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

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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.

1 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; Gustaffson, 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, & Knottnerus, 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 case 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).

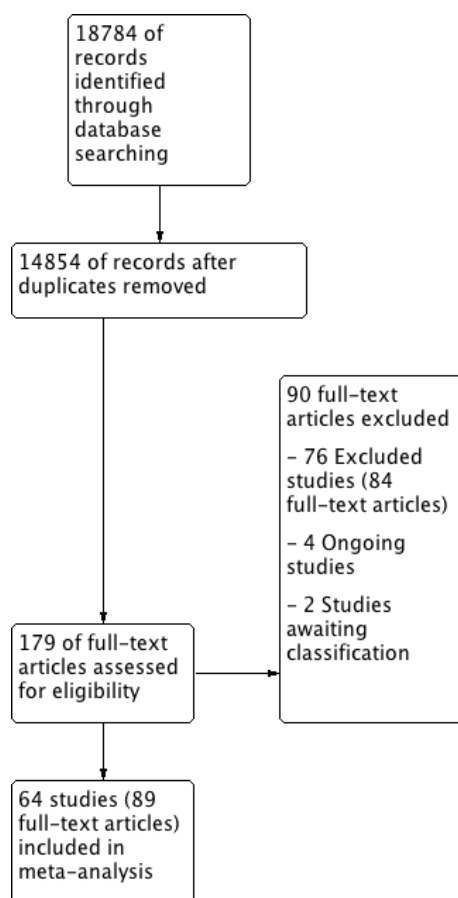
3 Results

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

Study ID	Country	No.	Mean age (yrs)	Focus	Visitor	Mean no of visits	Length (months)
Balaban 1988	USA	198	69	MGA	Combined	N/R	N/R
Bernabei 1998	IT	200	81	MGA	Other	6	12
Bouman 2008	NL	330	76	MGA	Nurse	6.9	18
Byles 2004	AU	1569	77	MGA	Combined	4.5	36
Campbell 1999	NZ	233	84	Falls	Other	4	12
Campbell 2005	NZ	194	84	Falls	Combined	6	12
Caplan 2004	AU	739	82	MGA	Nurse	2.29	1
Chandler 1998	USA	100	78	Falls	Other	30	3
Ciaschini 2009	CA	201	72	Both	Nurse	N/R	N/R
Ciechanowski 2004	USA	138	73	Neither	Other	6.6	4
Close 1999	UK	397	78	Both	Other	1	1
Counsell 2007	USA	951	72	MGA	Combined	13	24
Crawford-Shearer 2010	USA	59	78	Neither	Nurse	6	3
Dalby 2000	CA	142	79	MGA	Nurse	3	14
Davison 2005	UK	313	77	Both	Combined	N/R	N/R
Elley 2008	NZ	312	81	Falls	Nurse	N/R	12
Fabacher 1994	USA	254	73	Both	Combined	4	12
Gallagher 1996	CA	100	75	Falls	Nurse	3	6
Gitlin 2006	USA	319	79	Both	Combined	6	12
Green 2002	UK	170	73	Falls	Other	3	3
Gunner-Svensson 1984	DK	3743	N/R	MGA	Nurse	N/R	60
Gustafsson 2012	SE	288	86	Falls	Combined	1	1
Hall 1992	CA	167	78	MGA	Nurse	N/R	36
Hebert 2001	CA	503	80	MGA	Nurse	1	12
Hendriks 2008	NL	333	75	Both	Combined	1	1
Hogan 2001	CA	163	78	Falls	Combined	2	12
Holland 2005	UK	872	85	Neither	Other	2	6

Huang 2004	TW	120	72	Both	Other	2	4
Kingston 2001	UK	109	72	Falls	Other	1	12
Kono 2004	JP	119	83	MGA	Nurse	4.3	18
Kono 2011	JP	323	80	MGA	Combined	4	24
Krebs 1998	USA	120	74	Falls	Other	2	6
Lenaghan 2007	UK	134	84	Neither	Other	2	2
Lightbody 2002	UK	348	75	Both	Nurse	1	1
Lin 2007	TW	100	77	Falls	Other	8	4
Liu-Ambrose 2008	CA	59	82	Falls	Other	5	6
Luker 1981	UK	120	N/R	Neither	Other	4	4
Luukinen 2006	FI	486	88	Falls	Combined	3	3
Markle-Reid 2006	CA	288	84	MGA	Nurse	5	6
Markle-Reid 2010	CA	109	N/R	Both	Combined	6	6
McEwan 1990	UK	296	N/R	MGA	Nurse	1	20
Nelson 2004	USA	72	78	Neither	Other	11	6
Newbury 2001	AU	100	79	Both	Nurse	1	6
Pathy 1992	UK	725	73	MGA	Other	9	N/R
Pighills 2011	UK	165	79	Falls	Other	1	1
Ploeg 2010	CA	719	81	MGA	Nurse	3.03	12
Robertson 2001a	NZ	240	81	Falls	Nurse	5	6
Sahlen 2006	SE	594	79	Both	Combined	4	24
Shapiro 2002	USA	105	77	MGA	Nurse	N/R	18
Sommers 2000	USA	734	76	MGA	Combined	10+	18
Sorsensen 1988	DK	1555	80	MGA	Combined	1	12
Stevens 2001	AU	1879	77	Falls	Nurse	1	1
Stuck 1995	USA	414	81	MGA	Nurse	10.9	36
Stuck 2000	CH	791	82	MGA	Nurse	8	24
Thomas 2007	CA	520	81	MGA	Nurse	4	48
Tinetti 1994	USA	301	78	Both	Combined	7.8	3
van Haagstregt 2000	NL	316	77	Both	Nurse	5	12

van Hout 2010	NL	651	81	Both	Nurse	4	12
van Rossum 1993	NL	580	79	MGA	Nurse	12	36
Vetter 1984a	UK	554	78	MGA	Other	2	24
Vetter 1984b	UK	594	77	MGA	Other	2	24
Vetter 1992	UK	674	77	Both	Other	4	48
Wyman 2007	USA	272	79	Falls	Nurse	6	3
Yamada 2003	JP	368	79	Both	Nurse	5.1	18

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

Focus	Studies	Participants	Mean age (yrs)	Sex (% female)	Mean no visits	Duration (months)	Exercise component (%)
Falls	17	4990	79	71%	5.1	6.1	77%

MGA	25	16859	79	66%	5.1	23.2	4%
Both	16	5398	77	64%	3.7	11.4	38%
Neither	6	1395	80	76%	5.3	4.2	17%

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.

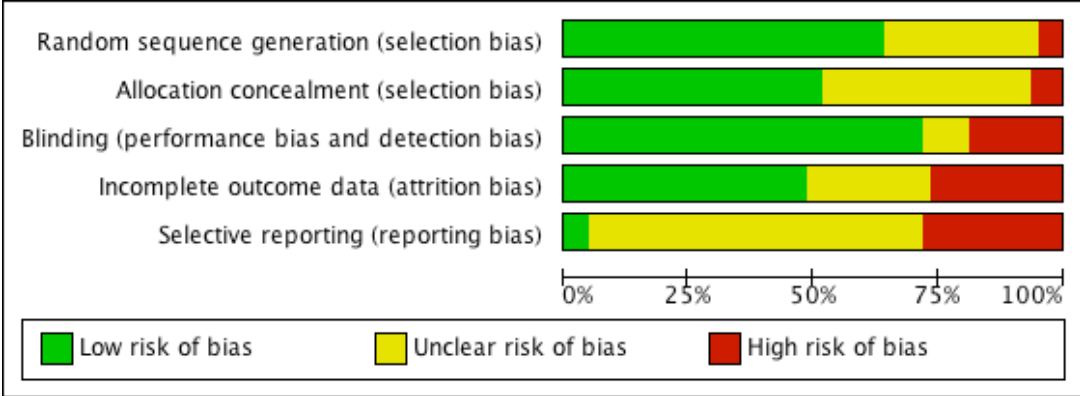


Figure 2: Risk of bias

There was high quality evidence of no clinically significant difference at longest follow-up (Risk ratio=0.93 [0.87, 0.99]; Chi²=54.89, df=53, p=0.40; I²=3%), and the absolute difference in mortality was close to zero (Risk difference=0.00 [-0.01, 0.00]; Chi²=64.72, df=55, p=0.17; I²=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)

Months	Included in Analysis		RR (95% CI), random effects	Heterogeneity I ² ; Chi ² (p value)
	Trials (%)	People (%)		
0-11	15 (23%)	4533 (16%)	0.85 (0.67 to 1.08)	0%; 6.54 (p=0.92)
12-23	32 (50%)	10759 (38%)	0.89 (0.78 to 1.02)	0%; 28.58 (p=0.54)

24-35	10 (16%)	5054 (18%)	0.82 (0.66 to 1.00)	32%; 13.29 (p=0.15)
36+	11 (17%)	10096 (35%)	0.93 (0.83 to 1.05)	39%; 18.13 (p=0.08)

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)

Subgroup	Included in Analysis		RR (95% CI), random effects	Heterogeneity I ² ; Chi ² (p value)
	Trials (%)	People (%)		
All studies	55 (86%)	24198 (84%)	0.93 (0.87 to 0.99)	3%; 54.89 (p=0.40)
Focus of Intervention (I ² =0%; Chi ² =2.72, p=0.44)				
Falls	12	2632	0.88 (0.66 to 1.18)	0%; 7.32 (p=0.60)
MGA	23	15011	0.95 (0.86 to 1.05)	27%; 31.42 (p=0.11)
Both	15	5278	0.82 (0.72 to 0.95)	0%; 9.12 (p=0.82)
Neither	5	1277	0.84 (0.61 to 1.16)	0%; 1.69 (p=0.79)
Average Age (I ² =30%; Chi ² =5.73, p=0.22)				
≤70	1	198	1.43 (0.88 to 2.33)	Not applicable
71-75	9	2914	0.97 (0.73 to 1.28)	0%; 4.99 (p=0.66)
76-80	26	9950	0.85 (0.76 to 0.94)	5%; 25.18 (p=0.40)
81-85	15	6214	0.95 (0.83 to 1.10)	0%; 12.73 (p=0.55)
86+	2	774	0.98 (0.69 to 1.39)	0%; 0.63 (p=0.43)
Type of Visitor (I ² =80%; Chi ² =9.94, p=0.007)				
Nurse	25	12132	1.00 (0.91 to 1.10)	2%; 22.43 (p=0.43)
Other	14	4937	0.78 (0.69 to 0.89)	0%; 6.52 (p=0.95)
Combined	16	7129	0.97 (0.84 to 1.11)	1%; 15.21 (p=0.44)
Number of visits (I ² =33%; Chi ² =2.98, p=0.23)				
1	7	2265	0.86 (0.57 to 1.29)	27%; 8.17 (p=0.23)
2-4	19	8038	0.84 (0.75 to 0.93)	0%; 12.37 (p=0.78)
5 or more	22	8856	0.96 (0.86 to 1.09)	0%; 20.30 (p=0.50)

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]; $\text{Chi}^2=37.64$, $\text{df}=26$, $p=0.07$; $I^2=31\%$). 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 ($\text{Chi}^2=2269.85$, $\text{df}=3$, $p < 0.00001$; $I^2=100\%$). 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]; $\text{Chi}^2=13.70$, $\text{df}=14$, $p=0.47$; $I^2=0\%$). 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 ($\text{Chi}^2=933.29$, $\text{df}=12$, $p < 0.00001$; $I^2=99\%$). 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]; $\text{Chi}^2=28.07$, $\text{df}=11$, $p=0.003$; $I^2=61\%$). 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]; $\text{Chi}^2=16.29$, $\text{df}=11$, $p=0.13$; $I^2=32\%$). 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]; $\text{Chi}^2=35.81$, $\text{df}=9$, $p<0.0001$; $I^2=75\%$). 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]; $\text{Chi}^2=43.59$, $\text{df}=22$, $p=0.004$; $I^2=50\%$). 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 ($\text{Chi}^2=4574.87$, $\text{df}=14$, $p<0.00001$; $I^2=100\%$). 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]; $\text{Chi}^2=18.26$, $\text{df}=13$, $p=0.15$; $I^2=29\%$). 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)

Months	Included in Analysis		OR (95% CI), random effects	Heterogeneity I^2 ; Chi^2 (p value)
	Trials (%)	People (%)		
0-11	7 (11%)	1433 (5%)	1.06 (0.73 to 1.54)	37%; 9.58 (p=0.14)
12-23	16 (25%)	5366 (19%)	0.77 (0.66 to 0.91)	40%; 24.98 (p=0.05)
24-35	1 (2%)	771 (3%)	1.11 (0.79 to 1.57)	Not applicable

36+	1 (2%)	450 (2%)	1.46 (0.99 to 2.16)	Not applicable
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Table 6: Subgroup analyses for people who fell (Odds ratio)

Subgroup	Included in Analysis		OR (95% CI), random effects	Heterogeneity I ² ; Chi ² (p value)
	Trials (%)	People (%)		
All studies	23 (36%)	7455 (26%)	0.86 (0.73 to 1.01)	50%; 43.59 (p=0.004)
Focus of Intervention (I ² =0%; Chi ² =1.89, p=0.39)				
Falls	11 (17%)	3848 (13%)	0.87 (0.73 to 1.02)	12%; 11.42 (p=0.33)
MGA	1 (2%)	771 (3%)	1.11 (0.79 to 1.57)	Not applicable
Both	11 (17%)	2836 (10%)	0.84 (0.61 to 1.16)	67%; 30.26 (p=0.0008)
Neither	0 (0%)	0(0%)	Not applicable	Not applicable
Average Age (I ² =0%; Chi ² =1.36, p=0.71)				
≤70	0(0%)	0 (0%)	Not applicable	Not applicable
71-75	6 (9%)	1242(4%)	1.01 (0.67 to 1.54)	40%; 8.34 (p=0.14)
76-80	12 (19%)	5064 (18%)	0.85 (0.69 to 1.06)	58%; 26.09 (p=0.006)
81-85	4 (6%)	791 (3%)	0.76 (0.48 to 1.22)	58%; 7.16 (p=0.07)
86+	1 (2%)	358 (1%)	0.72 (0.45 to 1.15)	Not applicable
Type of Visitor (I ² =49%; Chi ² =3.94, p=0.14)				
Nurse	7 (11%)	3293 (11%)	1.02 (0.85 to 1.24)	11%; 6.76 (p=0.34)
Other	8 (13%)	1650 (6%)	0.77 (0.49 to 1.21)	72%; 24.68 (p=0.0009)
Combined	8 (13%)	2512 (9%)	0.79 (0.65 to 0.96)	14%; 8.17 (p=0.32)
Number of visits (I ² =0%; Chi ² =0.46, p=0.79)				
1	7 (11%)	3873 (14%)	0.82 (0.62 to 1.08)	58%; 14.44 (p=0.03)
2-4	7 (11%)	1662 (6%)	0.87 (0.61 to 1.24)	56%; 13.56 (p=0.03)
5 or more	6 (9%)	1114 (4%)	0.74 (0.54 to 1.01)	33%; 7.49 (p=0.19)

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]; $\text{Chi}^2=7.24$, $\text{df}=9$, $p=0.61$; $I^2=0\%$). 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]; $\text{Chi}^2=4.32$, $\text{df}=6$, $p=0.63$; $I^2=0\%$). 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]; $\text{Chi}^2=55.40$, $\text{df}=26$, $p=0.0007$; $I^2=53\%$). 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]; $\text{Chi}^2=12.49$, $\text{df}=7$, $p=0.09$; $I^2=44\%$). 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], $\text{Chi}^2=35.69$, $\text{df}=28$, $p=0.15$; $I^2=22\%$). 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]; $\text{Chi}^2=18.06$, $\text{df}=14$, $p=0.20$; $I^2=22\%$). 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						
Patient or population: patients with prevention of impairment and death in older adults						
Settings:						
Intervention: Home Visits versus Controls						
Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of Participants (studies)	Quality of the evidence (GRADE)	Comments
	Control	Home Visits versus Controls				
Mortality	Study population		RR 0.93 (0.87 to 0.99)	24198 (55 studies)	⊕⊕⊕⊕ high	
	139 per 1000	130 per 1000 (121 to 138)				
	Low					
	50 per 1000	47 per 1000				

	1000	(44 to 50)			
	High				
	200 per 1000	186 per 1000			
		(174 to 198)			
Institutionalisation People	Study population		RR 1.02	16459	⊕⊕⊕ ⊖
	85 per 1000	87 per 1000	(0.88 to 1.18)	(27 studies)	moderate ^{1,2}
		(75 to 100)			
	Low				
	40 per 1000	41 per 1000			
		(35 to 47)			
	High				
	120 per 1000	122 per 1000			
		(106 to 142)			
Falls People	Low		OR 0.86	7455	⊕⊕⊕ ⊖
	200 per 1000	177 per 1000	(0.73 to 1.01)	(23 studies)	moderate ³
		(154 to 202)			
	High				
	600 per 1000	563 per 1000			
		(523 to 602)			
Hospitalisation People	Study population		RR 0.96	6288	⊕⊕⊕ ⊖
	410 per 1000	394 per 1000	(0.91 to 1.01)	(15 studies)	moderate ⁴
		(373 to 414)			
	Low				
	200 per 1000	192 per 1000			
		(182 to 202)			
	High				
	600 per 1000	576 per 1000			
		(546 to 606)			
Functioning ADL/IADL		The mean functioning adl/iadl in the intervention groups was 0.10 standard deviations lower (0.17 to 0.03 lower)		8769 (27 studies)	⊕⊖⊖⊖ very low ^{5,6,7}
Health Related QoL		The mean health related qol in the intervention groups was 0.06 standard deviations lower (0.11 to 0.01 lower)		9892 (29 studies)	⊕⊕ ⊖⊖ low ^{5,8}

*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

effects) were not undertaken because the results were judged not to change these conclusions based on the GRADE approach. However, we could not reliably describe all of the comparison conditions, which may have included effective interventions in some studies, so conclusions must be interpreted with some caution.

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 of 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).

6 Differences between protocol and review

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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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/

- 26 clinical trials/
- 27 exp treatment outcomes/
- 28 treatment effectiveness evaluation/
- 29 empirical study.md.
- 30 experimental replication.md.
- 31 followup study.md.
- 32 longitudinal study.md.
- 33 meta analysis.md.
- 34 prospective study.md.
- 35 retrospective study.md.
- 36 treatment outcome clinical trial.md.
- 37 placebo\$.tw.
- 38 randomi?ed controlled trial\$.tw.
- 39 rct.tw.
- 40 random allocation.tw.
- 41 (randomly adj1 allocated).tw.
- 42 (allocated adj2 random).tw.
- 43 ((singl\$ or doubl\$ or treb\$ or tripl\$) adj (blind\$3 or mask\$3)).tw.
- 44 (clinic\$ adj (trial? or stud\$3)).tw.
- 45 or/11-44
- 46 comment reply.dt.
- 47 editorial.dt.
- 48 letter.dt.
- 49 clinical case study.md.
- 50 nonclinical case study.md.
- 51 animal.po.
- 52 human.po.
- 53 51 not (51 and 52)
- 54 or/46-50,53
- 55 45 not 54
- 56 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

- #8 ((elderly or aged or old*) near/2 (person* or people* or man or men or woman or women or patient*)) or geriatric* or "senior citizen*":ti,ab,kw
- #9 (#7 OR #8)
- #10 (#6 AND #9)
- #11 from 2006 to 2008
- #12 (#10 AND #11)

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

Place of recruitment:	
Inclusion criteria:	
Number randomised:	
Mean age:	
Sex (% female):	
Mortality per year in the comparison group:	
Living alone (%):	
Visitors' professional group:	
Frequency and duration of visits:	
Fall prevention:	
Included exercise:	
Included multidimensional geriatric assessment:	
Description of intervention:	
Description of comparison:	
Implementation (fidelity and participant compliance):	
Outcome measures:	
Location:	
Funding Source:	
Other notes:	

Appendix 4: Table of Excluded Studies

Study ID	Reason for exclusion
Archbold 1995	Intervention for families, not for the elderly themselves.
Carpenter 1990	Visits not conducted by health professional.
Clarke 1992	Visits not conducted by health professional.
Clemson 2004	Intervention not a home visit: intervention conducted in community venue with follow-up home visit.
Comans 2010	Ineligible comparison: both intervention and control groups received home visits.
Dapp 2011	Only 8.8% of intervention participants opted for home visits.
Day 2002	Intervention not a home visit. All arms of intervention (including controls) were assessed at home by nurses.
de Vries 2010	Home visits not part of intervention.
Dunn 1994	Visit directly related to hospital discharge.
Engelhardt 1996	Intervention not a home visit: intervention carried out in out-patient clinic.
Epstein 1990	Intervention not a home visit: assessment conducted in hospital.
Ettinger 1997	Intervention not a home visit.
Fairhall 2008	Visit directly related to hospital discharge.
Fletcher 2004	Intervention not a home visit: less than half the assessments were conducted in participants' homes.
Fordyce 1997	Intervention not a home visit: health appraisal took place during office visits.
Fox 2010	Only 50.0% of intervention participants opted for home visits.
German 1995	Intervention not a home visit: preventive visits took place at physician's office.
Gill 2002	Ineligible comparison: both intervention and control groups received home visits.
Graham 2006	Home visits not carried out by health professionals. Quasi experimental design.
Hansen 1992	Visit directly related to hospital discharge.
Hay 1998	Intervention not a home visit: screening took place in primary care practice.
Hendriksen 1984	Not a randomised controlled trial: study refusers did not have equal chance of being assigned to either group. Informed consent followed randomisation.
Hornbrook 1994	Visits not conducted by health professional
June 2009	Not a randomised controlled trial
Karatay 2011	Visits not conducted by health professional – students.
Kerse 1999	Intervention targeted general practitioners
Kronborg 2006	Intervention targeted visitors
Legault 2011	Intervention not a home visit: centre-based intervention.

Leveille 1998	Intervention not a home visit: intervention carried out at senior centre.
Mahoney 2007	Ineligible comparison: both intervention and control groups received home visits.
Mann 1999	Visits directly related to rehabilitation and delivered to non-independent elderly.
Matzen 2007a	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.
Matzen 2007b	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.
McMurdo 1996	Ineligible comparison: control group received home visits.
Melin 1992	Visit directly related to hospital discharge.
Melis 2005	Some participants were living in a home for the aged.
Melis 2008	Some participants were living in a home for the aged.
Miller 1996	Visit directly related to hospital discharge. Not a randomised controlled trial.
Moore 1997	Intervention not a home visit.
Newcomer 2004	Intervention not a home visit: only 1% of visits were conducted in participant's homes.
Oktay 1990	Not a randomised controlled trial: intervention and control groups recruited over different time period.
Parsons 2011	Ineligible comparison: control group received home visits.
Peeters 2007	Home visits not part of intervention.
Poulsen 2007	Intervention focused on providers.
Poulstrup 2000	Study not a randomised controlled trial: quasi-experimental design.
Robertson 2001b	Study not a randomised controlled trial.
Robichaud 2000	Study not a randomised controlled trial.
Rosie 2007	Intervention not a home visit.
Salminen 2008	Intervention not a home visit.
Salminen 2009a	Intervention not a home visit.
Salminen 2009b	Intervention not a home visit.
Schraeder 2007	Not all participants required to have home visits as part of intervention.
Scogin 2007	Intervention was not a multidimensional geriatric assessment nor usual health visiting practice, but an advanced, home-delivered cognitive-behavioural therapy.

Silverman 1995	Intervention not a home visit: geriatric assessment took place in outpatient clinic.
Sjosten 2007a	Intervention not a home visit.
Sjosten 2007b	Intervention not a home visit.
Sjosten 2008	Intervention not a home visit.
Spice 2009	Intervention not preventative and not a home visit.
Steinberg 2000	Visits not conducted by health professional - student OTs.
Theander 2005	Study not a randomised controlled trial: no control group.
Toseland 1996	Intervention not a home visit.
Townsend 1988	Visit directly related to hospital discharge.
Tulloch 1979	Intervention not a home visit.
Vaapio 2007	Intervention not a home visit.
Vass 2005	Intervention targeted visitors.
von Renteln 2003	Study not a randomised controlled trial.
Wagner 1994	Intervention not a home visit.
Wallace 1998	Intervention not a home visit.
Wasson 1999	Intervention not a home visit.
Whitehead 2003	Visit directly related to hospital discharge.
Williams 1992	Directly related to hospital discharge.
Williams 2002	Participants resided in retirement communities.
Wolf 2001	Intervention not a home visit.
Yates 2001	Visit not conducted by a health professional - masters students.
Zimmer 1985	Intervention not a preventive home visit: visits to homebound, chronically or terminally ill patients.

Bias	Authors' judgement	Support for judgement
Allocation concealment (selection bias)	Unclear risk	Attempt was made to equate the kinship care and foster care groups through controlling for covariates; No evidence reported on the comparability of the groups
Blinding of participants and personnel (performance bias)	Unclear risk	Unclear if kinship care and foster care groups experienced different exposure to the intervention; Unclear if groups received different services during placement
Blinding of outcome assessment (detection bias)	Unclear risk	Unclear if kinship care and foster care groups were defined in same way; There was no evidence of biased assessment resulting from the type of placement
Incomplete outcome data (attrition bias)	Low risk	All participants were accounted for in the reporting of results; Attrition could not have influenced the results
Selective reporting (reporting bias)	Low risk	Instrumentation used to measure the outcomes was specified completely; Reliability and/or validity information was reported for instrumentation