Complex interventions to improve physical function and maintain independent living in elderly people: a systematic review and meta-analysis

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Summary

Background In old age, reduction in physical function leads to loss of independence, the need for hospital and long-term nursing-home care, and premature death. We did a systematic review to assess the effectiveness of community-based complex interventions in preservation of physical function and independence in elderly people.

Methods We searched systematically for randomised controlled trials assessing community-based multifactorial interventions in elderly people (mean age at least 65 years) living at home with at least 6 months of follow-up. Outcomes studied were living at home, death, nursing-home and hospital admissions, falls, and physical function. We did a meta-analysis of the extracted data.

Findings We identified 89 trials including 97,984 people. Interventions reduced the risk of not living at home (relative risk [RR] 0·95, 95% CI 0·93–0·97), but not death (1·00, 0·97–1·02). Risk of hospital admissions (0·94, 0·91–0·97) and falls (0·90, 0·86–0·95) were reduced, and physical function (standardised mean difference −0·08, −0·11 to −0·06) was better in the intervention groups than in other groups. Benefit for any specific type or intensity of intervention was not noted. In populations with increased death rates, interventions were associated with reduced nursing-home admission. Benefit in trials was particularly evident in studies started before 1993.

Interpretation Complex interventions can help elderly people to live safely and independently, and could be tailored to meet individuals’ needs and preferences.

Introduction

In old age, reduction in physical function can lead to loss of independence, the need for hospital and long-term nursing-home care, and premature death. The importance of physical, functional, psychological, and social factors in realising a healthy old age is recognised by elderly people,1,2 health-care professionals,3 and policy makers.4

The risk factors for reduced physical function in elderly people, as identified in longitudinal studies,5–14 relate to comorbidities, physical and psychosocial health, environmental conditions, social circumstances, nutrition, and lifestyle. The need for a preventive strategy based around identification and treatment of diverse risk factors was identified more than 40 years ago,1 and many trials of complex intervention packages have been reported and reviewed. In this context, a complex intervention can be regarded as a combination of interdisciplinary teamwork for health and social problems. Trials have focused on general and frail elderly populations,15,16 elderly people discharged from hospital,17 and those at risk of falling.18–20 However, the development of risk factors, admission to hospital, and risk of falling represent a common chain of experiences for many elderly people.21 Likewise, multifactorial interventions in these populations have common characteristics and, in addition to targeting specific outcomes relating to hospital readmissions and falls, share the common aims of physical function maintenance, disability limitation, and promotion of independence.

In the UK, yearly multidimensional assessments of physical and cognitive health for all individuals aged at least 75 years became a necessity in primary care in 1989,22 with guidelines on content and implementation provided for England.23 In due course, a targeted approach to assessment and care was developed and promoted with community nurse-led case management of elderly people with medical conditions identified from hospital admissions and general practice records.24 The report24 stresses the importance of a team-based approach incorporating appropriate skills to meet the health and care needs of elderly people.

Geriatric screening and multidimensional assessment are recognised in modernised health-care systems in Germany, Italy, France, the Netherlands, and Denmark.25 In US-managed care organisations, the focus of care is on frail elderly people and those discharged from hospital.26 Care is coordinated by case managers and this model has been applied in other countries, including England.27

The systematic reviews cited above examined the effectiveness of interventions in specific groups of elderly people or clinical settings. To guide new
preventive and anticipatory care efforts, we intended to answer the question of the effectiveness of all community-based complex interventions used to preserve physical function and independence in elderly people. We did a systematic review of randomised controlled trials with outcomes of independent living, hospital and nursing home admissions, physical function, and falls.

Methods
Search strategy and selection criteria
We used Cochrane systematic review methods to identify randomised controlled trials that met our inclusion criteria. We included trials that compared community-based multifactorial intervention with usual care or minimum intervention, with follow-up for at least 6 months. Interventions were eligible for the review if individuals received personalised assessment and provision of or referral for appropriate specialist medical and social care. Mean age of eligible study populations was at least 65 years at baseline, with individuals living at home or preparing for hospital discharge to home.

The search strategy covered issues related to: randomised controlled trials; elderly people; community and home setting; health, social, behavioural, and occupational therapy interventions; and hospital and nursing home admissions, physical function, and disability. Searches were tailored to individual computerised databases; Medline strategy is shown in the webappendix.

Searches were made in CENTRAL (issue 4, 2004) and updated to January, 2005, with searches of Medline and Embase from 2003 to January, 2005. Further searches were done of CINAHL from 1982 to January, 2005, PsycINFO from 1972 to January, 2005, and ISI Science and Social Science Citation Index from 1945 to January, 2005. Reference lists of trials and previous reviews were searched and follow-up reports of previously unfinished trials were sought. Additional trials reported after 2004 and before December, 2006, were identified by the Web of Science citation search facility with focus on previous reviews and key trials.

One reviewer (KR) scanned abstracts and titles. Potentially relevant articles were acquired and data were extracted in duplicate from most (64%) reports and recorded on a piloted form and Excel spreadsheet. All outcome data were further checked with original articles. Information was extracted on study characteristics (randomisation procedure, blind assessment at baseline and follow-up, follow-up period, intention-to-treat analysis, and losses to follow-up), participants (inclusion criteria, numbers of individuals in randomised groups, age of participants, baseline comparisons, and country and date of recruitment), intervention (aims, content, carer involvement, contributors, format, duration, and intensity), and outcomes. Disagreements in extracted data were resolved by discussion among reviewers. We did not exclude trials from the review once they had been included. Trials that were not intention to treat were not included in meta-analyses.

The outcomes studied were living at home at follow-up, death, nursing-home and hospital admissions, falls, and physical function.

Potential sources of heterogeneity that were investigated were context of intervention (geriatric assessment in general or frail elderly populations, community-based care after hospital discharge, fall prevention, or group education and counselling); quality of studies (losses to follow-up); mortality rate in study population; date recruitment commenced; mean age of participants; intensity of intervention; and extent of control group intervention activity. Frail populations typically included people with limitations in activities of daily living and chronic conditions, and those thought to be at risk of functional deterioration or hospital admission.

The intensity of interventions was calculated by addition of three measures of intervention intensity: multidisciplinary input (one discipline=1, two disciplines or two or more similar disciplines=2, and three or more different disciplines=3), number of scheduled visits (one to four=1, five to nine=2, ten or more=3), and the duration of the intervention (0–1 month=1, 2–6 months=2, more than 6 months=3). To create groups with similar numbers, scores of 1–4 were regarded as low, 5–6 as medium, and 7–9 as high intensity. Analyses were also done for every feature of intensity separately.

Statistical analysis
Meta-analyses were done with Review Manager and additional statistics with SPSS (version 12.0.1).
of articles published after 1991 were contacted for information that was not available in the published material.

We chose to use fixed-effects meta-analysis a priori because the complex interventions for elderly people we have defined had common characteristics and aims. For dichotomous outcomes, relative risks (RRs) were summarised with Mantel-Haenszel fixed-effects meta-analyses. 24 However, for results showing significant heterogeneity (I²>50%), random-effects meta-analysis was also done with the method of DerSimonian and Laird. 24 Meta-regression was done with Stata (version 10.0). By convention not living at home was used instead of living at home. For physical function, data were summarised as the standardised mean difference (SMD). Only intention-to-treat analyses were included, which for physical function mainly represented available case analyses. Results were summarised descriptively for those studies with insufficient data.

If not living at home was unavailable, the sum of deaths and nursing home admissions was used, which led to a potential overestimation by double counting of people admitted to a nursing home and who subsequently died. Analyses were done with or without estimates. For nursing-home admissions as an outcome, some trials reported permanent admission whereas others reported individuals living in a nursing home at follow-up. Results were analysed separately and combined.

Several measures of physical function were reported and we classified these as pertaining to severity of disability such as limitations in activities of daily living or generic physical function. Differences in activities of daily living and generic physical function at follow-up were analysed separately and combined. If SDs were unavailable, values were calculated as described in the Cochrane handbook for systematic reviews of interventions. 25 Otherwise, baseline values were used, either those from a trial in a similar population or from appropriate population statistics. For all outcomes, scales were recoded such that high values indicated poor physical function. Funnel plots were inspected at all stages of the review to identify possible publication bias.

Role of the funding source

The sponsor of the study had no role in study design, data collection, data analysis, data interpretation, or writing of the report. The corresponding author had full access to all the data in the study and had final responsibility for the decision to submit for publication.

Results

The review process is summarised in figure 1 according to QUOROM guidelines. 26 89 intervention trials meeting our inclusion criteria are summarised in webtable 1 with full details available from the authors. Trials assessed geriatric assessment in elderly people representing the general population (n=28), 27–50 community-based care after hospital discharge (n=21), 51,99–110 fall prevention (n=13), 111–116 group education and counselling (n=3), 117–119 All trial interventions were complex and many individuals would have been eligible for any of them.

Randomisation was by individual or household (n=80) or by clusters of general practices, community groups, or municipalities (n=9). The total number of people randomised was 97 984 with a median of 321 (range 54–43 219) in trials. One large study randomised 43 219 people. 21 Mortality rates ranged from 0 to 60.8% per year, with a median of 6.8%. Trials assessing geriatric assessment in general and frail populations

| Study context | Not living at home N=79 578 | Death N=93 754 | Nursing home admission N=79 575 | Hospital admission N=20 047 | People with falls N=15 607 | Physical function N=21 651 |
|---------------|------------------|---------|------------------|------------------|------------------|------------------|
| Geriatric assessment of general elderly people | 0.95 (0.93 to 0.98) | 1.00 (0.98 to 1.02) | 0.86 (0.83 to 0.90) | 0.98 (0.92 to 1.03) | 0.76 (0.67 to 0.86) | -0.12 (–0.16 to –0.08) |
| I² | 35.2% | 39.7% | 47.5% | 61.4% | 0 | 0 |
| Geriatric assessment of elderly people selected as frail | 1.00 (0.87 to 1.15) | 1.03 (0.89 to 1.19) | 1.01 (0.83 to 1.23) | 0.90 (0.84 to 0.98) | 0.99 (0.89 to 1.10) | -0.01 (–0.06 to 0.04) |
| I² | 43.3% | 0 | 28.8% | 11.0% | 0 | 57.9% |
| Community-based care after hospital discharge | 0.90 (0.82 to 0.99) | 0.92 (0.89 to 1.05) | 0.77 (0.64 to 0.91) | 0.95 (0.90 to 0.99) | 0.82 (0.61 to 1.08) | -0.05 (–0.15 to 0.04) |
| I² | 2.2% | 0 | 57.0% | 40.3% | 0 | 0 |
| Fall prevention | 0.86 (0.63 to 1.19) | 0.79 (0.66 to 0.98) | 1.26 (0.70 to 2.27) | 0.84 (0.61 to 1.16) | 0.92 (0.87 to 0.97) | -0.25 (–0.36 to –0.13) |
| Group education and counselling | 0.62 (0.43 to 0.88) | 0.80 (0.42 to 1.55) | 0.50 (0.05 to 5.49) | 0.75 (0.51 to 1.09) | n/a | 0.05 (–0.20 to 0.30) |
| I² | 0 | 0 | 0 | 65.8% | 0 | 4.1% |
| All complex interventions | 0.95 (0.93 to 0.97) | 1.00 (0.97 to 1.02) | 0.87 (0.83 to 0.90) | 0.94 (0.91 to 0.97) | 0.90 (0.86 to 0.95) | -0.08 (–0.11 to –0.06) |
| I² | 29.3% | 10.6% | 29.0% | 43.0% | 52.8% | 45.9% |

n/a=not applicable. *Activities of daily living −0.08 (−0.11 to −0.04, I²=37.5%) and generic physical function −0.09 (–0.13 to –0.05, I²=64.0%).
| Study                          | Intervention n/N | Control n/N | RR (fixed) 95% CI | RR (fixed) 95% CI |
|-------------------------------|------------------|-------------|-------------------|-------------------|
| **Geriatric assessment**      |                  |             |                   |                   |
| General elderly people        |                  |             |                   |                   |
| Gurren-Svensson 1984*         | 50/2075          | 63/2073     | 0.96 (0.88-1.06)  |                   |
| Tuulio 1979*                  | 33/46            | 31/50       | 1.30 (0.71-2.30)  |                   |
| Hendriksen 1984*              | 77/285           | 97/285      | 0.85 (0.66-1.08)  |                   |
| Vetter et al. 1984*           | 57/281           | 73/278      | 0.79 (0.69-0.91)  |                   |
| Vetter et al. 1984*           | 49/294           | 57/298      | 0.74 (0.58-0.94)  |                   |
| Carpenter 1990*               | 66/272           | 57/267      | 1.04 (0.83-1.31)  |                   |
| Carlé 1990*                   | 68/161           | 71/162      | 0.96 (0.72-1.28)  |                   |
| Pathy 1990*                   | 87/169           | 114/156     | 0.74 (0.54-0.99)  |                   |
| Falbacher 1994*               | 4/131            | 4/132       | 0.94 (0.24-3.60)  |                   |
| Stock 1995*                   | 30/235           | 33/239      | 0.84 (0.53-1.33)  |                   |
| van Rossum 1993*              | 49/292           | 53/288      | 0.88 (0.62-1.22)  |                   |
| Burton 1994*                  | 442/205          | 514/2090    | 0.95 (0.75-1.20)  |                   |
| Wanner 1994*                  | 41/137           | 23/130      | 1.07 (0.83-1.35)  |                   |
| Verk 1995*                    | 42/234           | 52/237      | 1.00 (0.76-1.30)  |                   |
| Retcher 2004*                 | 7720/1762        | 7935/1457   | 0.96 (0.93-0.98)  |                   |
| Kene 1995*                    | 41/75            | 41/76       | 0.24 (0.03-1.26)  |                   |
| Wallace 1998*                 | 0/5              | 0/5         | Not estimated     |                   |
| Byles 2004*                   | 145/942          | 69/627      | 1.40 (1.07-1.81)  |                   |
| Birkhoff 2000*                | 40/738           | 34/738      | 1.00 (0.74-1.32)  |                   |
| Newby 2001*                   | 3/50             | 7/50        | 0.43 (0.12-1.58)  |                   |
| Vass 2004*                    | 394/2092         | 254/1942    | 1.07 (0.92-1.26)  |                   |
| **Subtotal (95% CI)**         | 33054            | 32793       | 0.95 (0.93-0.98)  |                   |
| **Total events**              | 98644            | 98293       | 0.95 (0.93-0.98)  |                   |
| **Test for overall effect**   | Z = 2.23 (p < 0.05) |            |                   |                   |
| **Fall prevention**           |                  |             |                   |                   |
| Wagner 1994*                  | 16/85            | 23/87       | 0.75 (0.41-1.37)  |                   |
| Galagher 1996*                | 0/50             | 0/50        | Not estimated     |                   |
| Close 1999*                   | 37/154           | 45/153      | 0.85 (0.65-1.10)  |                   |
| Hogan 2001*                   | 4/79             | 6/84        | 0.73 (0.21-2.42)  |                   |
| **Subtotal (95% CI)**         | 948              | 954         | 0.96 (0.62-1.49)  |                   |
| **Total events**              | 3519             | 3623        | 0.95 (0.62-1.49)  |                   |
| **Test for overall effect**   | Z = 1.73 (p = 0.03) |            |                   |                   |
| **Group education and counselling** |              |             |                   |                   |
| Back 1997*                    | 7/60             | 11/61       | 0.55 (0.21-1.45)  |                   |
| Scott 2008*                   | 37/145           | 57/149      | 0.63 (0.43-0.91)  |                   |
| **Subtotal (95% CI)**         | 395              | 310         | 0.62 (0.43-0.89)  |                   |
| **Total events**              | 10240            | 10410       | 0.95 (0.83-1.10)  |                   |
| **Test for overall effect**   | Z = 3.36 (p < 0.01) |            |                   |                   |

* Studies were included in the meta-analysis.
had median mortality rates of 5.4% (0–10.5%) and 6.1% (1.1–60.8%), respectively, suggesting that the frail category was often subject to selection, probably indicating eligibility issues. In trials of community-based care after hospital discharge, the median mortality rate was 16.2% (6.3–53.0%); for fall prevention and group education, it was 4.3% (0–11.6%) and 3.4% (2.7–4.4%), respectively.

Losses to follow-up were used as a marker of study quality. In trials with death as an outcome, 40 (48%) of 84 had losses to follow-up of 1% or less (range 0–27.6%). For physical function, few trials included people who had died or moved to nursing homes in their analyses; exceptions were Close and Gagnon and their colleagues’ trials. 15 (35%) of 43 trials had losses of participants to follow-up for interview of 5% or less (0–33%).

The allocation process was described in 61 (69%) of 89 trials, but difficulties of assessing concealment and masking in complex intervention trials are unlikely to have been fully addressed. Intervention activity in control groups was evident in 40 (45%) of 89 trials.

Data for variability between clusters were insufficient and the effect of analysis errors arising from inclusion of cluster randomised trials was explored by sensitivity analysis. Inspection of funnel plots at all stages of the review gave no indication of selection bias in studies included in the analysis (data not shown).

Outcomes are summarised by type of intervention in the table. The outcome of living at home at follow-up was available for 51 interventions; in a further nine trials death and nursing-home admission were used, with the consequent inclusion of the large Medical Research Council (MRC) trial. Overall, 60 (67%) of 89 trials reported living at home at follow-up or an estimate. However, this outcome was reported in only 4 (31%) of 13 trials.

In a meta-analysis of 60 trials with 79,578 individuals (figure 2), the overall risk of not living at home was lower in the intervention group (RR 0.95, 95% CI 0.93–0.97) than in the control group. Geriatric assessment of general elderly people and community-based care after hospital discharge were the only types of intervention that had a significant effect on the risk of not living at home (figure 2). Removal of trials with estimated values had little effect (0.95, 0.90–1.00). Heterogeneity was only manifest in trials of geriatric assessment in general populations and those selected as frail (figure 2).

If typical rates of not living at home of about 7–6% (median in trials; range 0–12.1%) per year for the general population are used, and the reduction in risk from intervention is 5%, a number needed to treat of 263 is obtained. For the increased rates of not living at home in people receiving community-based care after hospital discharge of about 25% per year with an RR reduction of 9%, the number needed to treat is 40.

Data for death were available for 84 (94%) of 89 interventions including 93,754 people (webfigure 1). Interventions had no overall benefit (RR 1.00, 95% CI 0.97–1.02) and the only appreciable benefit by type of intervention was noted in 11 trials targeting fall prevention (0.79, 0.66–0.96; webfigure 1). Slight heterogeneity ($I^2=10.6\%$) was almost exclusively limited to trials of geriatric assessment in general elderly populations ($I^2=39.7\%$; webfigure 1).

Data for nursing-home admission (31 trials) or for residence at follow-up (23 trials) were available for 79,575 people (webfigure 2) and were widely reported in trials of geriatric assessment in general (20 [71%] of 28) or populations selected as frail (16 [67%] of 24) and community-based care after hospital discharge (14 [67%] of 21), but not in trials of fall prevention (3 [23%] of 13).

For combined nursing-home outcomes, risk of admission was reduced in the intervention group (RR 0.87, 95% CI 0.83–0.90; webfigure 2). Only a marginal effect was seen for residence at follow-up (0.93, 0.79–1.09). Geriatric assessment and community-based care after hospital discharge were the only types of intervention to have a significant effect on the combined outcome (webfigure 2). Some heterogeneity was recorded in trials ($I^2=29.0\%$), mainly in geriatric assessment in general populations ($I^2=47.5\%$; webfigure 2).

For nursing-home care, typical median rates for trial populations were 2.2% (range 0.1–5.4) per year for the general population and 11.1% (2.1–40.2) per year for people receiving community-based care after hospital discharge, generating numbers needed to treat of 354 and 39, respectively.

Hospital admissions were reported in between 5 (38%) of 13 (falls prevention) and 18 (86%) of 21 (community-based care after hospital discharge) trials. The most commonly reported outcome—number of people having an admission (41 trials with 20,047 people [webfigure 3])—was used in the meta-analysis. Risk of hospital admission was reduced by interventions (RR 0.94, 95% CI 0.91–0.97; webfigure 3). Geriatric assessment in elderly people selected as frail and community-based care after hospital discharge were the only types of interventions to show significant effect on this outcome. Heterogeneity ($I^2=43.0\%$) was largely restricted to geriatric assessment in general elderly patients and community-based care after hospital discharge (61.4% and 57.0%, respectively; webfigure 3). In random-effects meta-analysis, the overall RR was similar (0.94, 95% CI 0.89–0.99).

Trials with data that were incompatible with the meta-analysis were inconsistent, with hospital admissions reduced in seven, similar in four, and increased in five trials. The large MRC trial reported slightly reduced total admissions in the intervention group (RR 0.96, 99% CI 0.79–1.16). All 13 studies targeting fall prevention reported individuals who had fallen, whereas falls were less
likely to be reported in trials in general elderly people (6 [21%] of 28 trials), frail elderly people (5 [21%] of 24),
and those with community-based care after hospital
discharge (5 [24%] of 21). No trials of group education
reported falls. An overall benefit was noted in 25 trials
including 15,607 people (RR 0·90, 95% CI 0·86–0·95;
webfigure 4). Interventions targeting fall prevention
contributed 66% of the weight. Only trials of geriatric
assessment in general elderly populations and those of
fall prevention showed significantly reduced falls
(webfigure 4). Heterogeneity ($I^2=52·8%$; webfigure 4)
was restricted to trials of community-based care after
hospital discharge ($I^2=40·3%$) and fall prevention
($I^2=65·8%$). Use of random-effects meta-analysis
led to wide CIs including unity for the interventions
targeting fall prevention (0·91, 0·82–1·00), but the
overall effect including all trials was broadly similar
(0·89, 0·83–0·96). The four trials that did not report
individuals who had fallen showed benefit with reduced
total falls in intervention groups.

Physical function outcome was measured in 73 trials.
The Barthel index of activities of daily living restrictions
(n=14) and SF-36 physical function dimension (n=7) were frequently reported. Information on change and functional deterioration was available for only nine and 16 studies, respectively, and we used the widely available physical function at follow-up in our analyses.

Meta-analysis included 43 interventions with
21,651 individuals (webfigure 5). Sources of variance
data are available from the authors. In Reuben and colleagues’ trial,10 substantial differences at baseline
between randomised groups were reported, and analyses were done both with and without these data. In 36 trials with activities of daily living outcome, an
overall benefit for interventions was noted (SMD −0·08,
95% CI −0·11 to −0·04) and in 14 trials with a generic
physical function outcome that did not specifically
focus on disability, the effect was similar (−0·09,
−0·13 to −0·05). Exclusion of the trial with baseline
differences had little effect on the SMD, and heterogeneity was evident for both outcomes ($I^2=37·5%$
and $I^2=57·5%$, respectively).

When SF-36 physical function means and variances
from the 1992 Office for National Statistics survey were
used,10 an SMD of 0·09 translated as an improvement
in a representative elderly population of between 3·3%
and 7·2% dependent on age. For the Barthel index, in
the trials included in the review, the SMD of 0·08
equated to about half a point improvement in the
20-point score.

Combination of activities of daily living and generic
outcomes (only used if no activities of daily living outcome reported) showed a similar benefit (SMD −0·08,
95% CI −0·11 to −0·06; webfigure 5). Heterogeneity
was little different in the combined analysis to that seen
when activities of daily living and generic measures
were analysed separately and was mainly restricted to
trials of geriatric assessment in elderly people selected
as frail. However, RRs were much the same in
random-effects meta-analysis (−0·08, −0·13 to −0·04).
Geriatric assessment in general populations and falls
interventions showed benefit for physical function at
follow-up when grouped by context.

Physical function was reported in a form unsuitable for
meta-analysis in 30 trials. Two interventions showed
improvement in activities of daily living20,25 and five
showed weak evidence of benefit.33,50,71 However, no
improvement was noted in 19 trials.

Study quality in terms of losses to follow-up
(webtable 2) and randomisation process did not affect
our findings—eg, RR of not living at home in 19 trials
in which the randomisation process was not clear
was 0·92 (95% CI 0·85–0·98), similar to that reported
in trials with a clear description of randomisation (0·95,
0·93–0·98).

Analysis of results excluding trials with cluster
randomisation had little effect on the overall RRs and
variances. The contribution of the cluster randomised
MRC trial to the meta-analyses was large, providing
71%, 58%, and 73% of not living at home, death, and nursing-home admission events,
respectively. However, after exclusion of the trial, the
results were reasonably consistent with RRs of 0·94
(95% CI 0·90–0·98) and 0·90 (0·83–0·97) for not
living at home and nursing home admissions,
respectively. When the MRC trial was excluded, the RR
of death was reduced, although the 95% CI included
unity (0·96, 0·92–1·00).

In trials with increased death rates, the RR of not
living at home was reduced (second quartile of death
rate 0·91, 95% CI 0·84–0·98, p=0·02; third quartile 0·96,
0·93–0·98, p=0·01; and fourth quartile 0·88, 0·79–0·96,
p=0·05; webtable 3). Similarly, nursing-home
admissions were reduced after intervention
in populations with high death rates, which differed
significantly from one for the two highest quartiles
(third quartile of death rate 0·86, 0·82–0·90, p<0·0001,
and fourth quartile 0·75, 0·63–0·89, p=0·01;
webtable 3).

In trials with recruitment dates before the median
of 1993, interventions showed benefit with a combined
RR of not living at home of 0·89 (95% CI 0·84–0·93;
webtable 4), whereas in trials from 1993 onwards
the RR was 0·97 (0·94–0·99). Removal of the MRC trial
from the analysis made this difference even more
pronounced (1·04, 0·96–1·12) in trials from 1993
onwards. In meta-regression, the outcomes of not living
at home, death, and nursing home admission all
showed increased risk reduction in studies before 1993
(webtable 4). This increased risk reduction was also
apparent for specific contexts—eg, for community-based
Discussion
Our systematic review and meta-analysis showed that complex interventions can help elderly people to continue living at home, largely through prevention of the need for nursing-home care, and can help to reduce the rate of falls. Within the broad context of complex interventions, substantial variation in the format of care, involvement of health-care professionals, and site of care provision and intensity was reported. Evidence suggested that all elderly people might benefit from assessment and appropriate health and social interventions.

However, meta-analysis including trials done since 1993 suggested that modification of care beyond that achieved after earlier developments has been of little additional value. The 1980s to 1990s was a dynamic period in the specialty of care of elderly people. In the UK, the 1990 General Medical Services contract and the commission of the MRC trial of assessment and management of elderly people in the community affected the care of elderly people. Overall, care probably improved during this period because some of the principles of effective care became incorporated in normal practice.

The need for assessment of interventions was highlighted by the perceived ineffectiveness of the UK Evercare pilot programmes in relation to hospital admissions and death.27 This model involved nurse-led assessment and case management for people with long-term conditions. The UK assessment did not use a randomised approach and we identified no randomised controlled studies of the model in our widespread searches of published work. On the basis of our systematic review, we would not have expected reductions in hospital admissions or deaths in those people receiving assessment and case management.

In our review, the eligibility for care covered the broad experience of elderly people and in general the results were consistent with previous meta-analyses. Elkan and colleagues’ reported an overall benefit for home-visiting programmes in prevention of death and nursing-home admission. Stuck and coworkers’ noted benefit for home-visiting programmes, which was restricted to improvements in physical function with multidimensional assessment and many follow-ups, reduction in nursing-home admissions with increased numbers of follow-up visits, and reductions in mortality with application in younger populations (72.7–77.5 years). These reviews assessed 15 and 18 interventions, respectively. Assessment and multifactorial intervention have also shown benefit with reduction in rate of falls in three to 13 trials.12–15 Review of nine trials targeting people after hospital discharge showed some benefits with regard to living at home and institutionalisation.27

We used the principle that interventions relating to different aspects of care can be judged together as complex interventions. The interventions in this review had input from a wide range of health-care disciplines with different intensity and duration of care, but all addressed issues of preventive visits for elderly people with care based on assessment of medical and social need. Intensity, indicating direct multidisciplinary input, number of scheduled visits, and the duration of the intervention, might not capture the effective characteristics of the intervention. Inclusion of a qualitative element in trials would have helped to understand the care actually received by individuals.

Trials specifically targeting falls prevention included interventions that were more strongly focused on home safety and physical health than other trials included in this review. However, all interventions included in the review addressed diverse issues of medical and social care. Exclusion of trials specifically targeting fall prevention made little difference to overall outcomes, including risk of falling (RR 0·88, 95% CI 0·81–0·95).

The outcome of living at home might be an over simplistic marker for independent living. In Byles and colleagues’ study,28 increased admissions to nursing homes in the intervention group were attributed to the assessment process and advice given. The intervention might have led to improved understanding of the limitations of home-based care and increased awareness of alternative care available in nursing homes. Conversely, if limitation of health-care use and costs are the main objectives, unfavourable care patterns for both the individual and carers might arise.

Interpretation of results related to physical function is restricted by selective reporting in people readily available for interview follow-up and by the large losses to follow-up in trials. Previous reviews have reported the number of people with functional deterioration, but this outcome was only available for a small number of trials. A further limitation in reporting changes in physical function is the large number of different outcome measures reported.

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See Online for webtable 5

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Other outcomes, including empowerment, autonomy, independent decision making, improved self esteem, and self confidence might accurately describe the effect of an intervention to the individual. Close and colleagues measured ability to go out alone as an outcome, perhaps a better marker of independence; and Kerse and colleagues obtained information on how often people did something they really enjoyed and the frequency of interactions with family and friends. Rockwood and colleagues used goal attainment scaling as part of the intervention and follow-up. This method aimed to assess specific outcomes based on personal goals set during intervention. Various other outcome measures related to health and psychosocial status and satisfaction with care and health-service use were reported, but their diversity and application in only a few trials restricted their value in a systematic overview.

A strength of our review is the inclusion of the large MRC trial of assessment and management of elderly people in the community. Recruitment to the trial commenced in 1995 and in the context of our review is a late trial. However, the authors note that annual assessments, as promoted in the UK, were poorly implemented at this time. Although the cluster design was associated with reduced study power and the study lacked an untreated control group, the MRC trial served to support the overall meta-analysis. Although not significant at the prespecified 1% level, the reported RR for institutional admissions was 0.83 (99% CI 0.66–1.06), which was reasonably similar to that in our meta-analysis (0.87, 0.82–0.91). The outcome of living at home was not available, but an estimate based on the sum of deaths and institutional admissions again suggested similar benefit in the large trial and the meta-analysis. Neither approach showed benefit with regard to death.

Because the evidence did not suggest that one format of care provision was better than another, the possibility might exist to tailor different formats of care to the needs and preferences of the individual, a conclusion similar to that drawn from the UK assessment of an expert patient programme. Provision of alternative intervention formats and intensities could lead to better uptake and adherence with care without compromising potential benefit.

Our interpretation of the benefits of complex interventions that identify elderly people who have a high chance of reduction in ability for targeted specialist care differs from the conclusions of the MRC trial investigators who reported that, ‘The different forms of multidimensional assessment offered almost no differences in patient outcome’, which is certainly true in the context of the trial and the specific targeted versus universal interventions being assessed. We believe that our general conclusion, drawn from all the available randomised evidence, and a wide contextual understanding of the changes that have taken place in health care for elderly people during the last four decades, is of relevance in situations with less developed services for elderly people, and suggests that a withdrawal of existing well developed services would be inappropriate.

Contributors
SE conceived and designed the review and SE and PD provided supervision. ADB and KR identified and acquired reports of trials and extracted data. ADB contacted authors of trials for additional information and analysed and interpreted the data. SA provided statistical advice and input. PD, SA, RG-H, JH, and SE contributed to the interpretation of the data. ADB and SE drafted the manuscript. PD, SA, RG-H, and JH critically reviewed the manuscript. All authors saw and approved the final version of the manuscript.

Conflict of interest statement
We declare that we have no conflict of interest.

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