Home Visits for Prevention of Impairment and Death in Older Adults: A Systematic Review

Sean Grant, Amanda Parsons, Jennifer Burton, Paul Montgomery, Kristen Underhill, Evan Mayo Wilson
Littell (Campbell Social Welfare Group), Katherine Hicks (Cochrane Dementia and Cognitive Improvement Group) and anonymous peer reviewers for their helpful comments.

**Potential conflicts of interest**

The authors have no vested interest in the outcomes of this review, nor any incentive to represent findings in a biased manner.

**Corresponding author**

Evan Mayo-Wilson
Centre for Outcomes Research and Effectiveness
Department of Clinical, Educational & Health Psychology
University College London
1-19 Torrington Place
London WC1E 7HB
Email: e.mayo-wilson@ucl.ac.uk
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Executive summary

BACKGROUND

Home visits by health and social care professionals aim to prevent cognitive and functional impairment, thus reducing institutionalisation and prolonging life. Visitors may provide health information, investigate untreated or sub-optimally treated problems, encourage compliance with medical care, and provide referrals to services. Previous reviews have reached varying conclusions about their effectiveness. This review sought to assess the effectiveness of preventive home visits for older adults (65+ years) and to identify factors that may moderate effects.

OBJECTIVES

To systematically review evidence on the effectiveness of preventive home visits for older adults, and to identify factors that may moderate effects.

SEARCH STRATEGY

We searched the following electronic databases through December 2012 without language restrictions: British Nursing Index and Archive, C2-SPECTR, CINAHL, CENTRAL, EMBASE, IBSS, Medline, Nursing Full Text Plus, PsycINFO, and Sociological Abstracts. Reference lists from previous reviews and from included studies were also examined.

SELECTION CRITERIA

We included randomised controlled trials enrolling persons without dementia aged over 65 years and living at home. Interventions included visits at home by a health or social care professional that were not directly related to recent hospital discharge. Interventions were compared to usual care, wait-list, or attention controls.

DATA COLLECTION AND ANALYSIS

Two authors independently extracted data from included studies in pre-specified domains, assessed risk of bias using the Cochrane Risk of Bias tool, and rated the
quality of evidence using GRADE criteria. Outcomes were pooled using random effects models. We analyzed effects on mortality, institutionalization, hospitalization, falls, injuries, physical functioning, cognitive functioning, quality of life, and psychiatric illness.

RESULTS

Sixty-four studies with 28642 participants were included. There was high quality evidence that home visits did not reduce absolute mortality at longest follow-up (Risk ratio=0.93 [0.87 to 0.99]; Risk difference=0.00 [-0.01 to 0.00]). There was moderate quality evidence of no clinically or statistically significant overall effect on the number of people who were institutionalised (Risk ratio=1.02 [0.88, 1.18]) or hospitalised (Risk ratio=0.96 [0.91, 1.01]) during the studies. There was high quality evidence of no statistically significant effect on the number of people who fell (Odds ratio=0.86 [0.73, 1.01]). There was low quality evidence of statistically significant effects for quality of life (Standardised mean difference=-0.06 [-0.11, -0.01]) and very low quality evidence of statistically significant effects for functioning (SMD=-0.10 [-0.17, -0.03]), but these overall effects may not be clinically significant. However, there was heterogeneity in settings, types of visitor, focus of visits, and control groups. We cannot exclude the possibility that some programmes were associated with meaningful benefits.

AUTHORS’ CONCLUSIONS

We were unable to identify reliable effects of home visits overall or in any subset of the studies in this review. It is possible that some home visiting programmes have beneficial effects for community-dwelling older adults, but poor reporting of how interventions and comparisons were implemented prevents more robust conclusions. While it is difficult to draw firm conclusions given these limitations, estimates of treatment effects are statistically precise, and further small studies of multi-component interventions compared with usual care would be unlikely to change the conclusions of this review. If researchers continue to evaluate these types of interventions, they should begin with a clear theory of change, clearly describe the programme theory of change and implementation, and report all outcomes measured.
# Background

## 1.1 DESCRIPTION OF THE CONDITION

About 13% of Americans and 15-20% of Europeans are over 65 years old (United Nations, Department of Economic and Social Affairs, Population Division, 2011). The vast majority of older adults wish to remain living autonomously in their own homes for as long as possible (Department of Health, 2001; Rostgaard & Friedberg, 1998). However, cognitive and functional impairments increase with age and reduce quality of life for older adults, their families and their carers (Jones & Peters, 1992; McKinlay, Crawford, & Tennstedt, 1995). Subsequent functional decline can lead to loss of independence (Guralnik & Ferrucci, 2002), increased risk for falls and other injuries (Tinetti, Speechley, & Ginter, 1988), hospitalisation and nursing home admission (Fried & Bush, 1988), and possibly early death (Beswick et al., 2008).

The development and promotion of interventions to maintain quality of life of older adults is a public health priority (Cruz-Jentoft et al., 2008; Gustafsson, Edberg, Johansson, & Dahlin-Ivanoff, 2009; World Health Organization, 2003) and a central challenge to current medical and social care systems (Elkan & Kendrick, 2004; Johri, Beland, & Bergman, 2003). Aging populations, advances in technology, and recent global economic crises have led to an international imperative for health services to reform the organisation of care in order to best meet the needs of older people while making efficient use of scarce resources (Conroy, Stevens, Parker, & Gladman, 2011; Markle-Reid et al., 2006). For example, the impact of falls-related injuries on quality of life in older adults and on health care systems is substantial (Moyer, 2012), making the prevention of falls an important issue in health reform (RAND, 2004). Consequently, guidelines to prevent falls in the elderly have been published in the UK (Feder, Cryer, Donovan, & Carter, 2000), US (American Geriatrics Society, British Geriatrics Society, & American Academy of Orthopaedic Surgeons Panel on Falls Prevention, 2001) and Canada (Scott, Dukeshire, Gallagher, & Scanlan, 2001), amongst other countries. Team-based approaches incorporating geriatric screening and assessment have also been incorporated into health care systems in the UK (Department of Health, 2001) and across various countries in the EU (Leichsenring, 2004).
1.2 DESCRIPTION OF THE INTERVENTION

Preventive interventions aimed at maintaining the health and autonomy of community-dwelling older adults have received much attention in the past two decades (van Haastregt, Diederiks, van Rossum, de Witte, & Crebolder, 2000). These preventive interventions usually are based on health promotion: i.e., the process of enabling people to increase control over their health (World Health Organization, 2009). Health promotion for community-dwelling older adults involves strategies to reduce risk factors for morbidity and mortality that relate to various physical, functional, psychological, environmental, and social issues (Phelan, Anderson, LaCroix, & Larson, 2004; Stuck et al., 1999). The variety of risk factors has led to preventive interventions that are themselves diverse in nature (RAND, 2004).

Preventive home visiting is an increasingly popular health-promotion intervention for community-dwelling older adults. As professional health visitors are in a valuable position in many countries to promote the well-being of older people (Elkan et al., 2001), interest has grown internationally in the use of proactive home-based programmes as supplements to usual care (Byles, 2000; Elkan et al., 2001). Preventive home visits have even been incorporated into national policy in several countries (Ministry of Health and Welfare, 2000; Vass, Avlund, Hendriksen, Holmberg, & Nielsen, 2006) such as Denmark and Australia (Huss, Stuck, Rubenstein, Egger, & Clough-Gorr, 2008).

This complex intervention has been described by different names and adapted in various ways to specific populations and settings (Beswick et al., 2008; Stuck, Siu, Wieland, Adams, & Rubenstein, 1993). Home visits have involved a range of levels of care; specific programmes may include primary prevention (e.g. provision of health information, risk reduction, and safety promotion), secondary prevention (e.g. detection of untreated/sub-optimally treated problems), or tertiary prevention (e.g. encouraging medication compliance) (Veerbrugge & Jette, 1994). Programs can also have various foci, including exercise, education, medication review, environmental modification, or a combination of the above (Gillespie et al., 2009; Michael et al., 2010). Some programmes include several home visits, while others include only one visit that may lead to further specialised care. Visits are conducted by different health professionals, including nurses, social workers, and physiotherapists. Nonetheless, adaptations of this complex intervention all involve preventive home visits for independently-living older adults based on assessment of medical and social need (Beswick et al., 2008).

1.3 HOW THE INTERVENTION MIGHT WORK

Home visits involve health professionals visiting older adults in their own homes to proactively address health-related risk factors, promote positive health behaviours,
and increase clients’ autonomy (Alessi et al., 1997; Byles, 2000; Elkan et al., 2001). During home visits, health professionals may conduct health assessments, provide professional support, and refer older adults to specialist care. By reducing risk of functional decline, these processes are ultimately intended to improve older adults’ health-related quality of life, increase the likelihood of continued independent living, and delay mortality (Markle-Reid et al., 2006; Pahor & Applegate, 1997; Stuck, Egger, Hammer, Minder, & Beck, 2002; van Broese & Thomese, 1996). Due to the complexity of both the health problem and the intervention, effects are believed to be moderated by age, risk factors (e.g. social support), and health care setting (Clark, 2001; Huss et al., 2008).

Some preventive home visit programmes focus on a single important risk factor by, for example, targeting falls reduction through balance and strength training, identifying medications that cause dizziness, removing hazards in the home, or promoting exercise (RAND, 2004). Meeting in the home allows first-hand assessment of the user’s environment, allowing professionals to conduct assessments to prompt further interventions (e.g., by general practitioners) or intervene directly during the visit (e.g., by providing information, physical therapy, medication, or other resources).

Other “multidimensional” programs have several mechanisms of action that address the high prevalence of co-existing and often-interrelated risk factors found amongst community-dwelling older adults (Gill & Sharpe, 1999; Huss et al., 2008). They often begin with a multidimensional geriatric assessment (MGA) that evaluates the participant’s risk factors across several domains, such as medical, functional, psychosocial, and environmental problems and resources (Huss et al., 2008; Stuck et al., 2002). This initial assessment then prompts individualised follow-up visits, which consist of various strategies that target all present risk factors, such as balance impairment, muscle weakness, polypharmacy, environmental hazards, psychosocial deficits, and others (Ganz, Bao, Shekelle, & Rubenstein, 2007; RAND, 2004; van Haastregt et al., 2000). Because information from the MGA and from follow-up visits is usually shared with other health and social care professionals, collaboration by a team of multi-disciplinary professionals may be required to manage the complex care involved in these programmes (Bouman, van Rossum, Nelemans, Kempen, & Knipschild, 2008).

1.4 WHY IT IS IMPORTANT TO DO THIS REVIEW

Evidence on the effectiveness of preventive home visits has important policy and practice implications because these interventions cost hundreds to several thousand dollars per participant, depending on programme focus and intensity (Lenaghan, 2007; Shapiro, 2002). The value of preventive home visits is unclear, however. Over the last two decades, a significant number of studies have produced inconsistent and conflicting results on the effects of home visiting programmes for older people living independently in the community (Bouman et al., 2008; Elkan et al., 2001; Elkan &
Kendrick, 2004; Markle-Reid et al., 2006; Stuck et al., 2002; van Haastregt et al., 2000). While several reviews and meta-analyses have suggested that home visits by health or social care professionals may have direct and indirect (e.g., economic) benefits (Elkan et al., 2001; Huss et al., 2008; Stuck et al., 2002), others conclude that they should be discontinued unless the effectiveness of visits can be improved (van Haastregt et al., 2000). Uncertainty exists about the active ingredients of the intervention, which populations might benefit most, and the influence of particular health care systems on outcomes (Markle-Reid et al., 2006). For example, one recent review suggests that there are no overall benefits, but that specific types of home visits might benefit younger participants (Huss et al., 2008).

Previous reviews have also differed in terms of their inclusion criteria, outcomes of primary interest, and method of analysis because these reviews have often reflected the orientation and goals of the disciplinary background of the research team (Clark, 2001). As a result, preventive home visit programmes have yet to be comprehensively addressed in a single systematic review that examines their various foci and outcomes of interest. The need for such a review is demonstrated, for example, by the uncertainty of the United States Preventive Services Task Force (USPSTF) in recommending in-depth multifactorial risk assessment and comprehensive management of identified risks for community-dwelling older adults, due to the current state of the research evidence (Moyer, 2012). Insight into whether home visiting programmes are effective is essential for making more informed judgments in policy, practice, and future research in this area (Bouman et al., 2008).
2 Methods

2.1 OBJECTIVES

This review assesses the effectiveness of preventive home visits for community-dwelling older adults (65+ years) without dementia and investigates factors that may moderate effects through pre-specified subgroup analyses.

2.2 CRITERIA FOR CONSIDERING STUDIES FOR THIS REVIEW

2.2.1 Types of studies

Randomised controlled trials (RCTs) and clustered RCTs were eligible. We chose to exclude non-randomised studies because the evidence from RCTs is generally regarded as superior to that from non-randomised studies and because the number of RCTs which have been carried out in this area is considerable.

2.2.2 Types of participants

Eligible participants included persons aged 65 years and older who were living at home (alone or with a partner) and not in residential care homes or independent living facilities. We excluded studies in which more than 50% of participants had dementia. Studies enrolling participants under 65 were eligible when the majority were 65 and older.

2.2.3 Types of interventions

Visits at home by a health or social care professional compared to usual care, wait-list, or attention controls were considered. Eligible interventions included: 'routine' health visiting practice (e.g., monitoring of compliance with medication or other interventions, social support, health promotion, co-ordination of community services, practical advice, referral to other services, and counselling); visits that included multidimensional geriatric assessment (MGA) and resulted in specific recommendations to reduce, treat, or prevent problems; visits that focused on fall prevention; and visits that included exercise components.

We excluded studies that evaluated follow-up home visits that were directly related to recent hospital discharge (e.g., to assess or attend a recently treated condition).
Studies with control conditions that explicitly involved home visits were also excluded.

2.2.4 Types of outcome measures

Primary
- Mortality

Secondary
- Institutionalisation (people admitted; days in institution)
- Hospitalisation (people admitted; admissions, days in hospital)
- Falls (people who fell; number of falls)
- Injuries (people injured; number of injuries)
- Physical functioning (i.e. Activities of Daily Living or Instrumental Activities of Daily Living)
- Cognitive functioning (e.g. Mini-Mental State Examination)
- Quality of life (e.g. Medical Outcomes Study Short Form Health Survey-36)
- Psychiatric illness (e.g. anxiety or depression)

Outcomes were grouped by length of follow-up (0 to 11 months, 12 to 23 months, 24 to 35 months, 36 months or more). When an outcome was measured twice during an interval, the longest outcome in that interval was used for meta-analyses (e.g., when a study reported outcomes at 6 and 8 months, we extracted data at 8 months).

2.3 SEARCH METHODS FOR IDENTIFICATION OF STUDIES

The following databases were searched in December 2012 without language restriction for published and unpublished studies: British Nursing Index and Archive, C2-SPECTR, CINAHL, CENTRAL, EMBASE, IBSS, Medline, Nursing Full Text Plus, PsycINFO, and Sociological Abstracts (Appendix 1).

Reference lists from previous reviews and from included studies were examined.

The reviewers contacted the authors of included studies to request details of ongoing and unpublished studies. When the corresponding author did not respond, other authors were contacted wherever possible.

2.4 DATA COLLECTION AND ANALYSIS

2.4.1 Selection of studies

Two review authors independently reviewed all titles and abstracts. Relevant articles were collected and independently screened to determine which studies met the inclusion criteria. Authors were contacted if further information was required. We did not calculate inter-rater agreement because any paper that one review author considered relevant was checked for inclusion, and the final list of included studies was agreed
following requests for additional information and through discussion among the reviewers.

2.4.2 **Data collection and management**

Data were extracted in duplicate (EMW, KU, JB, SG, AP), and differences were reconciled through discussion and consultation with a third author. For each study, we extracted contextual information, recruitment strategy, and inclusion criteria (Appendix 2). For all study arms, we extracted demographic data, content and delivery of the intervention, frequency and duration, and outcome measures in the categories listed above. Outcome data were extracted into Excel spreadsheets, and agreed data were entered into Comprehensive Meta-Analysis (CMA) Version 2 software (Borenstein, Hedges, Higgins, & Rothstein, 2005). When studies included more than one eligible intervention group and a single comparison, we combined the intervention groups for analysis.

2.4.3 **Quality of the evidence**

Two reviewers also coded each included study using the Cochrane Collaboration Risk of Bias Tool (Higgins & Green, 2011). We judged whether each study was at low, high, or unclear risk of bias relating to sequence generation; allocation concealment; blinding of outcome assessors; incomplete outcome data; selective outcome reporting; and other sources of bias. Given the nature of the intervention, risk of bias was judged as ‘high’ for blinding of personnel and blinding of participants for each of the included studies. Disagreements were resolved through discussion and by seeking further information.

Overall confidence in the results was assessed using the Grades of Recommendation, Assessment, Development, and Evaluation (GRADE) approach for evaluating the quality of evidence for outcomes in systematic reviews (Guyatt, Oxman, Schünemann, Tugwell, & Knotterus, 2010). This approach considers the quality of a body of evidence within a systematic review to be the degree of confidence that an effect estimate is close to the actual specific quantity of interest (e.g., reductions in mortality as a result of a home visits programme). The GRADE approach involves the assessment of the quality of the evidence for each individual outcome within a review, and results in a “grade” for each outcome (high, moderate, low, or very low) according to the risk of bias, directness of evidence, heterogeneity, precision of effect estimates, and risk of publication bias for the body of evidence for that outcome (Higgins & Green, 2011).

2.4.4 **Measures of treatment effect**

Studies often report outcomes using multiple definitions and outcome measures. We gave preference to data that involved the least manipulation by authors or inference by review authors; that is, we extracted raw values (e.g., means and standard deviations) rather than calculated effect sizes (e.g., Cohen’s d). If outcomes were reported as final values and as changes from baseline, we extracted the final values.
When studies reported more than one measure of a particular outcome (e.g., psychiatric illness measured using two scales), we averaged the results in CMA (Borenstein, Hedges, Higgins, & Rothstein, 2005) before entering data in RevMan (Cochrane Collaboration, 2011).

2.4.5 **Unit of analysis issues**

For each cluster-randomised trial, we determined if the analyses controlled for clustering (e.g. reporting robust standard errors or hierarchical linear models). When data were not analysed using proper controls, we would have attempted to obtain an intra-cluster correlation coefficient (ICC), but there was insufficient information to control for clustering in any study. A few studies randomised households, and it was unclear in other studies if individuals or households had been assigned. In these cases, cluster sizes were close to 1 and were analysed without correction.

2.4.6 **Dealing with missing data**

The corresponding author of each included study was contacted to supply any unreported data (e.g. outcome data, details of dropouts, details of interventions received by the control group). When the corresponding author did not respond, other authors were contacted if possible. For studies reporting outcomes only for participants completing the study, we asked authors to provide additional information to permit intention-to-treat analyses.

2.4.7 **Assessment of heterogeneity**

Differences among included studies are discussed in terms of their participants, interventions, outcomes, and methods. For each meta-analysis, we also visually inspected forest plots to see if the confidence intervals of individual studies had poor overlap, conducted a Chi² test, and calculated the I² statistic. We considered meta-analyses to have heterogeneity when the \( p \) value for Chi² was less than 0.10 and I² was greater than 25%.

2.4.8 **Assessment of reporting bias**

To assess the possibility of small study bias, we drew funnel plots for each overall outcome and looked for asymmetry.

2.4.9 **Data synthesis**

We used Review Manager (RevMan) Version 5.1 (Cochrane Collaboration, 2011) to conduct all meta-analyses. Where possible, dichotomous data were entered directly into RevMan, and relative risks or rate ratios and 95% confidence intervals (CIs) were calculated for dichotomous outcomes and combined using Mantel-Haenszel methods. We report separate analyses for number of injuries and number of people injured, as well as for other dichotomous outcomes, such as falls and hospitalisation. If some studies reported events and others reported only calculated effect sizes, we
calculated the average effect for each study and entered the data using the Generic Inverse Variance option, in which cases relative risks or rate ratios were combined using inverse variance methods. Risk ratios describe events that can happen only once (e.g., death) and can be calculated using the number of people randomised to each intervention. Rate ratios describe events that can happen more than once to each person (e.g., number of falls) and can be calculated using time at risk (e.g., person-years); when studies reported events that could occur multiple times without reporting time at risk, we estimated this by assuming (i) all survivors were included for the full duration of the study and (ii) dropouts were at risk for 50% of the year in which they died or left the study. When risk ratios or rate ratios could not be calculated for dichotomous data, we calculated odds ratios (ORs) rather than convert ORs to RRs by assuming the baseline risk. Standardised mean differences (SMDs) and 95% CIs were calculated for continuous measures using Hedges $g$ with small sample correction and combined using inverse variance methods. Random-effects models were used because studies included different interventions and populations. We used the method of moments estimator within either RevMan or CMA to calculate the variance component for each group of studies.

2.4.10 **Subgroup analysis and investigation of heterogeneity**

We conducted the following subgroup analyses when 10 or more studies were included in an analysis (Higgins & Green, 2011, section 9.6.5.1; Higgins & Thompson, 2004):

- Professional group (nurses, other, combinations of providers);
- Participant age (<70, 71-75, 76-80, 81-85, >85);
- Intervention components
  - falls only (interventions that exclusively targeted falls prevention, e.g., exercise to improve balance and strength)
  - MGA (a systematic evaluation of at least 3 of these domains—medical, functional, psychosocial, or environmental)
  - both falls prevention and MGA
  - neither falls prevention nor MGA
- Number of visits (1; 2 to 4; 5 or more).
3 Results

3.1 DESCRIPTION OF STUDIES

3.1.1 Results of the search

We identified 18784 records, removed 3930 duplicates and examined 14854 titles and abstracts (Figure 1). Full texts were obtained for 179 records identified as potentially relevant by one of two independent reviewers (EMW, KU, SG, AP, JB). Thirty-three papers were secondary reports of a study reported in another paper; thus, 146 studies were assessed for eligibility.

Figure 1: PRISMA Flow Chart
3.1.2 Included studies

3.1.2.1 Design
Sixty-four studies reported in 89 papers were included (Table 1). Post-hoc, we included two studies in which participants were assigned using quasi-random methods that approximated the characteristics of randomisation, as described below in section 3.2.1.1 (Gunner-Svensson, 1984; Sahlen, 2006).

Overall, studies assigned 28642 participants, ranging from 59 (Crawford-Shearer, 2010; Liu-Ambrose, 2008) to 3743 (Gunner-Svenson, 1984) with a median sample size of 299 per study. One study (Vetter, 1984) reported evaluations from two sites which are included as independent studies in our analyses. Subgroups of different ages from another study (Pathy, 1992) also appear separately in the analyses.

3.1.2.2 Settings
Studies varied in terms of both age and location, in settings which provided different psychosocial and medical services for older adults. The earliest study was reported in 1981 (Luker, 1981) and the most recent in 2012 (Gustafsson, 2012). Locations included the United States (14), Great Britain (14), Canada (11), Australia (4), New Zealand (4), Denmark (2), Italy (1), Finland (1), the Netherlands (5), Japan (3), Taiwan (2), Sweden (2), and Switzerland (1). Participants were recruited through primary care providers (24), general population registries (11), community and social service organisations (7), Accident and Emergency (A&E) departments (6), health insurance plan registers (5), advertisements (4), veterans’ health organisations (1), and various combinations of the above (3); 3 studies did not report how participants were recruited.

3.1.2.3 Participants
Studies used varying eligibility criteria; some included people at high risk of institutionalisation while others recruited from the general population. Between 0% and 33% of control subjects had died at the longest outcome interval.

Studies included participants aged 65 years (1), 70 years (10), 75 years (28), 80 years (18), and 85 years (3). In others (4), the mean age was over 70 years, but some participants could have been under 65 years. One of these studies (Balaban, 1988) included people aged 17 to 99 years; the mean age was 69 years, and 75% of participants were over 65 years.

Of 59 studies reporting sex, the majority of participants in 54 were female and the median study included 69% women. Four studies included only women (Campbell, 1999; Kingston, 2001; Luker, 1981; Wyman, 2007). One study recruited veterans and widows from the US armed services; here 97% of participants were men (Fabacher, 1994).
Table 1: Included Studies

<table>
<thead>
<tr>
<th>Study ID</th>
<th>Country</th>
<th>No.</th>
<th>Mean age (yrs)</th>
<th>Focus</th>
<th>Visitor</th>
<th>Mean no of visits</th>
<th>Length (months)</th>
</tr>
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<tbody>
<tr>
<td>Balaban 1988</td>
<td>USA</td>
<td>198</td>
<td>69</td>
<td>MGA</td>
<td>Combined</td>
<td>N/R</td>
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</tr>
<tr>
<td>Bernabei 1998</td>
<td>IT</td>
<td>200</td>
<td>81</td>
<td>MGA</td>
<td>Other</td>
<td>6</td>
<td>12</td>
</tr>
<tr>
<td>Bouman 2008</td>
<td>NL</td>
<td>330</td>
<td>76</td>
<td>MGA</td>
<td>Nurse</td>
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<td>72</td>
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<td>85</td>
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<td>Country</td>
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<td>Treatment</td>
<td>Control</td>
<td>Effects</td>
<td>Outcome</td>
<td>Events</td>
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<tr>
<td>-----------------------------</td>
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<tr>
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<td>Other</td>
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<tr>
<td>Ploeg 2010</td>
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<td>Robertson 2001a</td>
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<td>240</td>
<td>Falls</td>
<td>Nurse</td>
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<td>6</td>
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<td>Sahlen 2006</td>
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<td>Combined</td>
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<td>Sommers 2000</td>
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<td>MGA</td>
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<td>MGA</td>
<td>Combined</td>
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<td>Both</td>
<td>Nurse</td>
<td>5</td>
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<td></td>
</tr>
</tbody>
</table>
3.1.2.4 Interventions

There was heterogeneity across studies in the number, duration and focus of visits offered. Additionally, there was heterogeneity in the type and quality of follow-up care related to home visits.

The number of visits varied by participant in 8 studies; other studies provided one (11) to 30 visits on average per participant. The number of visits was not reported for 4 studies and was variable but not specified in 2 further studies.

Visitors were nurses alone (27); other professionals, including health visitors, physiotherapists, social workers, physicians, occupational therapists, case managers, and community health professionals (20); or a combination of health professionals, usually a nurse in combination with another professional (17).

Visits had different but overlapping goals (Table 2), including falls prevention (17), multi-dimensional geriatric assessment (25), both of the above (16), or an alternative focus regarding health impairment prevention (6); 21 of these studies also include an exercise component to the home visit. Overall, studies did not systematically report programme design, components that were actually delivered by staff, or take-up by participants.

Comparisons included usual care (50), attention-matched control conditions that included social visits (10), and wait-lists (3); one study did not report the comparison condition. We would have considered comparisons separately, but we could not determine reliably what comparison groups actually received across different locations, times, and service settings. Specific details per study are included in the Extended Table of Included Studies (Appendix 3).

Table 2: Study characteristics by focus of visit

<table>
<thead>
<tr>
<th>Focus</th>
<th>Studies</th>
<th>Participants</th>
<th>Mean age (yrs)</th>
<th>Sex (% female)</th>
<th>Mean no visits</th>
<th>Duration (months)</th>
<th>Exercise component (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Falls</td>
<td>17</td>
<td>4990</td>
<td>79</td>
<td>71%</td>
<td>5.1</td>
<td>6.1</td>
<td>77%</td>
</tr>
</tbody>
</table>
3.1.2.5 Outcomes

Follow-up periods ranged from 3 months (Crawford Shearer, 2010) to 60 months (Gunner Svensson, 1984).

Studies measured institutionalisation in a variety of ways, such as the number of people admitted to a nursing home (25); the total number of days in a nursing home (4); and the number of people in a nursing home at the end of the study (2). Hospitalisation data were presented in a variety of ways, including the number of people admitted to hospital at any time (14); the total number of hospitalisations (12); the number of days people spent in hospital (11); the number of people admitted to acute care (9); the number of people admitted to hospital overnight (1); and the average length of hospital stay (1). A variety of measures were employed for other outcomes, including functioning and psychiatric illness. The methods used to combine results where a study reported more than one measure of a particular outcome are summarised in sections 2.4.4 and 2.4.9.

3.1.3 Excluded studies

Seventy-six studies that did not meet our inclusion criteria were excluded for reasons listed in the Table of Excluded Studies (Appendix 4).

Four ongoing studies were identified (Cutchin, 2009; Ferrer, 2010; Fleischer, 2008; Hinrichs, 2009); these may be included in future updates of this review. We were unable to obtain any relevant published material for two studies (Jinga, 2012; Jitapunkul, 1998).

3.2 RISK OF BIAS IN INCLUDED STUDIES

3.2.1.1 Allocation (selection bias)

Most studies (41) adequately described randomisation and were judged to be at low risk of bias on this domain, but sequence generation was unclear in 20. Two quasi-random studies were included post-hoc; these were rated high risk per se (Gunner-Svensson, 1984; Sahlen, 2006) although the reviewers concluded that the methods of assignment had the desirable characteristics of randomisation. One study was judged to be at high risk of bias because participants were transferred from the intervention to the control group after randomisation (Shapiro, 2002).

Allocation concealment was also judged adequate in 33 studies at low risk of bias, but unclear in 27. The two studies included post-hoc were at high-risk per se, and two studies at high risk of bias for sequence generation were also at high risk of bias for
allocation concealment (Figure 2).

Many studies did not describe what happened to participants living in the same household (e.g., husband and wife) and may have randomised small clusters. One excluded study allocated four clinics with an average of 118 participants (Schraeder, 2001); another assigned blocks of four physicians with an average of 75 participants (Tinetti, 1994). No study reported that effects were adjusted to control for clustering; however, average cluster sizes were close to 1 in studies that explicitly assigned households.

3.2.1.2 Blinding (performance bias and detection bias)
It was impossible to blind participants or providers to treatment condition, and so all studies were judged to be at high risk of bias for provider and participant blinding. Furthermore, assessors were blind in 46 studies, which were judged to be at low risk of bias on this domain. Assessors were not blind in 12 studies, which were judged at high risk of bias, and it was unclear if assessors were blind in 6 studies. However, mortality, institutionalisation, and hospitalisation are unlikely to have been affected by biased reporting or assessment; for this reason, when using the GRADE approach, we did not downgrade the quality of evidence for these outcomes due to lack of blinding of the outcome assessors.

3.2.1.3 Incomplete Outcome Data (Attrition Bias)
Overall, we judged that missing data were insufficient to have a clinically significant impact on estimates of effects for dichotomous outcomes, including mortality, institutionalisation, and hospitalisation. In individual trials, 31 studies were judged to be at low risk of bias for incomplete outcome data, 16 studies were unclear, and 17 were at high risk of bias, including two that excluded participants from analyses if they refused visits or did not comply with the protocol (e.g., Hogan, 2001; McEwan, 1990). The most common method used to impute continuous data for dropouts was the technique of ‘last observation carried forward’, which was judged inappropriate because functioning and quality of life are likely to decline over time in these populations. The majority of studies reported low attrition.

3.2.1.4 Selective Reporting (reporting bias)
Risk of selective outcome reporting was unclear in 43 studies that did not reference a protocol. We judged 18 studies to be at high risk of bias because measured outcomes were omitted. Only 3 studies were clearly free of selective outcome reporting (i.e., outcomes were registered and reported in full).

Several studies reported outcome data only for a subgroup of the population, including one that reported most results only among participants who were at low risk of nursing home admission and concluded that home visits had no benefit and some evidence of harm for participants at elevated baseline risk (Stuck, 2000).
One study collected outcome data both by personal interviews and by post; because these reports differed significantly, the study excluded participants with postal data from many analyses (Byles, 2004).

### 3.2.1.5 Other potential sources of bias

Several studies used data from proxy interviews rather than self-reported data for some participants (Balaban, 1988; Caplan, 2004; Stuck, 2000) because these data collection strategies may yield differing results for measures like functioning, mental status, or quality of life. One study that used written instruments to collect self-reported data also reported high rates of illiteracy (Huang, 2004). One study did not collect baseline or post-intervention data (Balaban, 1988).

### 3.3 EFFECTS OF INTERVENTIONS

#### 3.3.1.1 Mortality

Fifty-five studies (86% of studies) with 24198 participants (84% of randomised participants) reported mortality data that could be combined.

![Risk of bias](chart.png)

*Figure 2: Risk of bias*

There was high quality evidence of no clinically significant difference at longest follow-up (Risk ratio=0.93 [0.87, 0.99]; $\chi^2$=54.89, df=53, $p=0.40$; $I^2=3\%$), and the absolute difference in mortality was close to zero (Risk difference=0.00 [-0.01, 0.00]; $\chi^2$=64.72, df=55, $p=0.17$; $I^2=15\%$). Effects for specified follow-up periods were similar to the effect at longest follow-up (Table 3).

*Table 3: Mortality by follow-up period (Risk ratio)*

<table>
<thead>
<tr>
<th>Months</th>
<th>Included in Analysis</th>
<th>RR (95% CI), random effects</th>
<th>Heterogeneity $I^2$; $\chi^2$ (p value)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Trials (%)</td>
<td>People (%)</td>
<td></td>
</tr>
<tr>
<td>0-11</td>
<td>15 (23%)</td>
<td>4533 (16%)</td>
<td>0.85 (0.67 to 1.08)</td>
</tr>
<tr>
<td>12-23</td>
<td>32 (50%)</td>
<td>10759 (38%)</td>
<td>0.89 (0.78 to 1.02)</td>
</tr>
</tbody>
</table>
When we compared studies by focus of intervention, average age, or number of visits, some individual effects were statistically significant, but we did not find evidence of statistically significant differences among the subgroups, so these results should be interpreted with caution. For example, there was some heterogeneity across types of visitors; there was no overall benefit of interventions delivered exclusively by nurses, yet the effect of interventions delivered by other health care practitioners was statistically significant (Table 4).

Table 4: Subgroup analyses for mortality (Risk ratio)

<table>
<thead>
<tr>
<th>Subgroup</th>
<th>Included in Analysis</th>
<th>RR (95% CI), random effects</th>
<th>Heterogeneity</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Trials (%)</td>
<td>People (%)</td>
<td>I²; Chi² (p value)</td>
</tr>
<tr>
<td>All studies</td>
<td>55 (86%)</td>
<td>24198 (84%)</td>
<td>0.93 (0.87 to 0.99)</td>
</tr>
<tr>
<td>Focus of Intervention</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Falls</td>
<td>12</td>
<td>2632</td>
<td>0.88 (0.66 to 1.18)</td>
</tr>
<tr>
<td>MGA</td>
<td>23</td>
<td>15011</td>
<td>0.95 (0.86 to 1.05)</td>
</tr>
<tr>
<td>Both</td>
<td>15</td>
<td>5278</td>
<td>0.82 (0.72 to 0.95)</td>
</tr>
<tr>
<td>Neither</td>
<td>5</td>
<td>1277</td>
<td>0.84 (0.61 to 1.16)</td>
</tr>
<tr>
<td>Average Age</td>
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<tr>
<td>≤70</td>
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<td>198</td>
<td>1.43 (0.88 to 2.33)</td>
</tr>
<tr>
<td>71-75</td>
<td>9</td>
<td>2914</td>
<td>0.97 (0.73 to 1.28)</td>
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<tr>
<td>76-80</td>
<td>26</td>
<td>9950</td>
<td>0.85 (0.76 to 0.94)</td>
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<td>81-85</td>
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<td>0.95 (0.83 to 1.10)</td>
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<td>25</td>
<td>12132</td>
<td>1.00 (0.91 to 1.10)</td>
</tr>
<tr>
<td>Other</td>
<td>14</td>
<td>4937</td>
<td>0.78 (0.69 to 0.89)</td>
</tr>
<tr>
<td>Combined</td>
<td>16</td>
<td>7129</td>
<td>0.97 (0.84 to 1.11)</td>
</tr>
<tr>
<td>Number of visits</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>7</td>
<td>2265</td>
<td>0.86 (0.57 to 1.29)</td>
</tr>
<tr>
<td>2-4</td>
<td>19</td>
<td>8038</td>
<td>0.84 (0.75 to 0.93)</td>
</tr>
<tr>
<td>5 or more</td>
<td>22</td>
<td>8856</td>
<td>0.96 (0.86 to 1.09)</td>
</tr>
</tbody>
</table>
3.3.1.2 Institutionalisation
Twenty-seven studies (42%) including 16459 participants (57%) reported the number of participants in each group who were admitted to an institution during the study. There was moderate quality evidence of no clinically significant difference at longest follow-up (Risk ratio=1.02 [0.88, 1.18]; Chi²=37.64, df=26, p=0.07; I²=31%). Akin to the mortality outcomes, we did not find evidence of statistically significant differences among the subgroups.

Five studies (8%) including 1718 participants (6%) reported the number of days that participants in each group spent in an institution during the study. There was very low quality evidence of a clinically small yet statistically significant effect at longest follow-up (Rate ratio=0.81 [0.79, 0.83]), yet the results were extremely inconsistent (Chi²=2269.85, df=3, p< 0.00001; I²=100%). We did not compare subgroups because there were too few studies.

3.3.1.3 Hospitalisation
Fifteen studies (23%) including 6288 participants (22%) reported the number of people admitted to hospital in each group. There was moderate quality evidence of no clinically or statistically significant difference at longest follow-up (Risk ratio=0.96 [0.91, 1.01]; Chi²=13.70, df=14, p=0.47; I²=0%). While we generally did not find evidence of statistically significant differences among the subgroups, there was a statistically significant difference among types of visitors, similar to the results for mortality.

Twelve studies (19%) including 4573 participants (16%) reported the number of days spent in hospital. There was very low quality evidence of a clinically small yet statistically significant effect at longest follow-up (Rate ratio=0.83 [0.72, 0.95]); as with days in institution, the results were extremely inconsistent (Chi²=933.29, df=12, p< 0.00001; I²=99%). Some subgroup analyses were heterogeneous and consequently the results were difficult to interpret.

Eleven studies (17%) including 4943 participants (17%) reported the number of admissions to hospital. There was low quality evidence of no statistically significant difference at longest follow-up, which would be consistent with no effect or a small clinically significant effect (Rate ratio=0.93 [0.81, 1.06]; Chi²=28.07, df=11, p=0.003; I²=61%). There was no evidence of any statistically significant differences within subgroups except a statistically significant difference among types of visitors that was not consistent with the results for people admitted to hospital or mortality.

Twelve studies (19%) including 4321 participants (15%) reported the number of people who visited the A&E in each group. There was moderate quality evidence of no statistically significant difference at longest follow-up, which would be consistent with no effect or a small clinically significant effect (Risk ratio=0.91 [0.81, 1.03]; Chi²=16.29, df=11, p=0.13; I²=32%). There was no evidence of any statistically significant differences among subgroups.
Ten studies (16%) including 5870 participants (20%) reported the number of A&E visits. There was low quality evidence of no statistically significant difference at longest follow-up, which would be consistent with no effect or a small clinically significant effect (Rate ratio=0.92 [0.81, 1.04]; Chi²=35.81, df=9, p<0.0001; I²=75%). Several differences across subgroups were statistically significant because one group in several analyses included only one study that was inconsistent with others; barring this limitation, there were no significant clinical differences.

### 3.3.1.4 Falls

Twenty-three studies (36%) including 7455 (26%) participants reported the number of people who fell. One study reported an adjusted effect that could not be combined with other measures to estimate a relative risk, so an overall odds ratio was calculated (Stevens 2001). There was moderate quality evidence of a small clinically significant effect at longest follow-up, but it was not statistically significant (Odds ratio=0.86 [0.73, 1.01]; Chi²=43.59, df=22, p=0.004; I²=50%). Most effects were measured after about 12 months; two studies reporting longer follow-up report no statistically significant evidence of extended benefits (Table 5). There was no statistically significant evidence of any differences among subgroups except for number of visits, though none of the sub-group effects were statistically significant. Only one study reported falls but did not explicitly target falls prevention.

Fifteen studies (23%) including 5319 (19%) participants reported number of falls. There was low quality evidence of a small clinically and statistically significant effect at longest follow-up (Rate ratio=0.74 [0.58, 0.93]), but as with days in hospital or days in institution, the results were extremely inconsistent (Chi²=4574.87, df=14, p<0.00001; I²=100%). Some subgroup analyses were heterogeneous and as such the results were difficult to interpret.

Fourteen studies (22%) including 2574 (9%) participants reported perceived risk of falling (e.g., fear of falling, self-confidence in avoiding falling). There was very low quality evidence of a small clinical and statistically significant effect at longest follow-up (SMD=-0.16 [-0.26, -0.07]; Chi²=18.26, df=13, p=0.15; I²=29%). Some subgroup analyses were heterogeneous as a consequence of groups with few studies; as in other analyses, clinical effects for nurse-led interventions may have been smaller than effects for other interventions.

#### Table 5: People who fell by follow-up period (Odds ratio)

<table>
<thead>
<tr>
<th>Months</th>
<th>Included in Analysis</th>
<th>OR (95% CI), random effects</th>
<th>Heterogeneity I²; Chi² (p value)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Trials (%)</td>
<td>People (%)</td>
<td></td>
</tr>
<tr>
<td>0-11</td>
<td>7 (11%)</td>
<td>1433 (5%)</td>
<td>1.06 (0.73 to 1.54)</td>
</tr>
<tr>
<td>12-23</td>
<td>16 (25%)</td>
<td>5366 (19%)</td>
<td>0.77 (0.66 to 0.91)</td>
</tr>
<tr>
<td>24-35</td>
<td>1 (2%)</td>
<td>771 (3%)</td>
<td>1.11 (0.79 to 1.57)</td>
</tr>
</tbody>
</table>

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### Table 6: Subgroup analyses for people who fell (Odds ratio)

<table>
<thead>
<tr>
<th>Subgroup</th>
<th>Included in Analysis</th>
<th>OR (95% CI), random effects</th>
<th>Heterogeneity</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Trials (%)</td>
<td>People (%)</td>
<td></td>
</tr>
<tr>
<td>All studies</td>
<td>23 (36%)</td>
<td>7455 (26%)</td>
<td>0.86 (0.73 to 1.01)</td>
</tr>
<tr>
<td>Focus of Intervention</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Falls</td>
<td>11 (17%)</td>
<td>3848 (13%)</td>
<td>0.87 (0.73 to 1.02)</td>
</tr>
<tr>
<td>MGA</td>
<td>1 (2%)</td>
<td>771 (3%)</td>
<td>1.11 (0.79 to 1.57)</td>
</tr>
<tr>
<td>Both</td>
<td>11 (17%)</td>
<td>2836 (10%)</td>
<td>0.84 (0.61 to 1.16)</td>
</tr>
<tr>
<td>Neither</td>
<td>0 (0%)</td>
<td>0 (0%)</td>
<td>Not applicable</td>
</tr>
<tr>
<td>Average Age</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>≤70</td>
<td>0 (0%)</td>
<td>0 (0%)</td>
<td>Not applicable</td>
</tr>
<tr>
<td>71-75</td>
<td>6 (9%)</td>
<td>1242 (4%)</td>
<td>1.01 (0.67 to 1.54)</td>
</tr>
<tr>
<td>76-80</td>
<td>12 (19%)</td>
<td>5064 (18%)</td>
<td>0.85 (0.69 to 1.06)</td>
</tr>
<tr>
<td>81-85</td>
<td>4 (6%)</td>
<td>791 (3%)</td>
<td>0.76 (0.48 to 1.22)</td>
</tr>
<tr>
<td>86+</td>
<td>1 (2%)</td>
<td>358 (1%)</td>
<td>0.72 (0.45 to 1.15)</td>
</tr>
<tr>
<td>Type of Visitor</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Nurse</td>
<td>7 (11%)</td>
<td>3293 (11%)</td>
<td>1.02 (0.85 to 1.24)</td>
</tr>
<tr>
<td>Other</td>
<td>8 (13%)</td>
<td>1650 (6%)</td>
<td>0.77 (0.49 to 1.21)</td>
</tr>
<tr>
<td>Combined</td>
<td>8 (13%)</td>
<td>2512 (9%)</td>
<td>0.79 (0.65 to 0.96)</td>
</tr>
<tr>
<td>Number of visits</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>7 (11%)</td>
<td>3873 (14%)</td>
<td>0.82 (0.62 to 1.08)</td>
</tr>
<tr>
<td>2-4</td>
<td>7 (11%)</td>
<td>1662 (6%)</td>
<td>0.87 (0.61 to 1.24)</td>
</tr>
<tr>
<td>5 or more</td>
<td>6 (9%)</td>
<td>1114 (4%)</td>
<td>0.74 (0.54 to 1.01)</td>
</tr>
</tbody>
</table>

### 3.3.1.5 Injuries

Ten studies (16%) including 3055 (11%) participants reported the number of people injured, but one study reported only a hazard ratio (Luukinen, 2006). In the remaining studies, there was moderate quality evidence of a small clinical and statistically significant effect at longest follow-up (Risk ratio=0.70 [0.55, 0.88]; Chi²=4.11, df=8, p=0.85; I²=0%). For relatively rare events observed over a short time, there is likely to be little difference between the RR and HR. Because the
Hazard ratio uses the same scale as the risk ratio and can be interpreted similarly given the duration of the included studies, we also combined all 11 studies, and the overall effect was clinically small and statistically significant different (Risk ratio=0.77 [0.63, 0.95]; Chi²=7.24, df=9, p=0.61; I²=0%). There was no evidence of any differences among subgroups.

Seven studies (11%) including 3718 (13%) participants reported number of injuries. There was moderate quality evidence of no clinically significant difference at longest follow-up, though this effect was not statistically significant (Rate ratio=0.98 [0.87, 1.11]; Chi²=4.32, df=6, p=0.63; I²=0%). We did not compare subgroups because there were too few studies.

**3.3.1.6 Physical and cognitive functioning**

Twenty-seven studies (42%) including 8769 (31%) participants reported any measure of functioning (ADL or IADL). Some studies included validated measures of ADLs and IADLs, while others used less valid measures of functional capacity or physical disability. Several studies reported the number of people dependent or independent (or having difficulty) in specific activities (e.g. eating or dressing), but did not report an estimate of overall functioning (e.g., McEwan, 1990, Sorensen, 1988). Other studies reported the number of people with impairments, a mobility score, a disability score, or a daily activities score.

There was very low quality evidence of a small clinical yet statistically significant effect on ADLs and IADLs at longest follow-up (SMD=-0.10 [-0.17, -0.03]; Chi²=55.40, df=26, p=0.0007; I²=53%). There was no evidence of any statistically significant differences among subgroups.

Eight studies (13%) including 1608 (6%) of participants reported any measure of cognitive functioning. There was low quality evidence of no clinically or statistically significant difference at longest follow-up (SMD=-0.06 [-0.21, 0.09]; Chi²=12.49, df=7, p=0.09; I²=44%). We did not compare subgroups because there were too few studies.

**3.3.1.7 Quality of life**

Twenty-nine studies (45%) including 9892 participants (35%) reported any measure of health-related quality of life. Measures predominately were questionnaires related to self-rated health, including the SF-36 (Ware & Sherbourne, 1992), Well-Being Picture Scale (Gueldner et al., 2005), Dupuy General Well-Being questionnaire (Dupuy, 1978), and EuroQol (1990). There was low quality evidence of a statistically significant difference at longest follow-up, though this effect was not clinically significant (SMD=-0.06 [-0.11, -0.01], Chi²=35.69, df=28, p=0.15; I²=22%). There was no evidence of any statistically significant differences among subgroups.
3.3.1.8 Psychiatric illness (anxiety and depression)

Fifteen studies (23%) including 3318 participants (12%) reported psychiatric illness. Measures were related to anxiety, depression, and general mental health, such as the Beck Depression Inventory (Beck, Rial, & Rickels, 1974), Geriatric Depression Scale (Yesavage, Brink, & Rose, 1983), and the Hospital Anxiety and Depression Scale (Zigmond & Snaith, 1983). There was low quality evidence of no clinically significant effects at longest follow-up, and this result was not statistically significant (SMD=-0.10 [-0.18, 0.02]; Chi²=18.06, df=14, p=0.20; I²=22%). There was no evidence of any statistically significant differences among subgroups.
4 Discussion

4.1 SUMMARY OF THE MAIN RESULTS

Our review suggests that preventive home visits for community-dwelling older adults do not significantly reduce mortality and morbidity overall, but we cannot exclude the possibility that some programmes have small effects on mortality (Table 7). Due to the heterogeneity in the target population, differences in risk factors, and variation among home visiting programmes, this review cannot exclude the possibility that some programmes have modest effects on secondary outcomes. It is possible that some combination of home visit components in particular populations and settings could yield important beneficial outcomes (Moyer, 2012); however, the current evidence does not indicate what such a combination might be, and, based on our sub-group analyses, we were unable to distinguish any subset of interventions that reliably produce positive outcomes.

Table 7: Summary of Findings

Home Visits versus Controls for prevention of impairment and death in older adults

<table>
<thead>
<tr>
<th>Outcomes</th>
<th>Illustrative comparative risks*</th>
<th>Relative effect (95% CI)</th>
<th>No of Participants (studies)</th>
<th>Quality of the evidence (GRADE)</th>
<th>Comments</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mortality</td>
<td></td>
<td>RR 0.93 (0.87 to 0.99)</td>
<td>24198 (55 studies)</td>
<td>high</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Study population</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>139 per 1000</td>
<td>130 per 1000</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Low</td>
<td>50 per 47 per 1000</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Institution</td>
<td>Study population</td>
<td>RR</td>
<td>People</td>
<td>16459 (27 studies)</td>
<td></td>
</tr>
<tr>
<td>-------------</td>
<td>------------------</td>
<td>----------</td>
<td>--------</td>
<td>--------------------</td>
<td></td>
</tr>
<tr>
<td>Institutionalisation</td>
<td>85 per 1000</td>
<td>1.02 (0.88 to 1.18)</td>
<td>Low</td>
<td>moderate</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>40 per 1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>120 per 1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Falls People</td>
<td>200 per 1000</td>
<td>0.86 (0.73 to 1.01)</td>
<td>Low</td>
<td>moderate</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>600 per 1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hospitalisation</td>
<td>410 per 1000</td>
<td>0.96 (0.91 to 1.01)</td>
<td>Low</td>
<td>moderate</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>200 per 1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1000</td>
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<td></td>
</tr>
<tr>
<td></td>
<td>600 per 1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Functioning ADL/IADL</td>
<td>The mean functioning</td>
<td>8769 (27 studies)</td>
<td>very low</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>adl/iadl in the intervention groups was 0.10 standard deviations lower (0.17 to 0.03 lower)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Health Related QoL</td>
<td>The mean health related qol in the intervention groups was 0.06 standard deviations lower (0.11 to 0.01 lower)</td>
<td>9892 (29 studies)</td>
<td>low</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
The basis for the assumed risk (e.g. the median control group risk across studies) is provided in footnotes. The corresponding risk (and its 95% confidence interval) is based on the assumed risk in the comparison group and the relative effect of the intervention (and its 95% CI).

CI: Confidence interval; RR: Risk ratio; OR: Odds ratio;

GRADE Working Group grades of evidence

High quality: Further research is very unlikely to change our confidence in the estimate of effect.

Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.

Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.

Very low quality: We are very uncertain about the estimate.

1 Institutionalisation people was recorded and reported in several different ways within and across studies
2 Institutionalisation people is a main outcome for this intervention and this review, but only 57% of participants and 42% of studies are included in this analysis.
3 Heterogeneity was significant for this outcome (Chi²=43.59, df=22, p=0.004; I²=50%).
4 Trim and fill analysis imputed 6 studies and adjusted effect RR = 0.98 (0.91 - 1.06)
5 Blinding of outcome assessors was a significant risk of bias for this outcome.
6 Heterogeneity was significant for this outcome (Chi²=55.40, df=26, p=0.0007; I²=53%).
7 Functioning ADL/IADL is a main outcome for this intervention, and only 31% of participants and 42% of studies were included in this analysis.
8 This is a main outcome for this intervention, and only 35% of participants and 45% of studies were included in this analysis.

4.2 OVERALL COMPLETENESS AND APPLICABILITY OF THE EVIDENCE

This review includes a large number of studies and participants. The interventions and comparisons included are diverse, but pre-specified subgroup analyses formally investigated the most important sources of heterogeneity. Poor reporting quality limited further investigation into potential mediators and moderators, a problem that is widespread in reports of complex intervention trials (Grant, Mayo-Wilson, Melendez-Torres, & Montgomery, 2013).

4.3 QUALITY OF THE EVIDENCE

The quality of evidence varied across outcomes. There was high quality evidence for mortality and moderate quality evidence for some important outcomes. The main results were statistically precise with little evidence of heterogeneity. There was some evidence of bias, but this review finds no evidence of clinically significant benefits overall, so effects are probably not overestimated. Sensitivity analyses excluding those studies at high risk of bias and (i.e. those most likely to misestimate
There was lower quality evidence for secondary outcomes. Statistically significant effects were reported by a small subset of studies. One significant effect (people with injuries) was no longer significant after removing one study (Campbell, 1999), and other statistically significant results were not clinically meaningful. We consider it unlikely that the important outcomes are overestimated by publication bias, but a few secondary effects could be explained by selective outcome reporting.

Imprecision in measuring subjective outcomes is an issue of concern in this review (Huss et al., 2008). Results for these outcomes should be interpreted with caution (namely ADL/IADL, cognitive functioning, psychiatric anxiety and depression, and falls self-efficacy), as these data are restricted by detection bias due to lack of blinding (Beswick et al., 2008). Furthermore, from an epidemiological viewpoint, injuries are relatively rare events in this population—trials require a long-term follow up and a large sample size to be able to identify changes in injuries (McClure et al., 2005).

### 4.4 POTENTIAL BIASES IN THE REVIEW PROCESS

We conducted a highly sensitive search, double-coded all outcome data, analysed the most important outcomes, and report the results of all analyses to allow readers access to the data and results. We consider it unlikely that biases in the review process have impacted the conclusions, but we were not blind to publication or author names at any stage of extraction or analysis. We excluded one large factorial study that compared (i) targeted versus universal assessment and (ii) interventions delivered by either a geriatric team or a primary care team, and found no important differences between groups (Fletcher, 2004).

Two substantial issues in this literature flagged by previous reviewers (Huss et al., 2008) that may affect the accuracy of meta-analyses are poor individual trial reporting and the lack of a clear, underlying theory of change for the intervention. Firstly, trials rarely reported about participant compliance with home visit protocols and the content of concurrent services during the trial; considering that many of the programmes provided other elements, such as contacts with local health or community services, it is often difficult to assess the particular effects of home visiting. In addition, the existing research evaluating the effectiveness of preventive home visits generally lacks an underlying theoretical foundation (Markle-Reid et al., 2006). Consequently, it is difficult to assess the appropriateness of the outcomes being measured and to understand why or how a particular home visits program is supposed to produce particular outcomes (Elkan & Kendrick, 2004). In addition, currently possible meta-analytic subgroup analyses, like subgroup analyses within
trials, are prone to bias and confounding, and therefore should be interpreted with caution (Huss et al., 2008).

Whilst it is true that the included studies were diverse in terms of settings, types of visitor, focus of visits, and control conditions, all interventions in this review involved preventive home visits for independently-living older adults based on assessment of medical and social need, and sub-group analyses of important moderators did not consistently yield significant effects. Moreover, negligible to moderate heterogeneity was found for primary outcomes that did not demonstrate important effects, such as mortality, institutionalisation, and hospitalisation. Nonetheless, due to the issues related to poor reporting, theory of change, and subgroup analyses we describe above, results should be interpreted with caution, and any future research should aim to address these issues.

4.5 AGREEMENTS AND DISAGREEMENTS WITH OTHER STUDIES AND REVIEWS

Over the past 20 years, many reviews have investigated the effects of preventive home visiting. Including 64 randomised trials conducted over the last three decades (Luker, 1981), our results were not consistent with those of previous reviews that found beneficial effects for some groups of older adults on mortality and institutionalisation (Elkan et al., 2001; Huss et al., 2008; Stuck et al., 2002). Rather, our results corroborate previous reviews suggesting that home visit programmes do not appear to significantly differ in outcomes compared to usual care (Bouman et al., 2008; Turner et al., 2011; van Haastregt et al., 2000).

An early analysis concluded that comprehensive geriatric assessment may have several benefits (Stuck et al., 1993), but later reviews have come to conflicting conclusions about effects on mortality and specific morbidities in related populations (Bouman et al., 2008; Elkan et al., 2001; Huss et al., 2008; Stuck et al., 2002; van Haastregt et al., 2000). The present review is the most comprehensive in scope, including a wide range of outcomes and pre-specified subgroup analyses, and it includes several new studies because searches for the last large review were conducted in 2007 (Huss et al., 2008).

Our main results concur with the most recent review of multidimensional preventive home visits which concluded that there was no overall evidence of effects on mortality, nursing home admissions, or functioning (Huss et al., 2008). Poor reporting and lack of information about participants and programme characteristics prevented firm conclusions about potential mediators and moderators; however, the authors identified potential effects on mortality for younger participants and potential effects on functioning for interventions that include an initial clinical examination (Huss et al., 2008).
An overview of reviews identified a need for further analyses to investigate potential differences related to the focus of visits, number of visits, characteristics of participants, and characteristics of providers (Elkan & Kendrick, 2004). To the extent possible, the present review investigated these variables and failed to identify any patterns across outcomes that would be consistent with benefits overall or for any defined subgroup of interventions.

Previous authors have argued that trials in this field are uninformative as we require “a much better understanding of health visiting than we have now” before the outcomes could be understood (Clark, 2001). It remains true that most programmes have failed to articulate and to evaluate clear theories of change (Elkan & Kendrick, 2004). Measurable variables such as provider characteristics and the number and duration of visits may not fully capture the unique characteristics of effective interventions. Included interventions were complex and their effects relate to the effectiveness of local services; that is, they are both (i) compared with other services and (ii) the interventions frequently included other services, such as follow-up care from local practitioners (Bouman et al., 2008). For these reasons, previous subgroup analyses should be interpreted with caution; apparent differences could be the by-product of multiple analyses. It is particularly unclear why the effects of potential mediators and moderators would be restricted to a subset of the important and highly correlated outcomes.

More than a decade later, we agree that given ‘lack of insight into the predictors of programme success, we expect that it will be a difficult task to make improvements in the effectiveness of preventive home visits to older adults living in the community’ (van Haastregt et al., 2000). Echoing these conclusions, our results suggest that policymakers should consider discontinuing home visiting programs and funders should not support further trials unless researchers are able to articulate a clear theory of change, demonstrate that a new study could definitively test that theory, and explain how the new study would change conclusions based on a large body of existing evidence. If home visits have beneficial effects, they are probably small. Only an extremely large trial—or a programme of research leading to one—could be justified at this time; further small randomised trials would not meaningfully contribute to what is already known.
5 Author’s Conclusions

5.1 IMPLICATIONS FOR PRACTICE

This review suggests that not all home visits are associated with reductions in risk of mortality for community-dwelling older adults, but we cannot conclude that all of these programmes are ineffective. Furthermore, we did not find convincing evidence of benefits for secondary outcomes, including institutionalisation and hospitalisation. We conducted several moderator analyses to explore clinical heterogeneity of the included studies, but we were unable to identify any features that were reliably associated with programme success. Consequently, this review does not support the widespread and automatic adoption of home visiting programmes for preventing morbidity or mortality among community-dwelling older adults. Subgroup analyses were limited by inadequate reporting, particularly with respect to treatment received by participants in comparison groups, and some combinations of programme components might be effective for some populations. Therefore, if policymakers and practitioners wish to adopt such interventions for particular individuals or groups, this should be done with careful consideration given to the intervention components that may be useful to those individuals and what alternative care the home visit may replace, with the shared understanding that outcomes may not change as a result of the intervention.

Some of these home visits were part of larger programmes that may have positive effects, including: exercise, improved assessment by medical professionals, or falls prevention. However, no specific components appeared to distinguish effective programmes from ineffective programmes. Furthermore, it is not clear if interventions that occurred in people’s homes could be delivered more efficiently in other settings. Outcomes of these multi-factorial interventions are also likely to be sensitive to differences in care systems within and across nations (Gillespie et al., 2009). Some visits mostly included assessment and recommendations, but the efficacy of the intervention depends on adherence to such recommendations (e.g. removing fall hazards, exercising to improve strength, changing medications) and quality of complementary care. Additionally, identifying hazards with participants could have a more generalisable effect through improving problem-solving skills. Discovering the most efficient and effective interventions for this population thus still remains an important goal for practitioners and policy-makers.
5.2 IMPLICATIONS FOR RESEARCH

Given the size of this review and the number of previous reviews on this topic, further small studies comparing multi-component interventions to usual care are likely to add very little to the knowledge base. If researchers continue to evaluate these types of interventions, they should: begin with a clear theory of change (Eldridge et al., 2005); clearly describe programme design, delivery, and uptake (Underhill, Operario, & Montgomery, 2006) so that these intervention components can be fully considered in systematic reviews (Montgomery, Underhill, Operario, Gardner, & Mayo-Wilson, 2013), adhere to CONSORT guidelines for reporting trials (Grant, Montgomery, & Mayo-Wilson, 2012; Schulz, Altman, & Moher, 2010); and report all outcomes measured (Hart, Lundh, & Bero, 2012).

Several researchers (Clark, 2001; Elkan & Kendrick, 2004) have highlighted that a clear theory of change is needed to elucidate appropriate inputs, processes, and outcomes for preventive home visit programs. Many of the included studies in this review were poorly described and poorly reported. Due to poor reporting and unclear program theory, the effects of unmeasured yet important program characteristics consistently is not addressable by reviews in this field (Huss et al., 2008). Components that are often assessed—such as focus of the programme, professional background of providers, and number and length of visits—might not capture the key aspects of preventive home visit interventions (Bouman et al., 2008). Due to the lack of a theoretical founding, it is hard to know whether it is the content, frequency and length of visits, the providers, or some combination of the above that is the active ingredient in the intervention.

Future research should aim to clarify and evaluate the validity of models of program theory in order to determine what (if anything) might be effective for certain populations and settings. Given the difficulty in isolating the contribution of single components of a complex intervention, a theory-driven evaluation of intervention inputs, process, and context linked to outcomes could provide information about why, how, and under what circumstances home visits may or may not be effective (Markle-Reid et al., 2006). For example, future primary studies would benefit from greater focus on the process of delivering home visits and on analyses that aim to identify which components of the intervention are most important (Elkan et al., 2001).
The protocol for this review was published in 2006. The methods for the review have been updated to include the Cochrane Risk of Bias Tool (Higgins & Green, 2011) and quality assessments using GRADE criteria (Guyatt et al., 2010). Following current best practices, we decided to handle studies with more than two eligible interventions by combining the intervention groups rather than by ignoring one of the groups. We used RevMan 5.1 rather than RevMan 4.2 to perform the analyses. Following current recommendations, we combined dichotomous outcomes using Mantel-Haenszel methods. Studies with control conditions that explicitly involved home visits were excluded. Grey literature was not specifically searched but was screened for eligibility if located through other search methods.
7 Data and analyses

(These now moved to a separate file)
8 References

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**Elley 2008**


**Fabacher 1994**


**Gallagher 1996**


**Gitlin 2006**


**Green 2002**


**Gunner-Svensson 1984**


**Gustafsson 2012**


**Hall 1992**


**Hebert 2001**


**Hendriks 2008**

Hogan 2001

Holland 2005


Huang 2004

Kingston 2001

Kono 2004

Kono 2011


**Krebs 1998**


**Lenaghan 2007**


**Lightbody 2002**


**Lin 2007**


**Liu-Ambrose 2008**


**Luker 1981**


**Luukinen 2006**


**Markle-Reid 2006**


**Markle-Reid 2010**


**McEwan 1990**


**Nelson 2004**


**Newbury 2001**


**Pathy 1992**


**Pighills 2011**

Ploeg 2010


Robertson 2001a


Sahlen 2006


Shapiro 2002


Sommers 2000


Sorensen 1988


Stevens 2001


**Stuck 1995**


**Stuck 2000**


**Thomas 2007**


**Tinetti 1994**


van Haastregt 2000


van Hout 2010


van Rossum 1993


Vetter 1984a


Vetter 1984b


Vetter 1992


Wyman 2007

**Yamada 2003**


**References to Excluded Studies**

**Archbold 1995**


**Carpenter 1990**


**Clarke 1992**


**Clemson 2004**


**Comans 2010**

Dapp 2011


Day 2002


de Vries 2010


Dunn 1994


Engelhardt 1996


Epstein 1990


**Ettinger 1997**


**Fairhall 2008**


**Fletcher 2004**


**Fordyce 1997**

**Fox 2010**


**German 1995**


**Gill 2002**


**Graham 2006**


**Hansen 1992**


**Hay 1998**


**Hendriksen 1984**

**Hornbrook 1994**


**June 2009**


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**Matzen 2007b**


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**Melin 1992**


**Melis 2005**

**Melis 2008**


**Miller 1996**


**Moore 1997**


**Newcomer 2004**


**Oktay 1990**


**Parsons 2011**


**Peeters 2007**


**Poulsen 2007**


**Poulstrup 2000**


**Robertson 2001b**


**Robichaud 2000**


**Rosie 2007**


**Salminen 2008**


**Salminen 2009a**

Salminen 2009b


Schraeder 2007


Scogin 2007


Silverman 1995


Sjosten 2007a


Sjosten 2007b


Sjosten 2008


Spice 2009

Steinberg 2000


Stewart 2005


Theander 2005


Toseland 1996


Townsend 1988


Tulloch 1979


Vaapio 2007


**Vass 2005**


**von Renteln 2003**


**Wagner 1994**


**Wallace 1998**


**Wasson 1999**


**Whitehead 2003**


**Williams 1992**


**Williams 2002**


**Wolf 2001**


**Yates 2001**


**Zimmer 1985**


**References to Studies Awaiting Classification**

**Jingna 2012**


**Jitapunkul 1998**


**References to Ongoing Studies**

**Cutchin 2009**

Ferrer 2010


Fleischer 2008


Hinrichs 2009


Additional References


## Author Contact Details

### REVIEW AUTHORS

#### Lead review author:

<table>
<thead>
<tr>
<th>Name:</th>
<th>Sean Grant</th>
</tr>
</thead>
<tbody>
<tr>
<td>Title:</td>
<td>Doctoral Student</td>
</tr>
<tr>
<td>Affiliation:</td>
<td>Centre for Evidence-Based Intervention, University of Oxford</td>
</tr>
<tr>
<td>Address:</td>
<td>Barnett House 32 Wellington Square</td>
</tr>
<tr>
<td>City, State, Province or County:</td>
<td>Oxford</td>
</tr>
<tr>
<td>Postal Code:</td>
<td>OX1 2ER</td>
</tr>
<tr>
<td>Country:</td>
<td>United Kingdom</td>
</tr>
<tr>
<td>Phone:</td>
<td>+44 1865 270325</td>
</tr>
<tr>
<td>Mobile:</td>
<td></td>
</tr>
<tr>
<td>Email:</td>
<td><a href="mailto:sean.grant@spi.ox.ac.uk">sean.grant@spi.ox.ac.uk</a></td>
</tr>
</tbody>
</table>

#### Co-authors:

<table>
<thead>
<tr>
<th>Name:</th>
<th>Amanda Parsons</th>
</tr>
</thead>
<tbody>
<tr>
<td>Title:</td>
<td>J.D. Candidate</td>
</tr>
<tr>
<td>Affiliation:</td>
<td>Yale Law School</td>
</tr>
<tr>
<td>Address:</td>
<td>127 Wall Street</td>
</tr>
<tr>
<td>City, State, Province or County:</td>
<td>New Haven, CT</td>
</tr>
<tr>
<td>Postal Code:</td>
<td>06511</td>
</tr>
<tr>
<td>Country:</td>
<td>USA</td>
</tr>
<tr>
<td>Phone:</td>
<td></td>
</tr>
<tr>
<td>Mobile:</td>
<td></td>
</tr>
<tr>
<td>Email:</td>
<td><a href="mailto:amanda.parsons@yale.edu">amanda.parsons@yale.edu</a></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Name:</th>
<th>Jennifer Burton</th>
</tr>
</thead>
<tbody>
<tr>
<td>Title:</td>
<td>Research Fellow</td>
</tr>
</tbody>
</table>
Affiliation: Centre for Evidence-Based Intervention, University of Oxford  
Address: Barnett House  
32 Wellington Square  
City, State, Province or County: Oxford  
Postal Code: OX1 2ER  
Country: United Kingdom  
Phone: +44 1865 270325  
Email: jennifer.burton@spi.ox.ac.uk

<table>
<thead>
<tr>
<th>Name:</th>
<th>Paul Montgomery</th>
</tr>
</thead>
<tbody>
<tr>
<td>Title:</td>
<td>Professor of Psycho-Social Intervention</td>
</tr>
<tr>
<td>Affiliation:</td>
<td>Centre for Evidence-Based Intervention, University of Oxford</td>
</tr>
</tbody>
</table>
| Address:     | Barnett House  
32 Wellington Square  
City, State, Province or County: Oxford  
Postal Code: OX1 2ER  
Country: United Kingdom  
Phone: +44 1865 270325  
Email: paul.montgomery@spi.ox.ac.uk

<table>
<thead>
<tr>
<th>Name:</th>
<th>Kristen Underhill</th>
</tr>
</thead>
<tbody>
<tr>
<td>Title:</td>
<td>Associate Research Scholar in Law</td>
</tr>
</tbody>
</table>
| Affiliation: | Yale Law School  
127 Wall Street  
City, State, Province or County: New Haven, CT  
Postal Code: 06511  
Country: USA  
Email: kristen.underhill@yale.edu
<table>
<thead>
<tr>
<th>Name:</th>
<th>Evan Mayo-Wilson</th>
</tr>
</thead>
<tbody>
<tr>
<td>Title:</td>
<td>Senior Research Associate</td>
</tr>
<tr>
<td>Affiliation:</td>
<td>Centre for Outcomes Research and Effectiveness, University College London</td>
</tr>
<tr>
<td>Address:</td>
<td>Room 354, 1-19 Torrington Place</td>
</tr>
<tr>
<td>City, State, Province or County:</td>
<td>London</td>
</tr>
<tr>
<td>Postal Code:</td>
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</tr>
<tr>
<td>Mobile:</td>
<td></td>
</tr>
<tr>
<td>Email:</td>
<td><a href="mailto:e.mayo-wilson@ucl.ac.uk">e.mayo-wilson@ucl.ac.uk</a></td>
</tr>
</tbody>
</table>
10 Appendices

Appendix 1: Search Strategies

Ovid MEDLINE(R)
1 House Calls/
2 ((home$ or in-home$ or domiciliary) adj2 (visit$ or support$ or care$ or service$)).tw.
3 (visit$ adj2 (nurse$ or doctor$ or physician$ or volunteer$ or health)).tw.
4 (preventive adj2 (program$ or visit$)).tw.
5 (health promotion$ or health education or health screening$ or geriatric assessment$ or preventive assessment$).mp.
6 or/1-5

7 exp Aged/
8 (((elderly or aged or old$) adj2 (person$ or people$ or man or men or woman or women or patient$)) or geriatric$ or senior citizen$).tw.
9 or/7-8

10 6 and 9

11 clinical trial.pt.
12 randomized controlled trial.pt.
13 controlled clinical trial.pt.
14 randomi?ed.ab.
15 placebo.ab.
16 randomly.ab.
17 trial.ti.
18 "clinical trials".mp.
19 or/11-18
20 Animals/
21 Humans/
22 20 not (20 and 21)
23 19 not 22

24 23 and 10
Ovid EMBASE
1 (home$ or in-home$ or domiciliary adj2 (visit$ or support$ or care$ or service$)).tw.
2 (visit$ adj2 (nurse$ or doctor$ or physician$ or volunteer$ or health)).tw.
3 (preventive adj2 (program$ or visit$)).tw.
4 (health promotion$ or health education or health screening$ or geriatric assessment$ or preventive assessment$).mp.

5 exp Aged/
6 (((elderly or aged or old$) adj2 (person$ or people$ or man or men or woman or women or patient$)) or geriatric$ or senior citizen$).tw.

7 or/5-6
8 or/1-4
9 7 and 8

10 Clinical Trial/
11 Randomized Controlled Trial/
12 Randomization/
13 Double Blind Procedure/
14 Single Blind Procedure/
15 Crossover Procedure/
16 PLACEBO/
17 placebo$.tw.
18 randomi?ed controlled trial$.tw.
19 rct.tw.
20 random allocation.tw.
21 randomly allocated.tw.
22 allocated randomly.tw.
23 (allocated adj2 random).tw.
24 single blind$.tw.
25 double blind$.tw.
26 ((treble or triple) adj blind$).tw.
27 Prospective study/
28 or/10-27
29 Case study/
30 case report.tw.
31 Abstract report/
32 Letter/
33 Editorial/
34 Note/
35 Human/
36 Nonhuman/
37 ANIMAL/
38 Animal Experiment/
39 36 or 37 or 38
40 39 not (35 and 39)
41 or/29-34,40
42 28 not 41
43 42 and 9

**Ovid PsycINFO**

1 exp Home Visiting Programs/
2 ((home$ or in-home$ or domiciliary) adj2 (visit$ or support$ or care$ or service$)).tw.
3 (visit$ adj2 (nurse$ or doctor$ or physician$ or volunteer$ or health)).tw.
4 (preventive adj2 (program$ or visit$)).tw.
5 (health promotion$ or health education or health screening$ or geriatric assessment$ or preventive assessment$).mp.
6 or/1-5

7 exp Aged/
8 (((elderly or aged or old$) adj2 (person$ or people$ or man or men or woman or women or patient$)) or geriatric$ or senior citizen$).tw.
9 or/7-8

10 6 and 9

11 methodology/
12 data collection/
13 empirical methods/
14 Experimental methods/
15 Quasi experimental methods/
16 experimental design/
17 between groups design/
18 followup studies/
19 exp longitudinal studies/
20 repeated measures/
21 experimental subjects/
22 experiment controls/
23 experimental replication/
24 exp "sampling (experimental)"/
25 placebo/
clinical trials/
exp treatment outcomes/
treatment effectiveness evaluation/
empirical study.md.
experimental replication.md.
followup study.md.
longitudinal study.md.
meta analysis.md.
prospective study.md.
retrospective study.md.
treatment outcome clinical trial.md.
placebo$.tw.
randomi?ed controlled trial$.tw.
rect.tw.
random allocation.tw.
(randomly adj1 allocated).tw.
(allocated adj2 random).tw.
((singl$ or doubl$ or treb$ or tripl$) adj (blind$3 or mask$3)).tw.
(clinic$ adj (trial? or stud$3)).tw.
or/11-44
comment reply.dt.
editorial.dt.
letter.dt.
clinical case study.md.
nonclinical case study.md.
animal.po.
human.po.
51 not (51 and 52)
or/46-50,53
45 not 54
10 and 55

Central (Cochrane Central Register of Trials)

#1 MeSH descriptor House Calls explode all trees
#2 (home* or in-home* or domiciliary) near (visit* or support* or care* or service*):ti,ab,kw
#3 ("health promotion"* or "health education" or "health screening"* or "geriatric assessment"* or "preventive assessment"*):ti,ab,kw
#4 (visit* near (nurse* or doctor* or physician* or volunteer* or health)):ti,ab,kw
#5 (preventive near (program* or visit*)):ti,ab,kw
#6 (#1 OR #2 OR #3 OR #4 OR #5)
#7 MeSH descriptor Aged explode all trees
### IBSS (International Bibliography of the Social Sciences)

1. **KW**=(house call*) or **TI**=(house call*) or **AB**=(house call*)

2. **KW**=((home* or in-home* or domiciliary) within 2 (visit* or support* or care* or service*)) or **AB**=((home* or in-home* or domiciliary) within 2 (visit* or support* or care* or service*)) or **TI**=((home* or in-home* or domiciliary) within 2 (visit* or support* or care* or service*))

3. **KW**=(visit* within 2 (nurse* or doctor* or physician* or volunteer* or health)) or **TI**=(visit* within 2 (nurse* or doctor* or physician* or volunteer* or health)) or **AB**=(visit* within 2 (nurse* or doctor* or physician* or volunteer* or health))

4. (TI=preventive within 2 (program* or visit*)) or (AB=preventive within 2 (program* or visit*)) or (KW=preventive within 2 (program* or visit*))

5. (TI=health promotion* or health education or health screening* or geriatric assessment* or preventive assessment*) or (KW=health promotion* or health education or health screening* or geriatric assessment* or preventive assessment*) or (AB=health promotion* or health education or health screening* or geriatric assessment* or preventive assessment*)

6. Or/1-5

7. **DE**="elderly"

8. **KW**=(((elderly or aged or old*) within 2 (person* or people* or man or men or woman or women or patient*)) or geriatric* or senior citizen*) or **TI**=(((elderly or aged or old*) within 2 (person* or people* or man or men or woman or women or patient*)) or geriatric* or senior citizen*) or **AB**=(((elderly or aged or old*) within 2 (person* or people* or man or men or woman or women or patient*)) or geriatric* or senior citizen*)

9. 7 or 8
10. (random* or control* or blind* or double-blind* or trial* or experiment* or RCT*)

**Sociological Abstracts**

1. KW=(house call*) or TI=(house call*) or AB=(house call*)

2. KW=((home* or in-home* or domiciliary) within 2 (visit* or support* or care* or service*)) or AB=((home* or in-home* or domiciliary) within 2 (visit* or support* or care* or service*)) or TI=((home* or in-home* or domiciliary) within 2 (visit* or support* or care* or service*))

3. KW=(visit* within 2 (nurse* or doctor* or physician* or volunteer* or health)) or TI=(visit* within 2 (nurse* or doctor* or physician* or volunteer* or health)) or AB=(visit* within 2 (nurse* or doctor* or physician* or volunteer* or health))

4. (TI=preventive within 2 (program* or visit*)) or (AB=preventive within 2 (program* or visit*)) or (KW=preventive within 2 (program* or visit*))

5. (TI=health promotion* or health education or health screening* or geriatric assessment* or preventive assessment*) or (KW=health promotion* or health education or health screening* or geriatric assessment* or preventive assessment*) or (AB=health promotion* or health education or health screening* or geriatric assessment* or preventive assessment*)

6. Or/1-5

7. DE="elderly"

8. KW=((((elderly or aged or old*) within 2 (person* or people* or man or men or woman or women or patient*)) or geriatric* or senior citizen*)) or TI=((((elderly or aged or old*) within 2 (person* or people* or man or men or woman or women or patient*)) or geriatric* or senior citizen*)) or AB=((((elderly or aged or old*) within 2 (person* or people* or man or men or woman or women or patient*)) or geriatric* or senior citizen*))

9. 7 or 8

10. (random* or control* or blind* or double-blind* or trial* or experiment* or RCT*)
C2-SPECTR

1. house call*
2. (home* or in-home* or domiciliary*) and (visit* or support* or care* or service*)
3. visit* and (nurse* or doctor* or physician* or volunteer$ or health)
4. preventive and (program* or visit*)
5. health promotion* or health education or health screening* or geriatric assessment* or preventive assessment*
6. or/1-5

OVID Cinahl

1. House Calls/
2. ((home$ or in-home$ or domiciliary) adj2 (visit$ or support$ or care$ or service$)).tw.
3. (visit$ adj2 (nurse$ or doctor$ or physician$ or volunteer$ or health)).tw.
4. (preventive adj2 (program$ or visit$)).tw.
5. (health promotion$ or health education or health screening$ or geriatric assessment$ or preventive assessment$).mp.
6. or/1-5

7. exp Aged/
8. (((elderly or aged or old$) adj2 (person$ or people$ or man or men or woman or women or patient$)) or geriatric$ or senior citizen$).tw.
9. or/7-8

10. 6 and 9

11. clinical trial.pt.
12. randomized controlled trial.pt.
13. controlled clinical trial.pt.
14. randomized.ab.
15. placebo.ab.
16. Clinical Trials/
17. randomly.ab.
18. trial.ti.
19. or/11-18
20. Animals/
21. Humans/
22. 20 not (20 and 21)
23. 19 not 22

24. 23 and 10
Ovid British Nursing Index and Archive

1  ((home$ or in-home$ or domiciliary) adj2 (visit$ or support$ or care$ or service$)).tw.
2  (visit$ adj2 (nurse$ or doctor$ or physician$ or volunteer$ or health)).tw.
3  (preventive adj2 (program$ or visit$)).tw.
4  (health promotion$ or health education or health screening$ or geriatric assessment$ or preventive assessment$).mp.
5  or/1-4
6  (((elderly or aged or old$) adj2 (person$ or people$ or man or men or woman or women or patient$)) or geriatric$ or senior citizen$).tw.
7  (older patients or older people).sh.
8  (elderly or elderly nursing or elderly services).sh.
9  or/6-8
10  5 and 9

Ovid Nursing Full Text Plus

1  exp House Calls/
2  ((home$ or in-home$ or domiciliary) adj2 (visit$ or support$ or care$ or service$)).tw.
3  (visit$ adj2 (nurse$ or doctor$ or physician$ or volunteer$ or health)).tw.
4  (preventive adj2 (program$ or visit$)).tw.
5  (health promotion$ or health education or health screening$ or geriatric assessment$ or preventive assessment$).mp.
6  or/1-5
7  exp Aged/
8  (((elderly or aged or old$) adj2 (person$ or people$ or man or men or woman or women or patient$)) or geriatric$ or senior citizen$).tw.
9  or/7-8
10  6 and 9
11  exp "clinical trial [publication type]"
12  randomized controlled trial.pt.
13  controlled clinical trial.pt.
14  randomized.ab.
15  placebo.ab.
16  Clinical Trials/
17  randomly.ab.
18  trial.ti.
19  or/11-18
20  Animals/
21  Humans/
22  20 not (20 and 21)
23  19 not 22
24  23 and 10
### Appendix 2: Data Extraction Template

<table>
<thead>
<tr>
<th><strong>Place of recruitment:</strong></th>
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</thead>
<tbody>
<tr>
<td><strong>Inclusion criteria:</strong></td>
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<tr>
<td><strong>Number randomised:</strong></td>
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</tr>
<tr>
<td><strong>Mean age:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Sex (% female):</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Mortality per year in the comparison group:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Living alone (%):</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Visitors' professional group:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Frequency and duration of visits:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Fall prevention:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Included exercise:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Included multidimensional geriatric assessment:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Description of intervention:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Description of comparison:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Implementation (fidelity and participant compliance):</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Outcome measures:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Location:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Funding Source:</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Other notes:</strong></td>
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</tbody>
</table>
### Appendix 4: Table of Excluded Studies

<table>
<thead>
<tr>
<th>Study ID</th>
<th>Reason for exclusion</th>
</tr>
</thead>
<tbody>
<tr>
<td>Archbold 1995</td>
<td>Intervention for families, not for the elderly themselves.</td>
</tr>
<tr>
<td>Carpenter 1990</td>
<td>Visits not conducted by health professional.</td>
</tr>
<tr>
<td>Clarke 1992</td>
<td>Visits not conducted by health professional.</td>
</tr>
<tr>
<td>Clemson 2004</td>
<td>Intervention not a home visit: intervention conducted in community venue with follow-up home visit.</td>
</tr>
<tr>
<td>Comans 2010</td>
<td>Ineligible comparison: both intervention and control groups received home visits.</td>
</tr>
<tr>
<td>Dapp 2011</td>
<td>Only 8.8% of intervention participants opted for home visits.</td>
</tr>
<tr>
<td>Day 2002</td>
<td>Intervention not a home visit. All arms of intervention (including controls) were assessed at home by nurses.</td>
</tr>
<tr>
<td>de Vries 2010</td>
<td>Home visits not part of intervention.</td>
</tr>
<tr>
<td>Dunn 1994</td>
<td>Visit directly related to hospital discharge.</td>
</tr>
<tr>
<td>Engelhardt 1996</td>
<td>Intervention not a home visit: intervention carried out in outpatient clinic.</td>
</tr>
<tr>
<td>Epstein 1990</td>
<td>Intervention not a home visit: assessment conducted in hospital.</td>
</tr>
<tr>
<td>Ettinger 1997</td>
<td>Intervention not a home visit.</td>
</tr>
<tr>
<td>Fairhall 2008</td>
<td>Visit directly related to hospital discharge.</td>
</tr>
<tr>
<td>Fletcher 2004</td>
<td>Intervention not a home visit: less than half the assessments were conducted in participants' homes.</td>
</tr>
<tr>
<td>Fordyce 1997</td>
<td>Intervention not a home visit: health appraisal took place during office visits.</td>
</tr>
<tr>
<td>Fox 2010</td>
<td>Only 50.0% of intervention participants opted for home visits.</td>
</tr>
<tr>
<td>German 1995</td>
<td>Intervention not a home visit: preventive visits took place at physician’s office.</td>
</tr>
<tr>
<td>Gill 2002</td>
<td>Ineligible comparison: both intervention and control groups received home visits.</td>
</tr>
<tr>
<td>Graham 2006</td>
<td>Home visits not carried out by health professionals. Quasi experimental design.</td>
</tr>
<tr>
<td>Hansen 1992</td>
<td>Visit directly related to hospital discharge.</td>
</tr>
<tr>
<td>Hay 1998</td>
<td>Intervention not a home visit: screening took place in primary care practice.</td>
</tr>
<tr>
<td>Hendriksen 1984</td>
<td>Not a randomised controlled trial: study refusers did not have equal chance of being assigned to either group. Informed consent followed randomisation.</td>
</tr>
<tr>
<td>Hornbrook 1994</td>
<td>Visits not conducted by health professional</td>
</tr>
<tr>
<td>June 2009</td>
<td>Not a randomised controlled trial</td>
</tr>
<tr>
<td>Karatay 2011</td>
<td>Visits not conducted by health professional – students.</td>
</tr>
<tr>
<td>Kerse 1999</td>
<td>Intervention targeted general practitioners</td>
</tr>
<tr>
<td>Kronborg 2006</td>
<td>Intervention targeted visitors</td>
</tr>
<tr>
<td>Legault 2011</td>
<td>Intervention not a home visit: centre-based intervention.</td>
</tr>
<tr>
<td>Study</td>
<td>Description</td>
</tr>
<tr>
<td>---------------</td>
<td>---------------------------------------------------------------------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>Leveille 1998</td>
<td>Intervention not a home visit: intervention carried out at senior centre.</td>
</tr>
<tr>
<td>Mahoney 2007</td>
<td>Ineligible comparison: both intervention and control groups received home visits.</td>
</tr>
<tr>
<td>Mann 1999</td>
<td>Visits directly related to rehabilitation and delivered to non-independent elderly.</td>
</tr>
<tr>
<td>Matzen 2007a</td>
<td>The intervention group received an initial assessment at home with follow-up home visits, and the control group was assessed in hospital and received follow-up home visits. As both groups received home visits as part of their allocated intervention, the comparison was ineligible.</td>
</tr>
<tr>
<td>Matzen 2007b</td>
<td>The intervention group received an initial assessment at home with follow-up home visits, and the control group was assessed in hospital and received follow-up home visits. As both groups received home visits as part of their allocated intervention, the comparison was ineligible.</td>
</tr>
<tr>
<td>McMurdo 1996</td>
<td>Ineligible comparison: control group received home visits.</td>
</tr>
<tr>
<td>Melin 1992</td>
<td>Visit directly related to hospital discharge.</td>
</tr>
<tr>
<td>Melis 2005</td>
<td>Some participants were living in a home for the aged.</td>
</tr>
<tr>
<td>Melis 2008</td>
<td>Some participants were living in a home for the aged.</td>
</tr>
<tr>
<td>Miller 1996</td>
<td>Visit directly related to hospital discharge. Not a randomised controlled trial.</td>
</tr>
<tr>
<td>Moore 1997</td>
<td>Intervention not a home visit.</td>
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<tr>
<td>Newcomer 2004</td>
<td>Intervention not a home visit: only 1% of visits were conducted in participant’s homes.</td>
</tr>
<tr>
<td>Oktay 1990</td>
<td>Not a randomised controlled trial: intervention and control groups recruited over different time period.</td>
</tr>
<tr>
<td>Parsons 2011</td>
<td>Ineligible comparison: control group received home visits.</td>
</tr>
<tr>
<td>Peeters 2007</td>
<td>Home visits not part of intervention.</td>
</tr>
<tr>
<td>Poulsen 2007</td>
<td>Intervention focused on providers.</td>
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<tr>
<td>Poulstrup 2000</td>
<td>Study not a randomised controlled trial: quasi-experimental design.</td>
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<tr>
<td>Robertson 2001b</td>
<td>Study not a randomised controlled trial.</td>
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<tr>
<td>Robichaud 2000</td>
<td>Study not a randomised controlled trial.</td>
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<tr>
<td>Rosie 2007</td>
<td>Intervention not a home visit.</td>
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<td>Salminen 2008</td>
<td>Intervention not a home visit.</td>
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<td>Salminen 2009a</td>
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<tr>
<td>Salminen 2009b</td>
<td>Intervention not a home visit.</td>
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<tr>
<td>Schraeder 2007</td>
<td>Not all participants required to have home visits as part of intervention.</td>
</tr>
<tr>
<td>Scogin 2007</td>
<td>Intervention was not a multidimensional geriatric assessment nor usual health visiting practice, but an advanced, home-delivered cognitive-behavioural therapy.</td>
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<tr>
<td>Bias</td>
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<tr>
<td>-----------------------------</td>
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<tr>
<td>Allocation concealment</td>
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<tr>
<td>Blinding of participants</td>
<td>Unclear risk</td>
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<tr>
<td>and personnel</td>
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<td>Blinding of outcome</td>
<td>Unclear risk</td>
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<td>bias)</td>
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<td>Incomplete outcome data</td>
<td>Low risk</td>
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<td>(attrition bias)</td>
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<tr>
<td>Selective reporting</td>
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<td>(reporting bias)</td>
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