duration of intervention: defined as 'standard' = 6 weeks; 'long' = 7 to 12 weeks; some trials may offer an extended period of 're-ablement' to meet individual needs (Jones 2009).

## Sensitivity analysis

We will perform sensitivity analyses to evaluate the robustness of the pooled effect sizes across various components of methodological quality in order to see how robust the various effect estimates are. We will analyse the effects of excluding trials that are judged to be at high risk of bias across one or more of the domains of sequence generation, allocation concealment, attrition (rates larger than 20%) and outcome reporting (greater than 20% of data missing) for the meta-analysis of the primary outcomes. If the exclusion of trials at high risk of bias does not substantially alter the direction of effect or the precision of the effect estimates, then we will include data from these trials in the analysis. We will also undertake a sensitivity analysis to assess the effects of including data from trials where we used imputed values (e.g., ICC values from external sources for cluster-RCT trials).

## 'Summary of findings' table

Outcomes to be reported in a 'Summary of findings' table are listed at [Types of outcome measures](#). We will provide a source and rationale for each assumed risk cited in the table(s), and two

authors will independently assess the quality of the evidence as implemented and described in the GRADEprofiler (GRADEpro) software ([Schünemann 2011](#)). If meta-analysis is not possible, we will present results in a narrative 'Summary of findings' table format such as that used by [Chan 2011](#).

## Consumer participation

'Re-ablement' reflects a partnership between the older person and the service providers, and thus consumer participation in this review is considered important. We will therefore invite consumer referees to comment on the protocol and on the completed review through Cochrane Consumers and Communication Review Group editorial processes.

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\* Indicates the major publication for the study

## APPENDICES

### Appendix I. MEDLINE Search Strategy

OvidSP

1. home care services/
2. (home adj5 (care or visit\*)).tw.
3. homecare.tw.
4. house calls/
5. domiciliary care.tw.
6. own home?.tw.
7. (community dwelling or community setting or living in the community or home based).tw.
8. community health nursing/
9. or/1-8
10. exp rehabilitation/
11. (rehab\* or (activit\* adj2 daily living)).tw.
12. (re-abl\* or reabl\* or enablement or empower\* or restor\* or re-learn\* or relearn\*).tw.
13. "recovery of function"/
14. ((recover\* or optim\* or maintain\* or increas\* or improv\* or independen\* or ability or outcome\*) adj3 function\*).tw.
15. ((enabl\* or recover\* or maintain\* or develop\* or living) adj3 independen\*).tw.

16. self care/
17. (self adj (care or manag\*)).tw.
18. or/10-17
19. 9 and 18
20. randomized controlled trial.pt.
21. controlled clinical trial.pt.
22. randomized.ab.
23. placebo.ab.
24. drug therapy.fs.
25. randomly.ab.
26. trial.ab.
27. groups.ab.
28. or/20-27
29. exp animals/ not humans.sh.
30. 28 not 29
31. 19 and 30

## **CONTRIBUTIONS OF AUTHORS**

Andy Cochrane (AC) wrote the text of the protocol with input and amendments advised/provided by all members of the review team (Sinéad McGilloy (SMcG), Mairead Furlong (MF), Michael Donnolly (MD), David Molloy (DWM), and Michael Stevenson (MS). AC developed the search strategy for this protocol in conjunction with John Kis-Rigo, Trials Search Coordinator of the Cochrane Consumers and Communication Review Group.

## **DECLARATIONS OF INTEREST**

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Other authors (DWM, MF and MS): none known.

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