Evidence Synthesis/Technology Assessment
Number 94

Challenges in Synthesizing and Interpreting the Evidence From a Systematic Review of Multifactorial Interventions to Prevent Functional Decline in Older Adults

Prepared for:
Agency for Healthcare Research and Quality
U.S. Department of Health and Human Services
540 Gaither Road
Rockville, MD 20850
www.ahrq.gov

Contract No. HHSA-290-2007-10057-I, Task Order No. 3

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AHRQ Publication No. 12-05169-EF-1
October 2012

This report is based on research conducted by the Oregon Evidence-based Practice Center (EPC) under contract to the Agency for Healthcare Research and Quality (AHRQ), Rockville, MD (Contract No. HHSA-290-2007-10057-I). The findings and conclusions in this document are those of the authors, who are responsible for its content, and do not necessarily represent the views of AHRQ. No statement in this report should be construed as an official position of AHRQ or of the U.S. Department of Health and Human Services.

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None of the investigators has any affiliation or financial involvement that conflicts with the material presented in this report.

Suggested Citation:
Lin JS, Whitlock EP, Eckstrom E, Fu R, Perdue LA, Beil TL, Leipzig RM. Challenges in Synthesizing and Interpreting the Evidence From a Systematic Review of Multifactorial Interventions to Prevent Functional Decline in Older Adults. Evidence Synthesis No. 94. AHRQ Publication No. 12-05169-EF-1. Rockville, MD: Agency for Healthcare Research and Quality; October 2012.

Structured Abstract

Purpose: 1) To summarize the results of a systematic review of multifactorial assessment and management interventions to prevent functional decline in older adults for the U.S. Preventive Services Task Force and 2) to describe the methodological challenges in synthesizing and interpreting the review's findings.

Data Sources: We used two existing systematic reviews to identify trials published through January 2005 and then searched MEDLINE, the Cochrane Central Register of Controlled Trials, and the Cumulative Index to Nursing and Allied Health Literature from 2004 through June 3, 2010. We supplemented searches with suggestions from experts and citations from other publications.

Study Selection: Two investigators independently reviewed 5,553 abstracts and 208 articles against a set of a priori inclusion and quality criteria. Discrepancies were resolved by consensus. In total, we included 70 fair- to good-quality trials.

Data Extraction: One investigator abstracted data into evidence tables and a second reviewer checked these data. Activities of daily living (ADLs), instrumental activities of daily living (IADLs), falls, hospitalizations, and mortality were combined using a random effects model; institutionalizations were combined using a fixed effects model. We grouped trials based on the purpose of the trial and country in which the trial was conducted after an extensive investigation of important population, setting, and intervention characteristics.

Data Synthesis: A subset of rigorous randomized, controlled trials suggests that outpatient multifactorial assessment and management interventions have a statistically significant, albeit small, beneficial effect on measures of functional ability, such as ADL and IADL. However, we were unable to determine the clinical significance of this effect and the overall net benefit of these types of interventions due to heterogeneity amongst studies, including: older adults studied, the broad spectrum and multifactorial nature of interventions evaluated, the suboptimal and inconsistent use of outcomes measured, and the inconsistent and inadequate reporting of data that might allow comparison of populations, interventions, and outcomes across studies.

Conclusions: This review process illustrated the complexities encountered when synthesizing and interpreting the evidence in geriatric research and methods around reviewing complex interventions and multiple interrelated health outcomes. Based on the methodological challenges of this review, we offer suggestions to researchers on the design, reporting, and analysis of trials that would help address these challenges and allow for better interpretation of evidence in the future.

Chapter 1. Introduction

The U.S. Preventive Services Task Force (USPSTF) has been working to improve its methods and processes for the preventive needs of older adults.[1] Traditional methods for systematic reviews and evidence-based recommendations for prevention of cancer or chronic disease may fall short in addressing many geriatric conditions. This deficit is because of these conditions' multifactorial risk factors and/or broad interventions aimed at improving multiple related health outcomes. The multidimensional nature of geriatric care presents unique challenges to thoughtfully interpreting the evidence base for many geriatric topics.

Clinical care and research in older adults have, in part, reflected this multidimensional nature by developing comprehensive geriatric assessments—a multidisciplinary diagnostic process intended to determine an older adult's medical, psychosocial, and functional abilities/limitations in order to develop an overall plan for management.[2] Over the past two decades, there has been a large body of international research evaluating different interventions that incorporate both inpatient and outpatient geriatric assessment approaches aimed at improving various health, quality of life, and clinical care outcomes.

In 2010, the USPSTF posted for public comment a draft recommendation statement based on a commissioned systematic review that found that exercise and physical therapy interventions and vitamin D supplementation reduced falling in community-dwelling older adults.[3] However, other interventions (notably, multifactorial assessment and management interventions) did not appear to reduce risk for falling. Because this previous review focused on interventions whose primary aim was to prevent falls, the USPSTF commissioned a second review to more broadly address the net benefit of these types of assessment and management interventions in older adults. This second review presented the opportunity to test methods around evidence synthesis and evidence-based recommendations for highly related bodies of evidence (i.e., multifactorial interventions to prevent falls and/or functional decline), as well as methods for reviewing complex interventions and multiple interrelated health outcomes. This review was designed to answer two key questions: 1) Can outpatient multifactorial assessment and management interventions improve health-related quality of life (HRQL) or reduce hospitalization, institutionalization, disability, or mortality in community-dwelling older adults? and 2) What are the adverse effects associated with these multifactorial assessment and management interventions? This report briefly summarizes our review findings and focuses on the methodological challenges we encountered and methods we used in study design, reporting, and analysis.

Summary of Review Methods

Detailed methods are available in **Appendix A**, including a description of all data manipulation and meta-analyses.

We used existing systematic reviews[3-8] and database searches through June 3, 2010, to identify included trials. Two investigators independently reviewed all abstracts and articles against a

priori inclusion criteria (**Appendix A**). We included only fair- to good-quality randomized, controlled trials (RCTs) conducted in community-dwelling older adults (age 65 years or older) that evaluated outpatient multifactorial assessment and management interventions (a clinical assessment of two or more domains of function, generally supplemented by assessment of disability-related or geriatric risk factors, in which assessment results were used as a basis for management). Two independent investigators assessed studies against the USPSTF design-specific quality criteria to assign quality ratings (**Appendix A**).[9] Discrepancies were resolved by consultation with a third investigator.

We reviewed a total of 5,553 abstracts and 208 articles, and ultimately included 70 fair- to good-quality RCTs (**Figure 1**). Included trial details are available in **Appendix B Tables 1** and **2**. Excluded trials are available in **Appendix C**. A priori primary outcomes included functional status, falls, hospitalizations, institutionalizations, HRQL, and mortality.

We conducted both qualitative and quantitative syntheses of results. A detailed description of data syntheses is available in **Appendix A**. For continuous activities of daily living (ADL) and instrumental activities of daily living (IADL) outcomes, we pooled mean differences (using standardized mean difference [SMD] [Hedges' *g* statistic]) between intervention and control groups in changes from baseline to followup scores.[10] For binary outcomes (e.g., falls, hospitalizations, institutionalizations, and mortality), the number of events and total sample size for intervention and control groups were combined using risk ratios (RRs). All outcomes were combined using a random effects model,[11] except for institutionalizations, for which we used a fixed effects model because between-study heterogeneity was estimated to be zero. We assessed the presence of statistical heterogeneity among the trials using standard chi-square tests and the magnitude of heterogeneity was estimated using the I^2 statistic. Tests of publication bias were performed using funnel plots and Egger's linear regression method.[12,13] All analyses were performed using Stata 10.0 (StataCorp, College Station, TX).

The investigators worked closely with the USPSTF leads for this topic at key points throughout the review to resolve issues around scope and for detailed input during the data analysis phase. A draft of the systematic review was also externally reviewed by three experts before review findings were presented to the full USPSTF. The Agency for Healthcare Research and Quality (AHRQ) funded this research under a contract to support the work of the USPSTF. AHRQ staff provided oversight throughout the project.

Chapter 2. Results

We included 70 fair- to good-quality RCTs (n=40,917) published from 1984 to 2010 (**Figure 1**). The 70 included trials encompassed an enormous amount of clinical and methodological diversity. Although we summarize the results of our systematic review, we primarily focus on how the heterogeneity in available evidence limits the ability to interpret the evidence and present a simple framework on how to approach this heterogeneity (**Table 1**).

Challenges in Understanding Population Risk and Complex Interventions

Our review encompassed a broad range of community-dwelling older adult populations and any outpatient multifactorial assessment and management interventions that could prevent functional decline or improve functional ability. The average age of trial participants ranged from 71 to 87 years. Twenty-four of the 70 trials included unselected or general-risk populations.[14-29] While the majority of trials targeted older adults "at risk" for functional decline, high-risk designations were based on widely varying criteria: primary care physician identification as high risk,[30-32] recently hospitalized,[33-35] recently in the emergency department,[36,37] recent fall or at increased fall risk,[38-51] screened positive for risk for functional decline or hospitalization,[5,52-59] high health care utilizers,[60,61] low income,[62] minimally care-assisted,[63,64] multiple chronic health conditions,[65] frail seniors,[66-71] mild dementia,[72] or other multifaceted approaches.[73,74] These populations represented a heterogeneous group of "at risk" older adults (**Table 1**).

Likewise, there was no consistent categorization scheme for multifactorial assessment and management interventions and intervention details were often lacking in published reports. In addition, we focused on outpatient interventions; however, these multifactorial assessment and management interventions exist on a continuum, from purely outpatient management to including management of transitions of care to managing both inpatient and outpatient care. Based on our inclusion criteria, we excluded interventions with an inpatient component that could have resulted in an artificial exclusion of interventions that were otherwise similar to those we did include. Most importantly, due to the broad inclusion criteria for outpatient multifactorial interventions, we encountered the problem of comparing effectiveness across trials evaluating a very heterogeneous group of interventions (i.e., different aims, personnel, settings, intensities, and comprehensiveness) (**Table 1**).

Of the included trials, only half explicitly sought to reduce or prevent functional decline, while other trials evaluated similarly structured interventions aimed at other purposes (e.g., to prevent falls, decrease health utilization, or manage chronic disease). About half were conducted in the United States, and the other half in countries with different health care systems and social services. Even within general types of interventions, there was sufficient variation in the assessment and management components of these interventions to potentially affect intervention success. For example, about two thirds of the trials had a one-time assessment; the other one third had repeated assessments that varied both in their assessment frequency and intervals between assessments. Assessments also varied substantially in how they were delivered (e.g.,

individual geriatric assessment by health care professional or self-administered questionnaires). The intensity and comprehensiveness of the management of identified risk factors ranged from a single contact to full management within a single multidisciplinary clinic. About three fourths of the trials evaluated interventions that provided active management of at least some of the risks/problems identified during assessment (as opposed to referring these patients to the primary care physician), half of which provided comprehensive management of all identified problems. Contacts could be in-person (clinic- or home-based) or by phone and could involve different personnel. About half of the included trials did not include geriatric expertise or specify if geriatric expertise was involved in the assessment or management of patients.

In order to synthesize the findings across the broad body of evidence, we attempted various approaches to group similar populations and interventions. To estimate the overall effectiveness of the multifactorial interventions by population risk, we attempted to apply a more standardized definition using risk factors or proxies for functional decline, including age, control group mortality, control group baseline ADL or IADL, and a composite measure of baseline frailty (age, self-rated health, and loss of one or more ADLs). However, only age and control group mortality rate were routinely reported across trials. We developed several categorization schemes, based on our assessment of the variation in key trial attributes, as well as groupings suggested by previous researchers,[7,32,75-80] in order to synthesize and interpret the results (**Appendix A**). We performed stratified analyses and meta-regressions of groupings based on clinically relevant population and intervention characteristics that were reported in individual studies, including mean age of trial population, percent female population, baseline frailty of the population, baseline functional status of the population (ADL and IADL), control group mortality rate, type of intervention, applicability of trial to current U.S. setting, comprehensiveness of the management delivered following assessment, level of geriatric expertise included in the assessment and management, and intervention intensity as measured by the number and duration of assessment and management contacts with participants. Ultimately, however, we were unable to define truly cohesive bodies of literature, despite multiple categorization schemes based on multiple dimensions of the interventions, limiting the value of pooled analyses. After consultation with the USPSTF leads and the need for some estimation of the net benefit of these interventions, we used two basic dimensions to stratify our analyses: 1) the aim of the trial, because most trials with a primary purpose of preventing functional decline measured outcomes of functional ability, and 2) the country in which the trial was conducted, because trials conducted outside the United States were potentially less applicable to U.S. practice, given the large differences in health care delivery and social services, as well as the variability in standards of care for older adults across different countries.

Challenges in Conducting Outcome Analyses

We defined a set of important outcomes a priori, which included any measure of ADL or IADL (e.g., Katz, Barthel, and Lawton scales) and any measure of HRQL (e.g., 12- and 36-item Short-form Health Survey [SF-36] or EuroQol), in addition to falls, hospitalization, institutionalization, mortality, and serious adverse events. We did not include performance-based measures of function (e.g., gait speed, timed Get Up and Go test, Performance Oriented Mobility Assessment), as these were infrequently reported as an outcome (15 of 70 trials) and never

specified as a primary outcome. Gait speed was the most commonly reported performance-based measure of function, but was reported as an outcome measure in only four trials.

Our first challenge in conducting and understanding our outcome analyses was inconsistent reporting of outcomes across trials (**Table 1**). Although mortality was reported in nearly all of the trials, death was reported as part of the CONSORT flow diagram rather than as an outcome measure. While most (51 of 70) trials reported some measure of ADL and/or IADL, nine trials did not mention the name of the instrument, and the remaining 43 trials used 20 different instruments. The three most commonly used instruments were the SF-36 physical functioning domain (ADL) in nine trials, the Barthel scale (ADL) in eight trials, and the Lawton scale (IADL) in five trials. Although multiple validated patient-reported instruments exist to measure ADL and IADL, scales show only weak and inconsistent relationships, and therefore no single scale has been accepted as the gold standard to measure functioning.[81] Other outcomes were less commonly reported (**Table 2**): HRQL (21 trials), hospitalizations (21 trials), and institutionalizations (25 trials). As with functional ability, the 21 trials reporting HRQL outcomes used 11 different instruments, with the SF-36 being the most commonly used (eight trials). This variability in patient-reported outcome measures was further complicated by evidence of selective reporting of outcomes (i.e., trials included ADL as part of the assessment but did not report it as an outcome, individual domain scores of HRQL instruments were reported but not overall component scores) and by the inconsistency of reporting a set of outcomes at similar lengths of followup across trials (**Tables 3** and **4**). Thus, the studies addressing different outcomes represent different bodies of evidence and possibly reflect selective reporting bias.

Our second challenge was that our quantitative analyses could only include a subset of trials, due to variation in outcome measurement and limitations in reporting of ADL and IADL (**Table 2**). With expert consultation and audit of the ADL and IADL instruments used in the included trials, we determined that ADL and IADL measured different constructs and that even among different ADL instruments, measured constructs were not identical (**Tables 3** and **4**). Therefore, we conducted meta-analyses for ADL and IADL separately and only combined ADL and IADL measures as part of our sensitivity analyses. Due to limitations in how outcomes were reported (e.g., continuous versus dichotomous, change from baseline or only followup measurement), only a subset of studies could be included in each meta-analysis. We were also cautious not to pool short- and long-term outcomes given the wide range of followup (6 to 39 months) and the fact that treatment effects are often critically dependent on timing.[82] After limiting our analyses to pooling outcomes at similar lengths of followup (i.e., intermediate [6 to 18 months] or long-term [24 to 39 months]), meta-analyses for patient-reported outcomes represented at most half of trials reporting this outcome measure (**Table 2**). Although HRQL measures were more comparable (in terms of measured construct), they were less frequently reported. In addition, limitations in how HRQL outcomes were reported at the individual study level (e.g., continuous versus dichotomous, domain scores versus component/overall scores, and heterogeneity in timing of outcome measurement) prevented meaningful pooling of these outcomes.

Challenges in Interpretation of Results

Although 28 of the 34 trials with a primary purpose of preventing functional decline reported

ADL or IADL outcome measures, only 14 trials were included in the meta-analysis of ADL outcomes and 10 trials for IADL outcomes, and 17 trials for either ADL or IADL outcomes at 6 to 18 months (sensitivity analysis) (**Table 5**). Meta-analysis of change in ADL outcomes at 6 to 18 months shows small but statistically significant differences favoring intervention (SMD, 0.10 [95% CI, 0.04 to 0.17]; I^2=0.0%) (**Table 5**, **Figure 2**). Pooling only U.S. trials showed a slightly higher point estimate of benefit. The meta-analysis for change in IADL outcomes at 6 to 18 months (**Table 5**, **Figure 3**) was consistent with ADL findings. These results, however, were not statistically significant (SMD, 0.10 [95% CI, -0.01 to 0.22]) and the statistical heterogeneity was much higher (I^2=50.5%). Sensitivity analysis pooling trials reporting either ADL or IADL outcomes or using combined ADL/IADL outcome measures was also consistent, but still heterogeneous (SMD, 0.09 [95% CI, 0.01 to 0.16]; I^2=42.3%) (**Table 5**, **Figure 4**). Trials that could not be pooled in the meta-analyses were generally consistent with pooled results in terms of direction of effect, although results from individual trials were often not statistically significant. Longer-term outcome analyses included far fewer studies (**Table 2**), but results were consistent with 6–18 month outcome analyses, showing small but statistically significant effect sizes (data not shown). We did not find evidence of publication bias based on the funnel plot and Egger's test for any of the meta-analyses of functional ability in trials with a primary purpose of preventing functional decline.

The trials included in the ADL and IADL meta-analyses had minimal overlap with studies that reported hospitalizations and institutionalizations (**Tables 3** and **4**) and therefore represent essentially different bodies of evidence. Meta-analyses for hospitalizations (n=7,168; 16 trials) and institutionalizations (n=6,973; 19 trials) showed no detectable effect from multifactorial assessment and management interventions (**Table 5**, **Figures 5** and **6**). Overall, event rates were low, particularly for institutionalizations (**Figures 5** and **6**). Finally, but not surprisingly, since trials were generally not powered to detect a reduction in mortality, pooled results (1,475 deaths; n=28,891) showed no significant reduction in mortality at 12 months (RR, 0.91 [95% CI, 0.82 to 1.00]; I^2=0.0%) (**Table 5**, **Figure 7**).

Restricting analyses to similar-risk populations and/or more similar interventions substantially limited the number of trials included in the analyses without significantly affecting pooled results or reducing statistical heterogeneity. For example, of the 17 trials included in the ADL/IADL meta-analyses, only four trials evaluated comprehensive multifactorial assessment and management interventions in older adults at risk for functional decline,[31,33,37,53] and only three trials evaluated less comprehensive interventions in unselected older adults.[15,26,29] Likewise, only six trials of comprehensive interventions in at risk adults[36,39,40,56,60,66] and two trials of less comprehensive interventions in unselected adults[19,26] are included in the meta-analyses for hospitalization outcomes (total 16 trials).

There were numerous challenges in interpreting the clinical significance of the small but statistically significant average changes in patient-reported ADL and IADL. We calculated pooled SMDs using Hedges' *g* statistic to quantitatively synthesize functional limitations across many different measurement instruments that were primarily reported as a continuous outcome. Overall, we found a SMD of 0.09 [95% CI, 0.01 to 0.16] for changes in functional ability (ADL or IADL). An effect size of 0.2 to 0.3 represents a small effect, 0.5 a moderate effect, and 0.8 a large effect.[83] Thus, these findings represent a small to very small magnitude of effect, even

when considering the upper limit suggested by the 95 percent confidence interval. We looked at individual trials whose SMD was similar to the pooled SMD to understand the clinical significance of this change and examined the precise change in score for those trials. For ADLs, four trials had similar effect sizes.[5,33,37,55] In these trials, the change in score was approximately a 1- to 2-point improvement in the SF-36 physical functioning score (100-point scale),[37,55] or approximately a 0.2-score improvement on the Katz ADL scale (6-point scale).[33] For IADLs, five trials had similar effect sizes.[26,33,37,54,64] In one trial, the change was as high as a 9-point improvement on the SF-36 physical functioning score (100-point scale);[54] however, it was much lower in two other studies: a 0.4-point improvement on the Older American Resources and Services scale (14-point IADL scale),[37] or about a 0.8-point improvement on the Lawton and Brody scale (23-point scale).[33] On the basis of this approach, we concluded that overall there would be a very small clinical benefit (at best) to these interventions at a population level. Although there has been a growing body of literature using anchor-based minimally important differences (MIDs) to interpret the clinical significance of patient-reported outcomes,[84,85] we could not identify established MIDs for these commonly used ADL or IADL instruments. In fact, we identified only one study that established the MID for improvement using the Barthel Index (20-point scale) in stroke patients.[86] Using a generic threshold of 0.5 on a 7-point scale as a MID, these ADL and IADL changes would not be considered clinically significant.[85]

These findings should be interpreted with caution. Our meta-analyses suggesting a small or null finding does not mean that the multifactorial interventions studied are ineffective. First, the ADL and IADL instruments used have important limitations in their measurement properties. The ADL and IADL instruments most commonly used in included studies are not always responsive to clinically important changes in community-dwelling older adults. The Barthel Index (ADL), for example, was developed in institutionalized adults and thus is not necessarily appropriate for use in other populations.[87] Even in populations for which it was designed, the Barthel Index has been shown to have floor and ceiling effects.[88,89] The Lawton scale (IADL) has weak reliability, validity, and responsiveness.[90] The overwhelming majority of trials did not report the rationale guiding their selection of ADL or IADL instrument or the validity of the chosen instrument for the population studied.

Further, these average effects likely reflect a mixture of substantial benefits for some older adults and no benefit for many older adults. This heterogeneity of treatment effects is reflected by the individual trials' relatively large standard deviations in change in functional ability.[91] One major source of this heterogeneity is likely from the different baseline risk (or prognosis) of populations studied. Older adults, compared with middle-aged adults, have more variability in their health trajectories,[92] such that people with a similar baseline health status may decline at markedly different rates.[93] Trials infrequently reported the control group health trajectory (e.g., baseline ADL and IADL at followup), which might serve as a surrogate for between-trial differences in populations. In a subset of trials that did report this data, about one third of these trials showed no decline in the control group's mean ADL or IADL, despite selecting for trial participants at risk for functional decline. The stable trajectory of ADL or IADL could indicate that the trial participants did not have any functional decline or that the measures used were not responsive to changes in functional ability. Inconsistent (or lack of) reporting of patient risk and use of mean differences without subgroup exploration at the individual study level (i.e., persons who improved or maintained their level of function versus those who declined in function) made

it impossible to comment on potentially important differential effects by subgroups (with differing risk). Additionally, the majority of trials did not report dichotomous or categorical outcomes, which would have allowed an estimation of the proportion that might benefit more substantially from these interventions.

Finally, because of a relative lack of reporting about harms (or a constellation of outcomes), we were unable to ascertain the net benefit of these multifactorial assessment and management interventions in older adults. Very few trials reported or hypothesized on the harms of these interventions, other than falls. Individual trials may not have been sufficiently powered to detect harms with low event rates, although pooled analyses showed no evidence of paradoxical harms (e.g., increased falls, disability, hospitalizations, institutionalizations, or decreased quality of life). The possibility of unintended harms, however, cannot be fully understood given the inconsistent and incomplete outcome reporting. Increased hospitalizations, for example, may not necessarily represent a true harm if it prevents functional decline or institutionalization. For example, in one study (n=539), persons who were randomized to the intervention had increased hospitalizations (not statistically significant) but decreased institutionalizations.[24] Increased falls or fallers (not leading to serious injury) was reported in a few trials (not statistically significant), but may have been from increased physical activity resulting in improved quality of life (not reported as an outcome in those studies).

Chapter 3. Discussion

We were unable to clearly determine the net benefit of using multifactorial assessment and management interventions in older adults because of the heterogeneity in older adults studied, the broad spectrum and multifactorial nature of interventions evaluated, and the suboptimal and inconsistent use of outcomes measured. Our best attempt at synthesizing findings across this very heterogeneous body of evidence suggests a small, statistically significant benefit in functional ability (**Table 6**). This small effect on ADL and IADL within a subset of the included trials is difficult to interpret, given the 1) choice of ADL or IADL instruments that may not be responsive to detecting clinically significant changes in functioning in community-dwelling older adults, 2) likely heterogeneity of treatment effects for these interventions and inability to understand the heterogeneity of populations studied (due to inconsistent and inadequate reporting of risk factors or measures to assess patient risk for functional decline, and lack of important subgroup explorations at the individual trial level), and 3) inconsistent reporting of a set(s) of outcomes resulting in different bodies of literature (and therefore different interventions in different populations) being described with ADL and IADL outcomes versus other outcomes (e.g., HRQL, hospitalizations, or institutionalizations). Attempts to pool results by similar-risk populations and types of interventions significantly limited the number of studies in these analyses, without substantially affecting the magnitude or statistical heterogeneity of pooled results. However, variation in (and lack of) measurement or reporting of important population and intervention characteristics across this body of literature, as well as differences and inconsistencies in outcomes used across studies, limited truly meaningful subgroup analyses.

Our review has limited overlap in included studies as compared with other existing systematic reviews of multifactorial assessment and management interventions in geriatric populations [4,7,8,32,75-77] (**Appendix D**). These existing reviews all had slightly different focuses (i.e., preventive home visits, primary care–relevant interventions, comprehensive geriatric assessments, complex geriatric interventions) and used different inclusion criteria, as well as differing methodological approaches. Even among reviews with more similar scope, inexact and inconsistent terminology describing complex interventions and lack of a unified theory or model describing interventions makes locating and applying inclusion criteria to identify cohesive bodies of literature challenging. The most similar, and most current, existing review by Beswick and colleagues included 89 trials focused on a broad set of complex geriatric interventions that evaluated "interdisciplinary teamwork for health and social problems." [4] Despite differences in included studies and methodologies to pool results (e.g., our use of between-group change in score and Beswick's use of measures at followup only, our more conservative pooling of results across different lengths of followup), both reviews found a similarly modest degree of benefit in preventing functional decline, and a reduction in hospitalizations in a different subset of articles reporting this outcome. Neither review found any evidence for mortality benefit. The Beswick review also concluded that this benefit in preventing functional decline was primarily accrued in a subset of interventions in general-risk older adults (as opposed to the frail elderly); however, our review cannot confirm or refute this finding due to differences in included studies for our pooled analyses and possibly more limited power to detect subgroup differences, because of more conservative pooling of outcomes. Another recent, more focused review on preventive home visits in community-dwelling older adults by Huss and colleagues had very limited overlap

in included studies.[7] Both reviews found a modest benefit for functional outcomes; however, the Huss review found benefit only for interventions that included clinical examination in the initial assessment. Again, our inability to detect this difference is likely due to the difference in included studies for our pooled analyses on preventive home visits, as the Huss reviewers were able to quantitatively combine more studies by obtaining nonpublished data on dichotomous outcomes from individual study authors and conducted less conservative pooling across a range of lengths of followup.

We understand that there is a natural tension between the goals of primary research that is interested in asking a specific clinical question and that of secondary research that is intended to inform health policy decisionmaking by synthesizing evidence broadly across primary research. However, we believe that the methodological challenges encountered provide insight into important considerations for future research to improve care for older adults to prevent functional decline (**Table 7**). First, consistently and completely ascertaining study population baseline risk is extremely important. The considerable variability in the natural history of functional decline in older adults introduces random error and reduces the likelihood of finding a consistent group effect.[94,95] More complete and consistent ascertainment of population functioning and risk for decline in functioning would allow investigators to examine the effectiveness of interventions in subgroups that are at higher risk for functional decline and disability, as well as considering intervention effects on subgroups with differing functional status trajectories.

Second, complex interventions are hard to characterize, partly due to incomplete and inconsistent reporting. When possible, it is important to both enhance the consistency and reproducibility of interventions by improved reporting of important intervention details.[96,97] Trials evaluating complex interventions should capture important details about, for example, conditions/targets, mode of delivery, frequency, contact time, duration, and personnel involved for both assessment and management. More research is needed to test consistent models, or intervention components, across a series of trials, in similar populations for reproducibility of effectiveness, as well as across different populations and settings.

Third, there is considerable variability in reported trial outcomes, as well as methodological challenges around outcome measurement. For measures of function, we focused on self-reported measures (i.e., ADL and IADL). However, trials used many different ADL and IADL measures that were often validated in very different populations and occasionally not clearly identified. There is a strong need for consensus and standardization in measuring global functioning and functional decline in community-dwelling older adults. Other evidence suggests this need applies to hospitalized older adults as well.[98] Authors using ADL, IADL, and HRQL instruments need to report the name of the instrument, its intended purpose, and its appropriateness for intended use (e.g., document the instrument's validity and sensitivity to change in the study population). In evaluating outcomes, it is important to report baseline and followup values, not just change in scores, to allow for best interpretability of trials. Selective reporting of subcomponents of HRQL measures should only be done if these subcomponents are specified a priori as primary or secondary outcomes. Dichotomous outcomes are perhaps more clinically relevant, certainly more clinically intuitive, than continuous outcomes, but this needs to be based on clinically meaningful and consistent standards to allow for comparison across trials.

Future research would greatly benefit from using a focused and consistent set of agreed-upon measures, or core clinical outcomes, within a given population that 1) adequately capture clinically meaningful change in functioning with respect to a certain population (e.g., valid and responsive measures for functional ability may differ for community-dwelling versus institutionalized older adults), 2) capture multiple dimensions of health (e.g., HRQL), and 3) include common health care utilization measures (e.g., emergency department visits, hospitalizations, institutionalizations) that may be proxies for health outcomes. Of course, the choice of individual trial outcomes must be guided in part by the trial's population, intervention, and sample size. Some effort toward using a set of core clinical outcomes that are both responsive and multidimensional would greatly improve the ability of evidence synthesis to inform medical decisionmaking. Standards for these types of research should consider whether measurement of self-reported functioning should be enhanced by additional use of a set of well-validated performance-based measures. Although expert consensus on trial design aimed at preventing or slowing functional decline has recommended limiting outcome measures to "hard" measures of disability, such as measures of ADL,[99] more recent evidence supports the use of global performance-based measures. Gait speed, for example, has been shown to be associated with mortality.[100]

Most clinicians and researchers who care for older adults believe that we can only truly optimize the care of all older adults by affecting multiple aspects of health, from multiple perspectives/disciplines, over a span of aging that includes many possible functional trajectories. It is imperative that valid, consistent, and targeted trials be performed to clarify and solidify the appropriate health interventions for this growing population.

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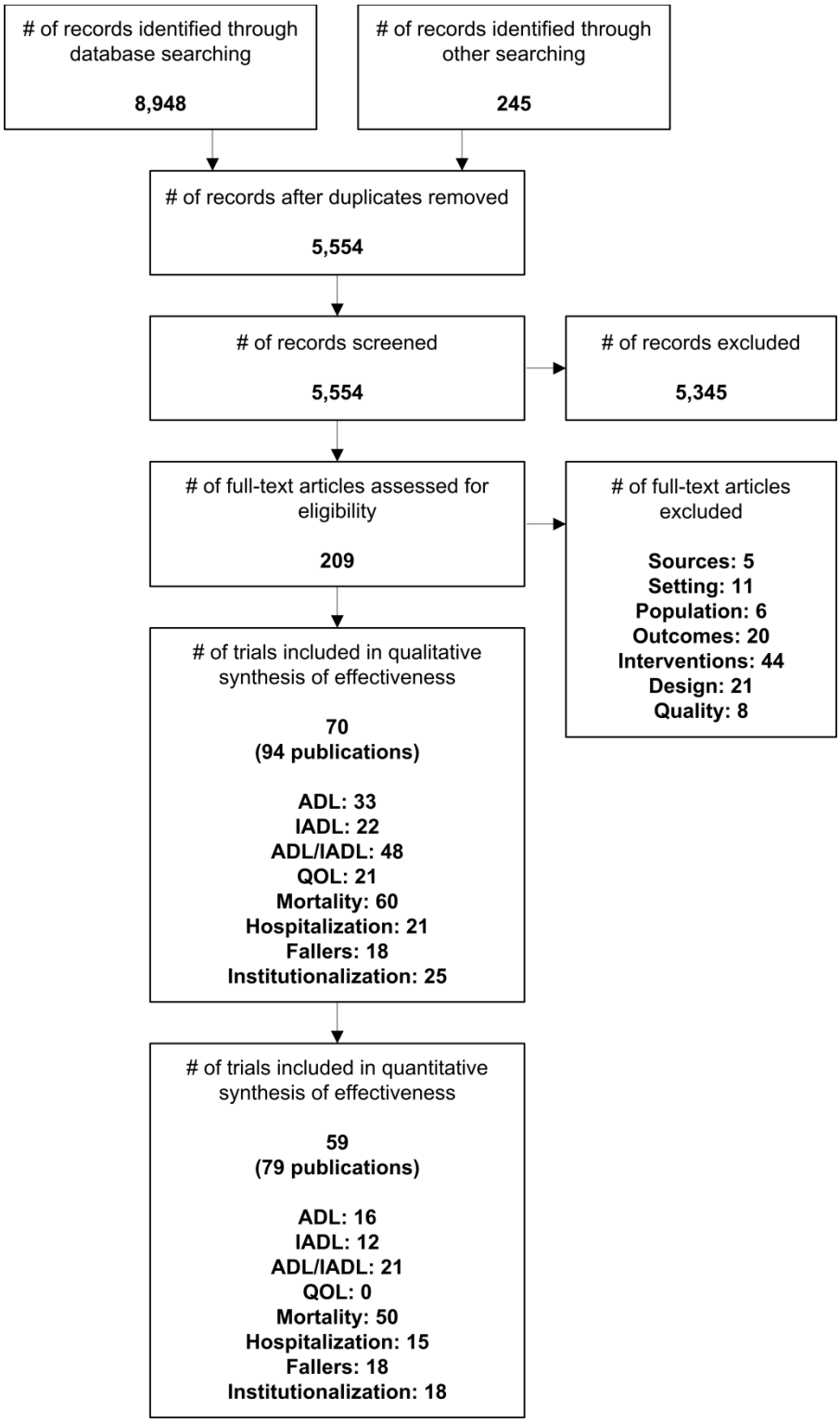
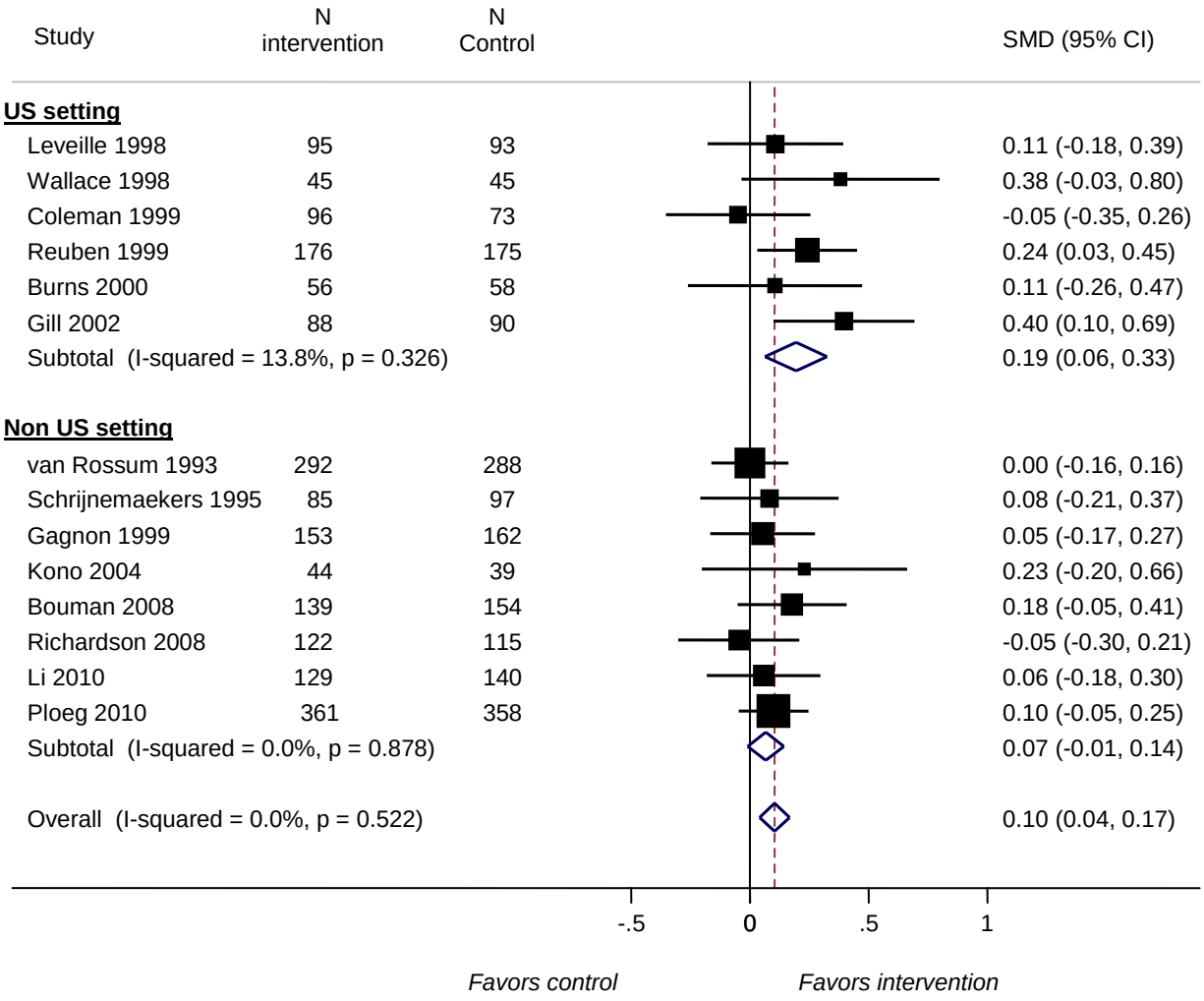
Figure 1. Search Results and Article Flow



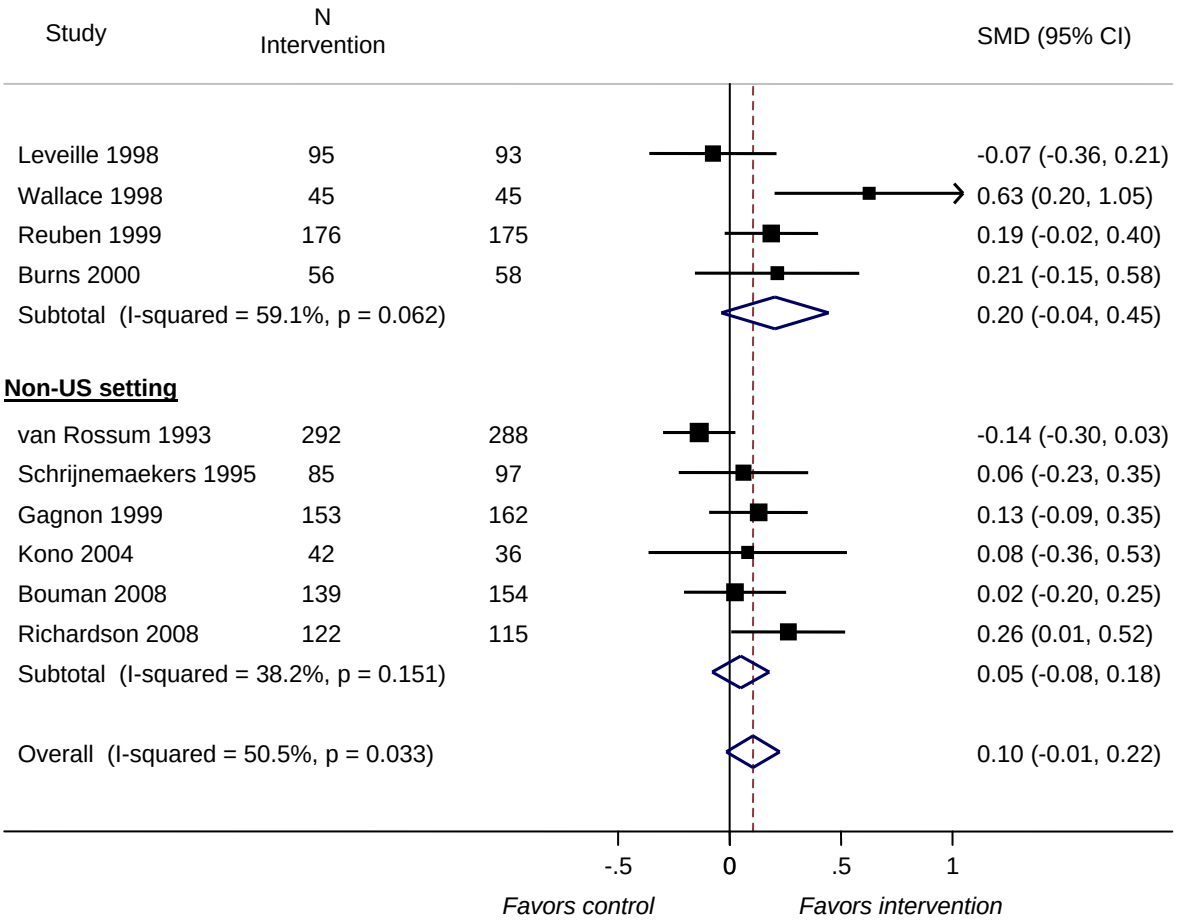
Abbreviations: ADL=activities of daily living; IADL=instrumental activities of daily living; QOL=quality of life.

Figure 2. Meta-Analysis of Activities of Daily Living at 12 Months for Interventions With the Primary Purpose of Preventing Functional Decline



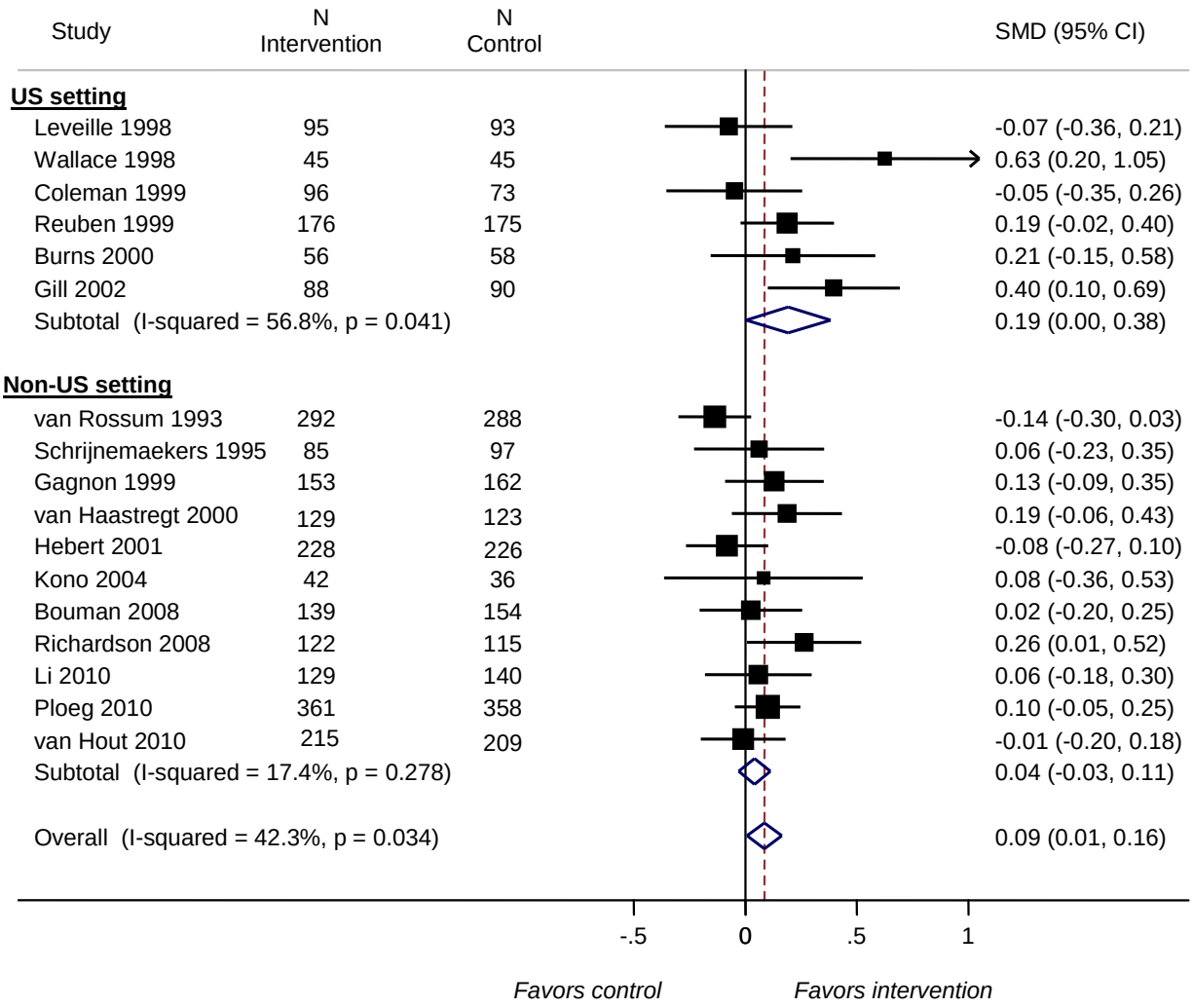
Abbreviations: CI=confidence interval; N=number; SMD=standardized mean difference; US=United States.

Figure 3. Meta-Analysis of Instrumental Activities of Daily Living at 12 Months for Interventions With the Primary Purpose of Preventing Functional Decline



Abbreviations: CI=confidence interval; N=number; SMD=standardized mean difference; US=United States.

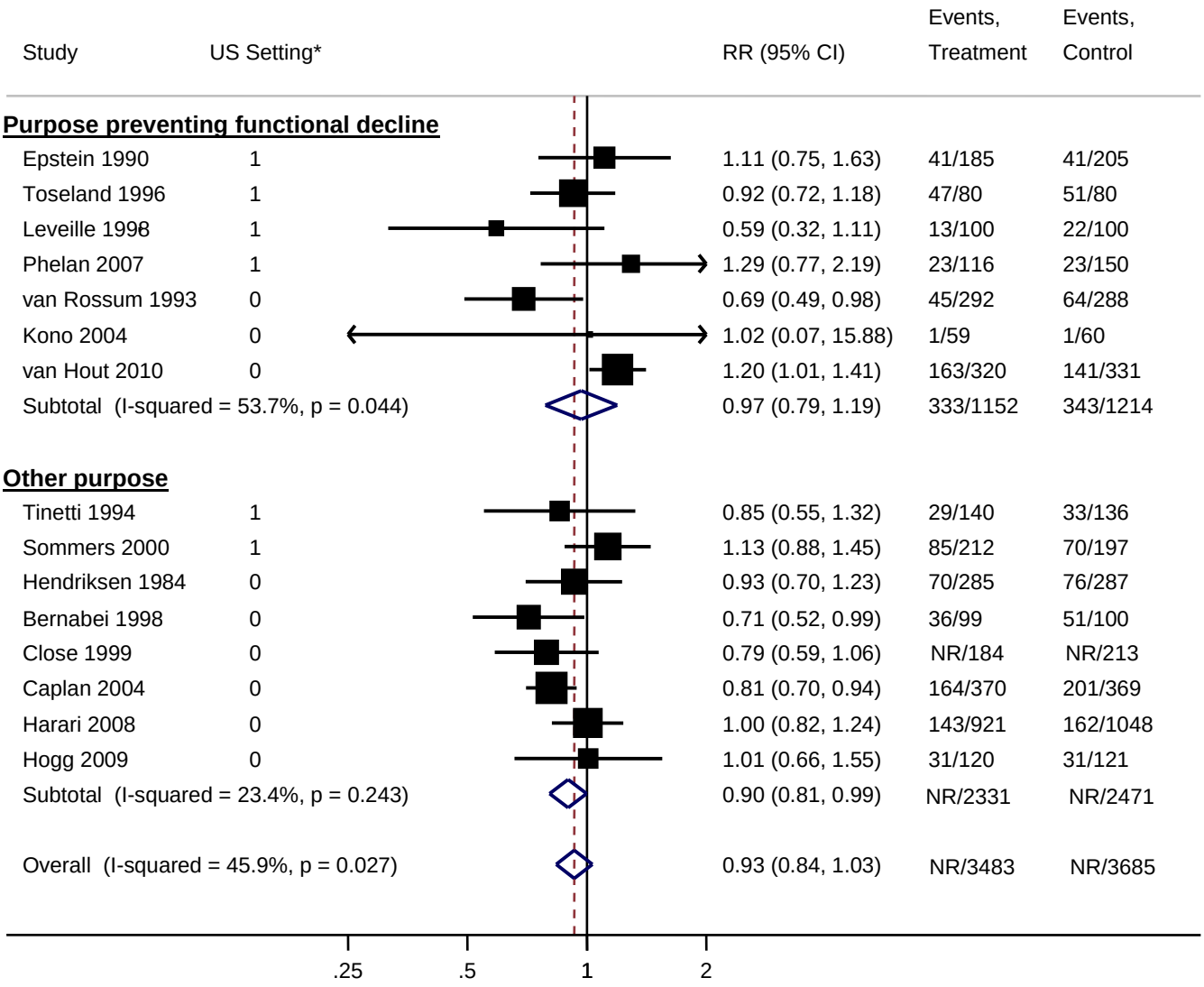
Figure 4. Meta-Analysis of Activities of Daily Living/Instrumental Activities of Daily Living, Instrumental Activities of Daily Living, or Activities of Daily Living* at 12 Months for Interventions With the Primary Purpose of Preventing Functional Decline



* If more than one instrument was available for an ADL, IADL, or ADL/IADL outcome, the ADL/IADL outcome was given preference, followed by the IADL outcome, and lastly the ADL outcome.

Abbreviations: ADL=activities of daily living; CI=confidence interval; IADL=instrumental activities of daily living; N=number; SMD=standardized mean difference; US=United States.

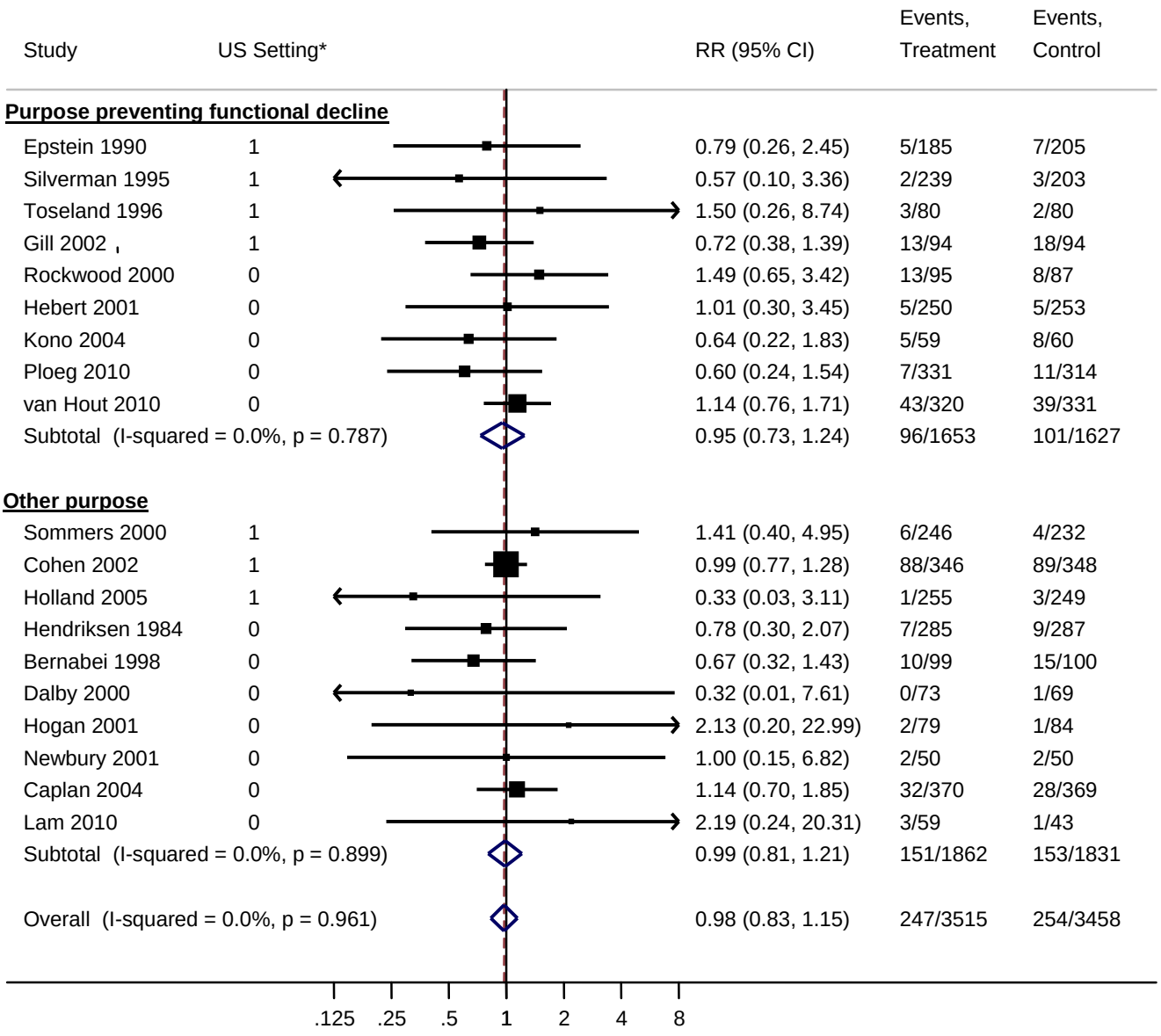
Figure 5. Meta-Analysis of Hospitalizations at 12 Months



* 1=U.S. setting; 0=non-U.S. setting.

Abbreviations: CI=confidence interval; NR=not reported; RR=relative risk; US=United States.

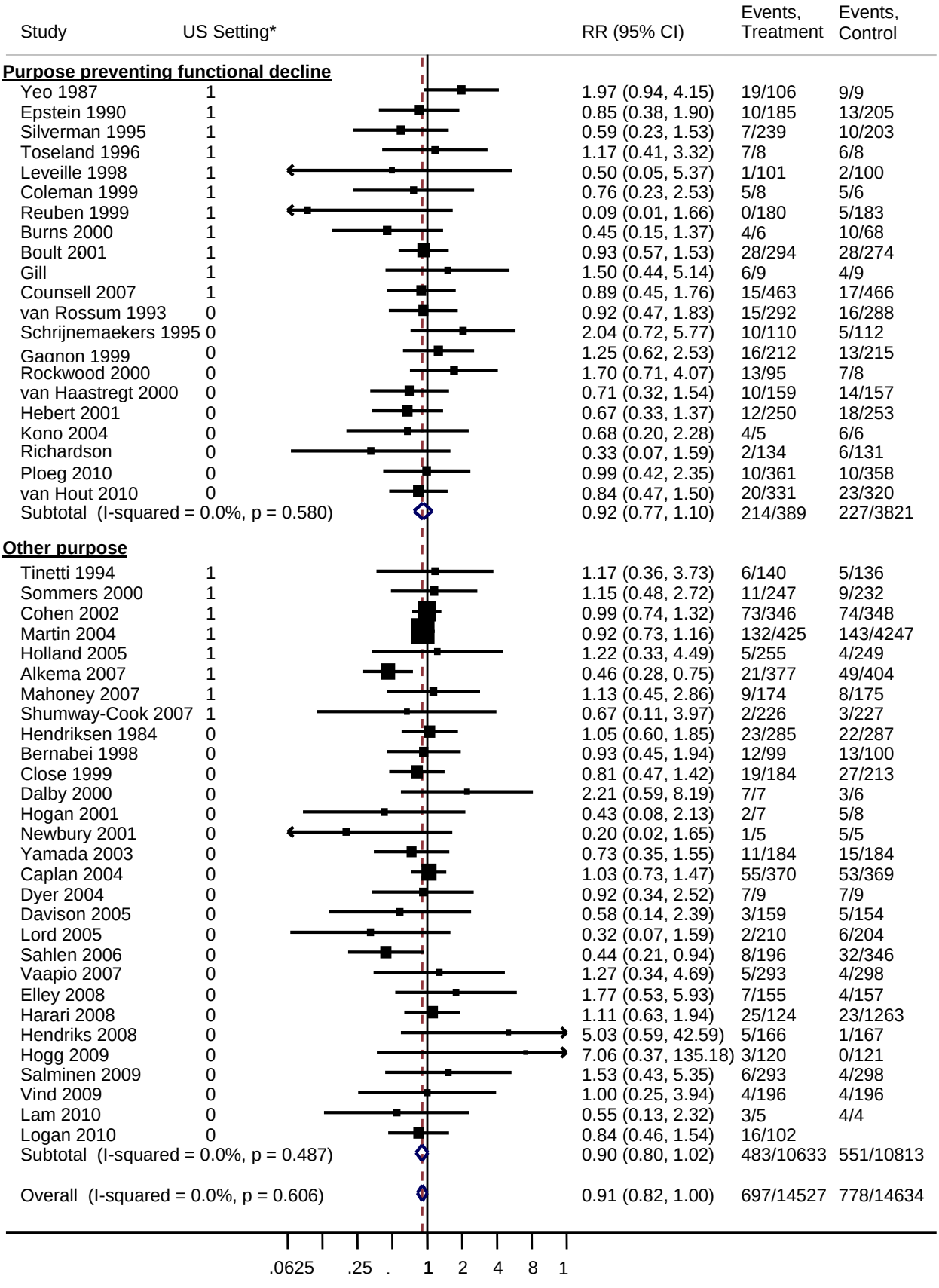
Figure 6. Meta-Analysis of Institutionalizations at 12 Months



* 1=U.S. setting; 0=non-U.S. setting.

Abbreviations: CI=confidence interval; RR=relative risk; US=United States.

Figure 7. Meta-Analysis of Mortality at 12 Months



* 1=U.S. setting; 0=non-U.S. setting.
Abbreviations: CI=confidence interval; RR=relative risk; US=United States.

Table 1. Framework for Understanding Heterogeneity Across Trials, With Examples From Evaluating Multifactorial Assessment and Management Interventions in Older Adults

Framing questions	What constructs are assessed? What are the potential sources of clinical heterogeneity?	How are these constructs measured? How good are the different methods of measurement?	What is the most informative way to summarize these constructs? Due to heterogeneity, what can (and cannot) be said about the findings?
Populations	*Risk*: General (unselected) population or population selected for increased risk for functional decline	2/3 of trials studied populations selected for increased risk for functional decline but used very different definitions of risk (e.g., recent emergency department visit or hospitalization, multiple chronic health conditions, frailty)	Difficulty applying a standardized definition of population risk due to lack of routine reporting of patient risk or use of crude measures of baseline risk Pooled analyses included stratification based on population risk (general vs. at risk); however, unclear if comparing similar risk populations
Interventions	*Aim*: Primary purpose of intervention (e.g., to prevent functional decline) *Personnel*: Training of individuals involved in assessment and management *Setting*: Where (and how) the assessment and management was delivered *Intensity*: Duration (time and frequency) of the assessment and management *Comprehensiveness*: Level of active management of the identified risk factors/conditions	1/2 of trials with primary aim to reduce functional decline, other stated aims varied widely (e.g., prevent falls, decrease resource utilization) 1/2 of trials involved geriatric expertise, some trials involved lay personnel Interventions were delivered through the home, community centers, primary care practices, or geriatric clinics Many trials did not report details about intervention to assess intensity or comprehensiveness; wide variation in delivery and frequency of assessments, as well as variation in intensity and comprehensive-ness of management subsequent to assessment	No consistent categorization scheme for interventions, and minimal reporting of intervention details made it difficult to group similar interventions Pooled analyses stratifying results based on single dimensions of intervention characteristics and population risk, or meta-regression of multiple dimensions of heterogeneity, substantially limited the number of trials to pool and were unsuccessful in explaining statistical heterogeneity
Comparators	*Type of control group*: Usual care, minimal care, wait-list control	Many trials did not report details about control group care Usual care control groups varied across trials as evidence base spanned 20 years (secular trends over time affect usual care) and 1/2 of trials conducted in countries with different health care systems and social services from the United States	Control groups or usual care not well defined, making comparison of findings over time and in different countries difficult Lack of intervention effect in more recent trials or trials conducted in Canada and Europe may be influenced by different standard of care in usual care group, and therefore may not be applicable to many U.S. practices
Outcomes	*Type*: A priori outcomes (i.e., functioning /ability, HRQL, hospitalization, institutionalization, mortality) *Followup*: Time points at which outcomes were measured *Subgroup analyses*: A priori subpopulations for which outcomes were reported	Inconsistent reporting of types of outcomes across trials, many outcomes not frequently reported (i.e., HRQL, hospitalization, institutionalization) Variation in how outcomes were measured and reported (e.g., missing data, continuous vs. dichotomous, change from baseline vs. followup measurement) ADL/IADL were commonly used as outcome measures but used different instruments, and certain instruments may not be responsive or sensitive in community-dwelling older adults	Little overlap in trials reporting different outcomes and lack of reporting of multiple (constellation of) outcomes limits ability to understand overall clinical effect of interventions Evidence of selective reporting of outcomes Many trials could not be included in meta-analyses because of limitations in how outcomes were reported or because of selective reporting Lack of a priori subgroup analyses limit ability to understand heterogeneity of treatment effects

Abbreviations: ADL=activities of daily living; HRQL=health-related quality of life; IADL=instrumental activities of daily living.

Table 2. Number of Included Trials for Meta-Analyses by Outcome

Primary aim	Outcome	Number of trials with outcome	Number of trials with ~12 month meta-analyses*	Number of trials with long-term meta-analyses**
To reduce functional decline (34 trials)	ADL or IADL	28	ADL: 14 IADL: 10 ADL or IADL: 17†	ADL: 6 IADL: 5 ADL or IADL: 7†
All (70 trials)	Hospitalization	21	16	5
	Institutionalization	25	19	7
	Mortality	66	50	21
	Quality of life	21	No meta-analysis	No meta-analysis

* ~12 month meta-analysis = 6 to 18 months.
** Long-term meta-analysis = 24 to 39 months (no trials reported outcomes between 18 and 24 months).
† Sensitivity analyses.

Abbreviations: ADL=activities of daily living; IADL=instrumental activities of daily living.

Table 3. Outcomes Reported for Interventions With a Primary Purpose of Preventing Functional Decline

Location	Author	ADL/IADL	Mortality	Hospitalization	Institutionalization	Quality of Life	Falls
U.S. Trials	Yeo 1987[1]	x	xx	-	-	x	-
	Epstein 1990[2]	x	xx	xx	xx	-	-
	Wagner 1994[3]	x	x	-	-	-	xx
	Silverman 1995[4]	x	xx	x	xx	-	-
	Stuck 1995[5]	xx	x	x	x	-	-
	Toseland 1996[6]	x	xx	xx	xx	-	-
	Leveille 1998[7]	xx	xx	xx	-	-	-
	Wallace 1998[8]	xx	-	-	-	-	-
	Coleman 1999[9]	xx	xx	-	-	-	xx
	Reuben 1999[10]	xx	xx	-	-	x	-
	Burns 2000[11]	xx	xx	-	-	x	-
	Boult 2001[12]	x	xx	-	-	x	-
	Gill 2002[13]	xx	xx	-	xx	-	-
	Counsell 2007[14]	xx	xx	-	-	x	-
	Phelan 2007[15]	x	-	xx	-	-	-
Non-U.S. Trials	Vetter 1984[16]	x	x	-	-	-	x
	Carpenter 1990[17]	x	x	x	-	-	-
	McEwan 1990[18]	x	x	-	-	-	-
	Pathy 1992[19]	-	x	-	x	x	-
	van Rossum 1993[20]	xx	xx	xx	-	-	-
	Schrijnemaekers 1995[21]	xx	xx	-	-	-	-
	Gagnon 1999[22]	xx	xx	-	-	-	-
	Rockwood 2000[23]	x	xx	-	xx	x	-
	Stuck 2000[24]	x	x	-	x	-	-
	van Haastregt 2000[25]	x	xx	-	-	-	xx
	Hebert 2001[26]	x	xx	-	xx	x	-
	Kono 2004[27]	xx	xx	xx	xx	-	-
	Chi 2006[28]	x	-	-	-	-	-
	Bouman 2008[29]	xx	x	x	x	-	-
	Melis 2008[30]	x	x	-	-	-	-
	Richardson 2008[31]	xx	xx	-	-	-	-
	Li 2010[32]	xx	x	-	-	-	-
	Ploeg 2010[33]	xx	xx	-	xx	x	-
	van Hout 2010[34]	xx	xx	xx	xx	-	-

x=data available; xx=data included in meta-analysis.

Abbreviations: ADL=activities of daily living; IADL=instrumental activities of daily living.

Table 4. Outcomes Reported for Interventions With a Primary Purpose Other Than Preventing Functional Decline

Location	Author	ADL/IADL	Mortality	Hospitalization	Institutionalization	Quality of Life	Falls
U.S. Trials	Tinetti 1994[35]	-	xx	xx	-	-	xx
	Morrissey 1995[36]	-	-	-	-	x	-
	Sommers 2000[37]	x	xx	xx	xx	-	-
	Cohen 2002[38]	-	xx		xx	-	-
	Martin 2004[39]	x	xx	-	-	x	-
	Alkema 2007[40]		xx	-	-	-	-
	Mahoney 2007[41]	x	xx	-	-	-	-
	Shumway-Cook 2007[42]	-	xx	-	-	-	xx
	Holland 2005[43]	x	xx	x	xx	-	-
Non-U.S. Trials	Hendriksen 1984[44]	-	xx	xx	xx	-	-
	Vetter 1992[45]	-	x	-	-	-	x
	Gallagher 1996[46]	x	-	-	-	x	-
	Bernabei 1998[47]	x	xx	xx	xx	-	-
	Close 1999[48]	x	xx	xx	-	-	xx
	Dalby 2000[49]	-	xx	-	xx	-	-
	Hogan 2001[50]	-	xx	-	xx	-	xx
	Newbury 2001[51]	x	xx	-	xx	-	xx
	Lightbody 2002[52]	x	-	-	-	-	xx
	Yamada 2003[53]	-	xx	-	-	x	-
	Byles 2004[54]	x	x	x	x	x	-
	Caplan 2004[55]	x	xx	xx	xx	-	-
	Dyer 2004[56]	-	xx	-	-	-	xx
	Davison 2005[57]	-	xx	-	-	-	xx
	Lord 2005[58]	-	xx	-	-	-	xx
	Sahlen 2006[59]	-	xx	-	-	-	-
	Salminen 2009[60]	-	xx	-	-	-	xx
	Thomas 2007[61]	-	-	-	x	-	-
	Vaapio 2007[62]	-	xx	-	-	-	-
	Elley 2008[63]	x	xx	-	-	x	xx
	Harari 2008[64]	-	xx	xx	-	-	-
	Hendriks 2008[65]	x	xx	-	-	x	xx
	Peri 2008[66]	x	-	-	-	x	xx
	Vind 2009[67]	x	xx	-	-	-	xx
	Hogg 2009[68]	x	xx	x	-	x	-
	Lam 2010[69]	-	xx	-	xx	x	
	Logan 2010[70]	x	xx	-	-	-	x

x=data available; xx=data included in meta-analysis.

Abbreviations: ADL=activities of daily living; IADL=instrumental activities of daily living.

Table 5. Pooled Effect Sizes for Various Outcomes at 6–18 Months Post Baseline

Outcome	Location	Effect Size (95% CI)*	Number of Trials	I^2 (%)
ADL	U.S.	0.19 (0.06 to 0.33)	6	13.8
	Non-U.S.	0.07 (-0.01 to 0.14)	8	0.0
	All	0.10 (0.04 to 0.17)	14	0.0
IADL	U.S.	0.20 (-0.04 to 0.45)	4	59.1
	Non-U.S.	0.05 (-0.08 to 0.18)	6	38.2
	All	0.10 (-0.01 to 0.22)	10	50.5
ADL/IADL**	U.S.	0.19 (0.00 to 0.38)	6	56.8
	Non-U.S.	0.04 (-0.03 to 0.11)	11	17.4
	All	0.09 (0.01 to 0.16)	17	42.3
Hospitalizations	U.S.	1.00 (0.87 to 1.15)	7	0.0
	Non-U.S.	0.90 (0.78 to 1.03)	9	58.3
	All	0.93 (0.84 to 1.03)	16	42.2
Institutionalizations	U.S.	0.94 (0.76 to 1.18)	7	0.0
	Non-U.S.	1.01 (0.80 to 1.27)	12	0.0
	All	0.98 (0.83 to 1.15)	19	0.0
Mortality	U.S.	0.89 (0.78 to 1.02)	19	5.7
	Non-U.S.	0.93 (0.81 to 1.07)	31	0.0
	All	0.91 (0.82 to 1.00)	50	0.0

* ADL, IADL, and ADL/IADL are continuous outcomes, and any effect size greater than 0 indicates the intervention had a favorable effect. Hospitalizations, institutionalizations, and mortality are dichotomous outcomes, and any effect size greater than 1 indicates the intervention had a favorable effect.
** ADL/IADL as an outcome included, in order of preference, combined measures of ADL and IADL, IADL alone, and ADL alone.

Abbreviations: ADL=activities of daily living; CI=confidence interval; IADL=instrumental activities of daily living; US=United States.

Table 6. Summary of Evidence

Body of Evidence	Validity	Consistency	Findings	Limitations
70 trials (n=40,917)	**Internal:** Fair quality; very few good-quality trials. **External:** Fair applicability; only 1/3 of trials in United States, many interventions involved geriatric expertise, many interventions not widely available in primary care or referable from primary care.	Large amount of clinical heterogeneity: • Trials in different risk populations, about 1/3 of trials in general risk populations • Large variation in types of interventions evaluated • Difficult to adequately categorize similar populations and interventions primarily due to limitations in reporting at the trial level Variation in what outcomes measures reported, how they were reported, and length of followup for which they were reported. Variation in outcomes resulted in different bodies of literature being represented by each set of outcome analyses.	**Effectiveness:** All of the trials with a primary purpose of preventing functional decline reported some measure of ADL and/or IADL. Meta-analysis for functional ability (17 trials) showed a small but statistically significant increase in ADL and/or IADL at about 12 months (SMD, 0.09 [95% CI, 0.01 to 0.16]). Results at longer-term followup were consistent with 12-month findings but included far fewer studies (7 trials). Results from trials that could not be pooled were consistent with meta-analyses. These very small relative changes may not be clinically significant. HRQL outcomes, hospitalizations, and institutionalizations were not commonly reported. Meta-analyses for hospitalizations, institutionalizations, and mortality outcomes showed no statistically significant differences, but event rates were low. **Harms:** Although some trials reported slightly higher mortality, institutionalizations, hospitalizations, and falls in the intervention group compared with the control group, the difference was only statistically significant in one instance (for institutionalization). Meta-analyses for these outcomes did not show any statistically significant evidence of harms.	Different instruments used to measure ADL and IADL outcomes, incomplete reporting of details about outcome measurement, unclear validity and comparability of some instruments. Many trials could not be included in meta-analyses because of limitations or differences in reporting outcomes and lengths of followup; HRQL outcomes could not be pooled due to sparse reporting and differences in how outcomes were reported (likely reflecting selective reporting). Small, nonclinically significant differences might not reflect limited benefit given nonresponsive-ness of outcome measures and heterogeneity of treatment effects that could not be explored. Specific harms rarely reported and individual trials likely not powered to detect harms; unclear if some unintended consequences represent net harm (e.g., if increase in hospitalization is truly a harm). Inconsistent reporting of a set of comprehensive, comparable outcomes across trials complicates the assessment of the overall effect of interventions.

Abbreviations: ADL=activities of daily living; CI=confidence interval; HRQL=health-related quality of life; IADL=instrumental activities of daily living; SMD=standardized mean difference.

Table 7. Considerations for Future Research on the Health and Functional Decline of Older Adults

Considerations	For individual studies	For the field of research
On populations	Use commonly accepted measures for risk of functional decline (e.g., age, ADL/IADL, self-rated health) Report measure of functional ability (e.g., ADL/IADL) at baseline and followup to demonstrate trajectory of health for intervention and control groups Define a priori important clinical subgroups based on different levels of risk for functional decline	Identify robust measures for characterizing an individual's or population's risk for functional decline Identify clinically important subgroups that are at greater risk for functional decline; subgroups that should be routinely considered
On interventions (and comparators)	Provide details of complex interventions (i.e., purpose, personnel, setting, intervention components, and intensity and comprehensiveness of components) Be explicit about control group intervention; be explicit about usual care (because usual care varies widely across settings and countries)	Evaluate consistent interventions or intervention components across trials (i.e., test reproducibility of similar interventions in similar populations, and across different populations and settings)
On outcomes	Report outcomes at baseline and followup, not just the difference or change in outcome scores Consider reporting dichotomous outcomes (based on standard thresholds) of functional decline or ability Avoid selective reporting of outcomes and subcomponents of scores (unless subcomponents are identified a priori as a primary or secondary outcome)	Identify core clinical outcomes for community-dwelling older adults (i.e., constellation of outcomes that capture overall health and function) Identify robust outcome measures for global function for community-dwelling older adults (i.e., specific instruments for performance-based and patient-reported functional abilities) Identify meaningful population-level change thresholds in commonly used patient-reported outcomes measuring overall function or quality of life

Abbreviations: ADL=activities of daily living; IADL=instrumental activities of daily living.

Analytic Framework and Key Questions

Building on the methods and approach of the 2009 USPSTF evidence review of interventions to prevent falls in older adults,[1] we developed an analytic framework and formulated two key questions to guide our systematic review. The key questions were designed to evaluate the effectiveness and harms of primary care–relevant multifactorial assessment and management interventions in reducing disability and maintaining independence among community-dwelling older adults. Multifactorial assessment and management interventions are defined as a preventive strategy aimed at the identification and treatment of multidimensional geriatric risk factors. This strategy generally includes evaluation of physical functioning, with or without assessment of cognitive functioning or social problems. Multifactorial assessment and management interventions for this review must include a clinical assessment of two or more domains of functioning, generally supplemented by assessment of disability-related or general geriatric risk factors and/or conditions. The assessment results also must be used as the basis for ongoing management.

Key question 1: Do primary care–relevant multifactorial assessment and management interventions improve quality of life; reduce hospitalization, disability, or mortality; or maintain independent living in community-dwelling older adults?

Key question 2: What are the adverse effects associated with these multifactorial assessment and management interventions (e.g., paradoxical increase in falls, hospitalization, or institutionalization)?

Search Strategy

Incorporating a published strategy,[2] we used two recent systematic reviews as a foundation for our literature search.[1,3] The first review was conducted for the USPSTF and addressed multifactorial assessment and management interventions to prevent falls in older adults.[1] The second review was a comprehensive systematic review that included 87 trials of "complex interventions" to improve physical functioning and maintain independent living in older adults published before 2005.[3] We reviewed all the full-text articles of both the included and excluded trials from these two reviews, as well as checked the reference listing for included trials from several other relevant reviews.[3-7] We then conducted a search for both key questions in MEDLINE, the Cochrane Central Registry of Controlled Trials, and the Cumulative Index to Nursing and Allied Health Literature from 2004 through June 3, 2010 (see **Appendix A Table 1** for the search string).

Inclusion and Quality Criteria

Two investigators independently reviewed all abstracts and articles against inclusion and exclusion criteria (see **Appendix A Table 2** for the inclusion and exclusion criteria) and critically appraised all included articles using design-specific criteria and USPSTF methods.[8] The USPSTF has defined a three-category quality rating of "good," "fair," and "poor" based on specific criteria (see **Appendix A Table 3** for quality criteria). Discrepancies in quality ratings

were resolved by consultation with a third investigator. All trials rated as poor quality were excluded from the review.

Data Abstraction and Outcomes

One investigator abstracted data from included trials into evidence tables and a second investigator reviewed abstracted data for accuracy. We abstracted prespecified study details into evidence tables that included the following items: study purpose, setting (location, target population, recruitment strategy), population characteristics (study inclusion and exclusion criteria, participant age, sex, race/ethnicity, and socioeconomic status, as defined by income or education), baseline health status (self-rated health, disability, previous hospitalizations, living situation, and cognitive impairment) and high-risk categorization (as defined by the trial), intervention characteristics (assessment components and delivery approach, management components and personnel), and outcomes. Relevant outcomes for abstraction included any measure of ADL or IADL (e.g., Katz or Lawton scale, SF-36 physical functioning domain) and any measure of HRQL (e.g., SF-12, SF-36, or EuroQol), in addition to falls, hospitalization, institutionalization, mortality, and adverse events (e.g., harms requiring unexpected medical attention).

Patient-reported outcomes: ADL, IADL, and HRQL. A variety of instruments were used for ADL, IADL, and HRQL. Some studies used more than one instrument to measure the same outcome, and author designations of the purpose of the instrument were not always clear or consistent across studies. We included any instrument that was consistent with measuring typical ADL or IADL, and consulted with a geriatrician when needed. We included any HRQL instrument. Often the instrument was not reported, but the authors detailed the questions in the methods of the article so we could determine if it measured traditional ADL, IADL, or HRQL domains. Briefly, we applied the following hierarchical decision rules to selecting functional ability and HRQL outcomes to be abstracted from each study, after conducting an audit of all instruments used to report these outcomes across all included studies. For ADL, IADL, and HRQL, a minimum of 6 months followup was required.

Typical ADLs included bathing, dressing, grooming, toileting, transferring, continence, feeding, mobility, and stairs. One ADL instrument was not carried over to the meta-analysis because we could not find any information indicating it was similar to the other ADL instruments used, specifically, the Chinese version of the Minimum Data Set–Home Care. When given a choice between a lesser known ADL instrument and the SF-36 physical functioning domain, the SF-36 was carried over to the meta-analysis instead of the other instrument. Likewise, when given a choice between the Barthel Index and the SF-36, the Barthel Index was carried over to the meta-analysis as determined by consensus. If a trial used a different instrument (not listed above) to measure functional status, we reviewed that instrument for comparability with other included instruments. As an example, one study[11] used a component of the Sickness Impact Profile (SIP) as a measure of functional ability. Using a priori decision rules, we determined this instrument should represent HRQL. On examination, we determined the physical functioning dimension of the SIP to be much more extensive than the other included measures of ADL and more in line with the physical component score of the SF-36. Therefore, these results were not included in the ADL meta-analyses; however, they are captured in the master evidence table under HRQL.

Typical IADLs include telephoning, shopping, food preparation, housekeeping, laundry, transportation, medications, and finances. When given a choice between a lesser known IADL instrument and the SF-36, the SF-36 was carried over to the meta-analysis instead of the other instrument. Two IADL instruments were not carried over to the meta-analysis because we could not find any information indicating they were similar to the other IADL instruments used: social ADL and the Chinese version of the Minimum Data Set–Home Care (involvement and capacity).

When data on ADL or IADL outcomes seemed to have been measured but not reported, or reported in a format we could not use, we emailed the authors to request the data if the study was published after 2000. Many authors replied with additional data, and we included both published and unpublished data in the meta-analysis. Some trials used instruments that measured combined ADL and IADL. These data were not utilized in the meta-analysis, as deficits in ADL and IADL could not be teased apart from one another, although these combined outcomes were abstracted using the same methodology as for ADL and IADL.

For HRQL, we abstracted only overall or component scores (specifically, mental and physical component scores). If only selected domains (or subscales) were reported, we did not abstract these at the risk of selective reporting bias. Except when we determined that a HRQL tool (as designated by study authors) fit within our schema as a measure of function, we did not limit data abstraction according to the type of HRQL instrument used.

Dichotomous outcomes were also abstracted for ADL, IADL, and HRQL. The method the authors used to report data was collected in the evidence table. No further manipulations of these data were made.

Other outcomes: falls, hospitalizations, institutionalizations, and mortality. The number of fallers, number hospitalized, and number institutionalized at a minimum of 6 months followup and at each time point thereafter for the intervention and control groups were abstracted from each study. If raw numbers were not available, we abstracted any odds ratios or risk ratios. For fallers, we looked for the number of participants who fell over the course of the study and the number analyzed for the intervention and control groups. For institutionalizations, we looked for the number of participants who were institutionalized and the number randomized to the intervention and control groups. The number of participants who died within a minimum of 12 months followup and at each time point thereafter in the intervention and control groups was abstracted from each study. For mortality, we looked for the number of participants who died over the course of the study and the number randomized to the intervention and control groups.

Data Analyses

Clinical heterogeneity. Given the clinical heterogeneity associated with the populations selected and study characteristics, we identified a series of explanatory variables from previous research and from our own observations of variability between studies to explore in synthesizing the results. In addition to the categorizing variables, study purpose, and trial country, we considered other potential explanatory variables, including: mean age of trial population, baseline frailty/risk status of the population, baseline functional status of the control group, control group mortality

rate, applicability of trial to current U.S. setting, comprehensiveness of the management delivered following multifactorial assessment, whether geriatric expertise was used in the assessment and management, intensity of the interventions, year of trial publication, and quality of trial.

To understand the clinical heterogeneity of the populations included in the trials, we grouped trials by health status, frailty, or risk for functional decline of the trial population, using four separate approaches: 1) mean age, 2) a three-level composite measure to approximate frailty (which included age, self-rated health, and loss/dependency of one or more ADLs or IADLs), 3) baseline functional status of the control group (based on ADL and IADL, if reported, and categorized into low, medium, and high tertiles for each instrument used), and 4) control group mortality rate at 12 months. None of these approaches were informative.

To understand the clinical heterogeneity of the interventions evaluated, we grouped trials by applicability to current care in the United States, intervention comprehensiveness, geriatric expertise involvement, and intervention intensity:

1. Applicability of the intervention was based on whether the current health care system had comparable health professionals, mechanism to pay for service, and adequate access to the type of service. Highly applicable interventions used interventions and services currently available in the U.S. health care system and a mechanism to pay for these services. Lowly applicable interventions used health professionals not currently available in the U.S. health care system, services not generally available, and no reimbursement mechanism for the services being delivered.
2. Intervention comprehensiveness was categorized as low, moderate, or high. Low comprehensiveness management included feedback of assessment results to the patient and primary care physician, moderate comprehensiveness included some additional management as part of the intervention (e.g., referrals to specialists), and high comprehensiveness included full management of identified risks during multifactorial assessment as part of the intervention.
3. Geriatric expertise involvement was based on whether a clinician with geriatric expertise (e.g., geriatrician, geriatric nurse practitioner) conducted the assessment and/or was involved in the management.
4. Intensity of the intervention was measured by the number, frequency, and duration of the assessments and management contacts with each participant, as well as the overall duration of the entire intervention.

We also ordered trials by year of publication and grouped trials by quality to determine if these variables had any effect on any of the outcomes. None of these explanatory variables helped explain differences in results when plotted or reduced statistical heterogeneity when examined in meta-regression or through stratified analysis. We therefore undertook a novel approach to categorize studies according to primary intent (intervention focused on preventing functional decline) as opposed to secondary intent (primarily focused on related issues, such as reducing utilization or improving comprehensive health care delivery) and according to setting (United States vs. non-United States). Given the large differences in health care delivery systems and financing between countries, including variation in how much social services are integrated with

medical care, we judged that results for the United States would be most applicable for this review.

To categorize each study's purpose related to functional decline prevention, one investigator considered the stated purpose of the study (i.e., if the stated primary aim was to prevent functional decline or if the population was selected based on their functional status), the outcomes assessed (i.e., were measures of functional ability assessed), and whether the stated primary study outcomes were functional ability measures. At least two of three of these factors categorized a study's primary purpose to be prevention of functional decline. Study purpose categorizations were examined by another investigator and an outside geriatric expert for consistency and accuracy.

Within the strata of study purpose and setting, we also looked at interventions based on authors' descriptions (i.e., comprehensive geriatric assessment, home visit care, primary care redesign, and falls prevention). Studies that did not clearly fit into one of these research streams were described briefly and not grouped (e.g., senior center disability prevention/exercise program, geriatric resource team assisting a health maintenance organization physician to change practice/outcomes, screening/case findings with physician for functional issues prior to physician visit, physical function performance assessments with feedback to the patient/physician). These intervention groupings were not utilized further in the analysis or synthesis of the results.

Quantitative pooling of outcomes. We conducted meta-analyses to summarize data and obtain more precise estimates on outcomes that were reported by trials homogeneous enough to provide a meaningful combined estimate. For all outcomes in the meta-analysis, we pooled data reported for followup time points of 6 to 18 months for the meta-analysis (except mortality, for which we only pooled 12 to 18 months). If two time points were available to pool in the 6 to 18 month range, we preferentially used 12-month data. If a lesser and greater time point from 12 months was available, we preferred the longest followup time. Longer-term followup data, between 24 and 39 months, was pooled for meta-analysis when available. To ensure uniformity among reported outcomes, we undertook multiple data manipulations to compute similar data across trials for each outcome, including maintaining a consistent directionality in functional measures, with higher numbers indicating better function. Continuous (rather than dichotomous) outcomes were most consistently reported for functional outcomes, while other outcomes (e.g., fallers, hospitalizations, institutionalization, and mortality) were primarily dichotomous.

For continuous ADL and IADL outcomes, we only included trials with a primary purpose of preventing functional decline. We abstracted either the mean or the standard deviation at baseline and followup times, the mean change or the standard deviation between baseline and followup times for both the intervention and control groups, or a mean difference in change and standard error between baseline and followup times between the intervention and control group. When the preferred measure of dispersion was not available, it was calculated from the provided information, such as the confidence interval or p value. If only followup mean values from a model adjusted for baseline values of the outcome of interest were reported, they were used as a mean change from baseline. Adjusting for baseline values at followup or subtracting baseline values at followup are both acceptable methods to use to adjust for baseline differences.[12] We performed our meta-analyses using measures adjusted for baseline differences. Additionally, to

ensure uniformity, we determined that a higher score would always indicate a more desirable level of functioning or quality of life for the ADL, IADL, and HRQL outcomes. Therefore, a score needed to be reversed if a higher score indicated poorer functioning. If mean values at baseline and followup were presented for groups with the undesired directionality, then the data were coded in the meta-analysis table to reverse the directionality. If mean change in scores were presented for both groups at followup and the directionality was the undesired direction, then the sign of the mean change was reversed (i.e., a negative sign was added to the change). If a mean difference in change in score was presented and the directionality of the instrument was not in the desired direction, no change was necessary.

ADL and IADL outcomes were combined using the SMD (Hedges' *g* statistic), as they were measured using different instruments. We conducted three separate analyses for these functional ability outcome measures: 1) ADL only, 2) IADL only, and 3) ADL and/or IADL. For the ADL and/or IADL analysis, if a trial reported more than one functional outcome, we chose the combined (ADL/IADL) measure (first choice) over the IADL measure (second choice), and the IADL measure over the ADL measure (third choice). In approximately 50 percent of cases, we had to perform some calculation to incorporate reported data into our meta-analyses. All calculations were double-checked for accuracy. All data were combined using a random effects model.[13] While dichotomous ADL and IADL outcomes were also abstracted, we did not combine them due to the relatively sparse reporting and variation in thresholds for dichotomization. We did not conduct meta-analyses for HRQL outcomes, given the relatively few trials reporting similar HRQL measures.

For binary outcomes (i.e., falls, hospitalizations, institutionalizations, and mortality), the number of events and total sample size for intervention and control groups were abstracted and combined using risk ratio. We used only the most intensive intervention arm in the meta-analyses for trials with multiple intervention arms. All outcomes were combined using a random effects model,[13] except for institutionalizations, for which we used a fixed effects model. For institutionalizations, between-study heterogeneity was estimated to be zero.

Several trials used clustered randomization for both continuous and binary outcomes.[14-21] For these trials, if a study reported an estimate adjusted for clustering effect, we used the reported estimate. If not, we adjusted the clustering effect by multiplying the standard error of the reported estimate by the square root of the design effect. Here, design effect = $1+(m-1)\rho$, where m is the average cluster size and ρ is the intracluster correlation coefficient. No study reported an estimate of ρ, so we assumed a conservative estimate of 0.10 for ρ in the main analysis. We also performed sensitivity analyses assuming a range of plausible values for ρ, which indicated no differences in results.

We assessed the presence of statistical heterogeneity among the trials using standard chi-square tests and the magnitude of heterogeneity was estimated using the I^2 statistic. We used meta-regression to explore heterogeneity and investigate whether the size of effect estimates or heterogeneity were associated with important study-level characteristics (e.g., study purpose, country, or quality). We also explored heterogeneity using forest plots ordered by year of trial publication and control group mortality rate, and stratified analyses by study purpose, country, and quality and intervention applicability and comprehensiveness. Tests of publication bias

(whether the distribution of the effect sizes was symmetric with respect to the precision measure) were performed using funnel plots and Egger's linear regression method[22] when the number of trials was about 10 or more.[23]

All analyses were performed using Stata 10.0 (StataCorp, College Station, TX).

Appendix A Table 1. Search String

1. geriatric assessment.sh. 2. health services for the aged.sh. 3. preventive health services.sh. 4. community health services.sh. 5. community health nursing.sh. 6. home care services.sh. 7. preventive medicine.sh. 8. nursing assessment.sh. 9. disability evaluation.sh. 10. house calls.sh. 11. (house adj5 calls).tw. 12. home-based.tw. 13. (geriatric$ adj5 assess$).tw. 14. (home adj5 intervention$).tw. 15. (home adj5 visit$).tw. 16. (home adj5 assessment$).tw. 17. (preventive adj5 program$).tw. 18. health visitor$.tw. 19. (preventive adj5 care).tw. 20. (health adj5 assessment$).tw. 21. (preventive adj5 medicine).tw. 22. health promotion.sh. (24649) 23. (health adj5 promotion).tw. 24. occupational therapy.sh. 25. (occupation$ adj5 therap$).tw. 26. counseling.sh. 27. psychotherapy.sh. 28. social work.sh. 29. (behavior$ adj5 modif$).tw. 30. Relaxation Therapy/ 31. (behaviour$ adj5 modif$).tw. 32. (behavior$ adj5 therap$).tw. 33. (behaviour$ adj5 therap$).tw. 34. (cognitive adj5 therap$).tw. 35. (relax$ adj5 program$).tw. 36. (social adj5 program$).tw. 37. (social adj5 work$).tw. 38. counseling.tw. 39. counselling.tw. 40. psychotherap$.tw. 41. (physical$ adj5 exercise).tw. 42. (physical$ adj5 fitness).tw. 43. (exercise adj5 program$).tw. 44. (exercise adj5 behavi$).tw. 45. (physical$ adj5 activit$).tw. 46. exercise therapy.sh. 47. physical fitness.sh. 48. walking.sh. 49. tai chi.tw. 50. tai ji.sh. 51. or/1-50	52. activities of daily living.sh. 53. hospitalization.sh. 54. institutionalization.sh. 55. (independent$ adj5 living).tw. 56. (independent$ adj5 life).tw. 57. function$.tw. 58. disabilit$.tw. 59. balance.tw. 60. proprioception.tw. 61. hospitalisation.tw. 62. hospitalization.tw. 63. institutionali$.tw. 64. (activit$ adj5 daily).tw. 65. ADL.tw. 66. (nursing adj5 home).tw. 67. health status.sh. 68. aging.sh. 69. quality of life.sh. 70. aging.tw. 71. locomot$.tw. 72. mobility.tw. 73. (quality adj5 life).tw. 74. or/52-73 75. 51 and 74 76. ("Aged, 80 and over" or Aged).sh. 77. Frail Elderly.sh. 78. elderly.tw. 79. elders.tw. 80. geriatric$.tw. 81. (old adj5 people).tw. 82. or/76-81 83. 75 and 82 84. (clinical trial or controlled clinical trial or randomized controlled trial or meta-analysis).sh. 85. meta-analysis as topic.sh. 86. (clinical trials as topic or controlled clinical trials as topic or randomized controlled trials as topic).sh. 87. (control$ adj3 trial$).tw. 88. random$.tw. 89. clinical trial$.tw. 90. or/84-89 91. 83 and 90 92. exp animal/ not human/ 93. 91 not 92 94. limit 93 to english language 95. limit 94 to yr="2004 - 2010" 96. remove duplicates from 95 97. from 96 keep 1-500

Appendix A Table 2. Inclusion Criteria

Populations	Include	Ambulatory community-dwelling adults age 65 years or older, including those post-hospital or emergency department discharge Studies with samples age 65 years or older on average or studies that present results for adults age 65 years or older separately
	Exclude	Studies limited to persons in nursing homes or care homes, rehabilitation centers, or other long-term care facilities
Settings	Include	Ambulatory care, home-based interventions, primary care, generalizable to U.S. practice, and primary care–referable settings in countries listed as "high" (>0.90) on the United Nations Development Index (Australia, Austria, Belgium, Canada, China, Denmark, Finland, France, Germany, Greece, Hong Kong, Iceland, Ireland, Israel, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Singapore, Slovenia, Spain, Sweden, Switzerland, Taiwan, United Kingdom, and United States)
	Exclude	Hospitals, nursing homes, rehabilitation centers, and other long-term care facilities
Interventions	Include	Multifactorial assessments and management; includes a clinical assessment of two or more domains of functioning, generally supplemented by assessment of disability-related or general geriatric risk factors and/or conditions, with assessment results used as a basis for remedial management Either conducted in a primary care setting or judged to be feasible in primary care or be primary care–referable: • Involve individual-level identification and management of health (and social) problems • Usually involve primary care physicians, other physicians, nurses, nurse practitioners, physician assistants, or related clinical staff (e.g., health educators, other counselors), or the intervention is seen as connected to the health care system by the participant • Individual or small group format (15 people or less, generally does not primarily involve group-level interventions outside the primary care settings or more than 8 group sessions) • Located anywhere, as long as linked to primary care Or, must be primary care–referable, such that the intervention needs to be conducted as part of a health care setting or be widely available for referral in most communities
	Exclude	Community, nonreferral: • Community programs (e.g., senior residence programs) • Social marketing (e.g., media campaigns) • Policy (e.g., local and State public or health policy) Hospital-based and other institutional methods; model of care
Outcomes	Include	Hospitalization, institutionalization, disability (activities of daily living or instrumental activities of daily living), health related quality of life, and death Any harm (adverse effects)
	Exclude	Less than 6 months of followup for outcomes
Study Designs	Include	Randomized, controlled trials Trials must have a control arm with no intervention, minimal intervention, or attention control English language only
	Exclude	Nonrandomized trials, comparative effectiveness trials (i.e., without a control arm), case-control studies, nonsystematic reviews, cohort studies, and observational literature, including editorials, letters, or opinion pieces

Appendix A Table 3. Quality Criteria

Design	USPSTF Quality Rating Criteria[9]	National Institute for Health and Clinical Excellence Methodology Checklists[10]
Systematic reviews and meta-analyses	• Comprehensiveness of sources considered/search strategy used • Standard appraisal of included studies • Validity of conclusions • Recency and relevance are especially important for systematic reviews	• Study addresses an appropriate and clearly focused question • Description of the methodology used is included • Literature search is sufficiently rigorous to identify all the relevant studies • Study quality is assessed and taken into account • Enough similarities between the studies selected to make combining them reasonable
Randomized, controlled trials	• Initial assembly of comparable groups employs adequate randomization, including first concealment and whether potential confounders were distributed equally among groups • Maintenance of comparable groups (includes attrition, crossovers, adherence, contamination) • Important differential loss to followup or overall high loss to followup • Measurements are equal, reliable, and valid (includes masking of outcome assessment) • Clear definition of the interventions • All important outcomes considered	• Study addresses an appropriate and clearly focused question • Assignment of subjects to treatment groups is randomized • Adequate concealment method is used • Subjects and investigators are kept blind about treatment allocation • Treatment and control groups are similar at the start of the trial • Only difference between groups is the treatment under investigation • All relevant outcomes are measured in a standard, valid, and reliable way • Percentage of the individuals or clusters recruited into each treatment arm of the study who dropped out before the study was completed is reported • All the subjects are analyzed in the groups to which they were randomly allocated (often referred to as intention-to-treat analysis) • When the study is carried out at more than one site, results are comparable for all sites

Appendix B Table 1. Evidence Table: Population and Intervention Details for Trials With a Primary Purpose of Preventing Functional Decline

Trial author, year, and quality	Population target, # randomized, and age	Intervention duration	Intervention frequency	Intervention comprehensive, geriatric expertise	Outcomes
U.S. Trials					
Yeo 1987[24] Fair	Older veterans IG: 106; CG: 109 71.5 years	18 months	Assessment: Once Management: Individually tailored	Yes Yes	Total QOL (+) (18 mo) Physical QOL (+) (18 mo) Mental QOL (NS) (18 mo)
Epstein 1990[25] Good	Ambulatory older adults age 75 years or older or 70–74 years and rated as having fair or worse health or very likely or probable deterioration by their PCP IG: 185; CG: 205 76.9 years	12 months	Assessment: Once Management: 3 visits (at least)	No Yes	ADL/IADL (NS) (12 mo) Hospitalization (NS) (12 mo) Institutionalization (NS) (12 mo)
Wagner 1994[26] Fair	Community-dwelling older adult HMO members IG: 635; CG: 607 73 years	Unclear (limited)	Assessment: Once Management: Individually tailored, but not ongoing	No No	ADL (+) (12 mo) ADL (NS) (24 mo) Fallers (+) (12 mo); (NS) (24 mo)
Silverman 1995[27] Fair	Experiencing instability or recent change in health status IG: 239; CG: 203 74.6 years	12 months	Assessment: Once Management: Individually tailored	No Yes	ADL (NS) (12 mo) (OARS, Barthel) Hospitalization (NS) (12 mo) Institutionalization (NS) (12 mo)
Stuck 1995[28] Good	Community-dwelling older adults IG: 215; CG: 199 81.2 years	36 months	Assessment: Repeated annually Management: 12 visits (at least)	Yes Yes	ADL (NS) (36 mo) IADL (+) (36 mo) Hospitalization (NS) (36 mo) Institutionalization (+) (36 mo)
Toseland 1996[29] Fair	Above average users of VAMC outpatient clinics (10+ visits in the previous 12 months) IG: 80; CG: 80 72.15 years	12 months	Assessment: Periodic (not specified) Management: Individually tailored	Yes Yes	ADL/IADL (NS) (16 mo) Hospitalization (NS) (16 mo) Institutionalization (NS) (16 mo)
Leveille 1998[30] Fair	Frail older adults receiving treatment for at least one chronic condition IG: 101; CG: 100 77.1 years	12 months	Assessment: Once Management: 12 visits on average	Yes Yes	ADL (NS) (12 mo) IADL (NS) (12 mo) Hospitalization (NS) (12 mo)
Wallace 1998[31] Fair	Community-dwelling older adults IG: 53; CG: 47 71.9 years	16 weeks (calls); 6 months (exercise)	Assessment: Once Management: 3 calls + 78 exercise classes	No No	ADL (NS) (6 mo) IADL (+) (6 mo)
Coleman 1999[15] Fair	Frail older HMO members at risk for hospitalization and functional decline over the next 4 years IG: 96; CG: 73 77.3 years	24 months	Assessment: Repeated every 3 to 4 months Management: 6 to 8 visits (combined with assessment)	Yes Yes	ADL (NS) (12, 24 mo) Fallers (NS) (12, 24 mo)
Reuben 1999[32] Good	Community-dwelling older adults at risk for functional or health-related QOL decline (failed screening for 1+ conditions) IG: 180; CG: 183 75.9 years	2 weeks	Assessment: Once Management: 1 mailing + 1 call	No Yes	ADL (+) (15 mo) IADL (NS) (15 mo) Physical QOL (+) (15 mo) Mental QOL (NS) (15 mo)

Appendix B Table 1. Evidence Table: Population and Intervention Details for Trials With a Primary Purpose of Preventing Functional Decline

Trial author, year, and quality	Population target, # randomized, and age	Intervention duration	Intervention frequency	Intervention comprehensive, geriatric expertise	Outcomes
Burns 2000[33] Fair	Hospitalized veterans with ADL impairment, chronic disease, polypharmacy, or 2+ hospitalizations in previous year IG: 60; CG: 68 71.2 years	24 months	Assessment: Once Management: Individually tailored	Yes Yes	ADL (NS) (12, 24 mo) IADL (NS) (12, 24 mo) QOL (+) (12, 24 mo)
Boult 2001[11] Fair	Medicare beneficiaries at risk for hospitalization and functional decline IG: 294; CG: 274 78.8 years	6 months	Assessment: Once Management: 6 visits + calls	Yes Yes	Physical QOL (+) (12, 18 mo)
Gill 2002[34] Fair	Frail community-dwelling older adults (physically frail: >10 seconds to walk a 10-foot course and back or cannot stand up from a seated position in a hardback chair with arms folded) IG: 94; CG: 94 83.2 years	12 months	Assessment: Once Management: 16 visits (6 months) + calls (6 months)	Yes No	ADL (+) (12 mo) Institutionalization (NS) (12 mo)
Counsell 2007[16] Fair	Low-income older adults (<200% of federal poverty level) IG: 474; CG: 477 71.7 years	24 months	Assessment: Repeated annually Management: 1 visit + 1 call or visit per month	Yes Yes	ADL (NS) (24 mo) IADL (NS) (24 mo) Physical QOL (NS) (24 mo) Mental QOL (+)(24 mo)
Phelan 2007[19] Fair	Community-dwelling older adults IG: 130; CG: 169 81.5 years	24 months (2 months intensive)	Assessment: Once Management: 1 visit + individually tailored	Yes Yes	ADL (NS) (12, 24 mo)* Hospitalization (NS) (12, 24 mo) * For those without disability at baseline
Non-U.S. Trials					
Vetter 1984[35] Fair Wales	Community-dwelling older adults IG: 350; CG: 324 Age 70 years or older	24 months	Assessment: Repeated annually Management: 2 visits (combined with assessment)	No No	ADL (NS) (24 mo) Fallers (NS) (48 mo)
Carpenter 1990[36] Fair United Kingdom	Community-dwelling older adults IG: 272; CG: 267 75–84 years: 87% 85+ years: 13%	36 months	Assessment: Once Management: 6 visits (no disability) or 12 visits (with disability)	No No	ADL/IADL (NS) (39 mo) Hospitalization (NS) (39 mo)
McEwan 1990[37] Fair United Kingdom	Older adults age 75 years or older IG: 151; CG: 145 NR	Unclear (limited)	Assessment: Once Management: Individually tailored	No No	Summary ADL score data NR
Pathy 1992[38] Fair Wales	Older adults living at home IG: 369; CG: 356 71.2 years	36 months	Assessment: Repeated annually Management: Individually tailored	No No	QOL (NS) (36 mo) Institutionalization (NS) (36 mo)
van Rossum 1993[39] Fair The Netherlands	Community-dwelling older adults IG: 292; CG: 288 75–79 years: 72.6% 80–84 years: 27.4%	36 months	Assessment: Repeated every 3 months + extra if needed Management: 12 visits + extra visits if needed (combined with assessment)	No No	ADL (NS) (18, 36 mo) IADL (NS) (18, 36 mo) Hospitalization (+) (12 mo) Hospitalization (NS) (36 mo)

Appendix B Table 1. Evidence Table: Population and Intervention Details for Trials With a Primary Purpose of Preventing Functional Decline

Trial author, year, and quality	Population target, # randomized, and age	Intervention duration	Intervention frequency	Intervention comprehensive, geriatric expertise	Outcomes
Schrijnemaekers 1995[40] Fair The Netherlands	Frail older adults meeting criteria for fragility IG: 110; CG: 112 77–84 years: 70.3% 85+ years: 29.7%	Unclear (limited)	Assessment: Once Management: Individually tailored	No Yes	ADL (NS) (6 mo) IADL (NS) (6 mo)
Gagnon 1999[41] Fair Canada	Frail older people discharged from hospital ED, required assistance with 1+ ADL or 2+ IADL, at risk for hospital readmission IG: 212; CG: 215 81.6 years	10 months	Assessment: Once Management: 36 visits + 28 calls	Yes Yes	ADL (NS) (10 mo) IADL (NS) (10 mo)
Rockwood 2000[42] Fair Canada	Rural-dwelling frail older persons with concern about community living, recent bereavement, hospitalization or acute illness, frequent physician contact, multiple medical problems, polypharmacy, adverse drug events, functional impairment, or functional decline IG: 95; CG: 87 81.8 years	3 months	Assessment: Once Management: Individually tailored	Yes Yes	ADL (NS) (12 mo) IADL (NS) (12 mo) QOL (NS) (12 mo) Institutionalization (NS) (12 mo)
Stuck 2000[43] Good Switzerland	Community-dwelling older adults IG: 264; CG: 527 81.6 years	24 months	Assessment: Once Management: 8 visits + calls in exceptional cases	Yes Yes	ADL (NS) (36 mo) IADL (+) (36 mo) Institutionalization (NS) (36 mo)
van Haastregt 2000[44] Fair The Netherlands	Community-dwelling older adults with moderate impairments in mobility or a history of recent falls IG: 159; CG: 157 77.2 years	12 months	Assessment: Repeated every 2.5 months Management: 5 visits (combined with assessment)	No No	ADL/IADL (+) (12 mo) ADL/IADL (NS) (18 mo) % Fallers (NS) (12, 18 mo)
Hebert 2001[45] Fair Canada	Community-dwelling older adults at risk for functional decline (at least one positive answer on the Sherbrooke Postal Questionnaire) IG: 250; CG: 253 80.3 years	12 months	Assessment: Once Management: 12 calls	No NR	ADL/IADL (NS) (12 mo) QOL (NS) (12 mo) Institutionalization (NS) (12 mo)
Kono 2004[46] Japan	Ambulatory, housebound, frail older adults needing assistance to live in their own community but not assistance to walk IG: 59; CG: 60 82.7 years	18 months	Assessment: Repeated every 3 months Management: 6 visits (combined with assessment)	No No	ADL (NS) (18 mo) IADL (NS) (18 mo) Hospitalization (NS) (18 mo) Institutionalization (NS) (18 mo)
Chi 2006[14] Fair Hong Kong	Chinese older adults attending the elderly health centers of the Department of Health IG: 472; CG: 453 73.6 years	Unclear (limited)	Assessment: Once Management: Individually tailored	No Yes	ADL (NS) (12 mo) IADL (NS) (12 mo)

Appendix B Table 1. Evidence Table: Population and Intervention Details for Trials With a Primary Purpose of Preventing Functional Decline

Trial author, year, and quality	Population target, # randomized, and age	Intervention duration	Intervention frequency	Intervention comprehensive, geriatric expertise	Outcomes
Bouman 2008[4] Fair The Netherlands	Older adults with poor health status (scored 5 or more out of 10 on the health status questionnaire) IG: 160; CG: 170 75.7 years	18 visits	Assessment: Repeated (8 visits) Management: 8 visits (combined with assessment) and 8 calls	Yes No	ADL (NS) (12, 24 mo) IADL (NS) (12, 24 mo) Hospitalization (NS) (24 mo) Institutionalization (NS) (24 mo)
Melis 2008[47] Fair The Netherlands	Frail older adults living independently with chronic conditions IG: 88; CG: 67 82.2 years	3 months	Assessment: Repeated every 2 weeks Management: 6 visits (combined with assessment)	Yes Yes	ADL/IADL (NS) (6 mo)
Richardson 2008[48] Fair Canada	Community-dwelling older adults IG: 134; CG: 131 73.8 years	18 months	Assessment: Repeated every 6 months Management: 3 visits (combined with assessment)	No No	ADL (NS) (18 mo) IADL (NS) (18 mo)
Li 2010[49] Fair Taiwan	Community-dwelling frail or prefrail older adults (Fried Frailty Criteria) IG: 152; CG:158 78.8 years	6 months	Assessment: Repeated at 6 months Management: Individually tailored	No No	ADL (NS) (6 mo)
Ploeg 2010[50] Good Canada	Older adults at risk for functional decline (Sherbrooke questionnaire) IG: 361; CG:358 81.2 years	12 months	Assessment: Repeated every 6 months Management: Individually tailored	No No	ADL (NS) (12 mo) QOL (NS) (12 mo) Institutionalization (NS) (12 mo)
van Hout 2010[51] Fair The Netherlands	Community-dwelling frail older adults at risk for functional decline (COOP-WONCA charts) IG: 331; CG:320 81.4 years	18 months	Assessment: Repeated at 12 months Management: Individually tailored (at least 4 visits per year)	No No	ADL/IADL (NS) (18 mo) Mental QOL (NS) (18 mo) Physical QOL (NS) (18 mo) Hospitalization (NS) (18 mo) Institutionalizations (NS) (18 mo)

Abbreviations: ADL=activities of daily living; CG=control group; ED=emergency department; HMO=health maintenance organization; IADL=instrumental activities of daily living; IG=intervention group; NR=not reported; NS=not significant; PCP=primary care physician; QOL=quality of life; VAMC=Veterans Affairs Medical Center; (+)=significant association in favor of the intervention group.

Appendix B Table 2. Evidence Table: Population and Intervention Details for Trials With a Primary Purpose Other Than Preventing Functional Decline

Trial author, year, and quality	Population target, # randomized, and age	Intervention duration	Intervention frequency	Intervention comprehensive, geriatric expertise	Outcomes
U.S. Trials					
Tinetti 1994[21] Fair	Older adults at risk for falling IG: 153; CG: 148 77.9 years	6 months	Assessment: Repeated after 4.5 months (2nd visit does not inform intervention) Management: Individually tailored + 3 calls	Yes No	Hospitalizations (NS) (12 mo) Fallers (+) (12 mo)
Morrissey 1995[52] Fair	Medicare enrollees living in the community IG: 954; CG: 960 65–74 years: 60.0% 75+ years: 40.0%	24 months	Assessment: Repeated annually Management: 6 contacts (2 combined with assessments)	No No	QOL (+) (24 mo)
Sommers 2000[20] Fair	Community-dwelling older adults with difficulties living independently IG: 280; CG: 263 77.5 years	24 months	Assessment: Once Management: 17 contacts (at least)	Yes Yes	ADL/IADL (NS) (12, 24 mo – group x year interaction) Hospitalizations (NS) (12 mo) (+) (24 mo) Institutionalizations (NS) (12, 24 mo)
Cohen 2002[53] Fair	Frail older adults who were hospitalized at a VAMC IG: 346; CG: 348 74.2 years	12 months	Assessment: Once Management: Individually tailored	Yes Yes	Institutionalizations (NS) (12 mo)
Martin 2004[54] Fair	Members of a Medicare Plus Choice HMO IG: 4257; CG: 4247 72.9 years	18 months	Assessment: Repeated every 3 months Management: Individually tailored	Yes No	ADL (NS) (18 mo) IADL (NS) (18 mo) Physical QOL (NS) (18 mo) Mental QOL (NS) (18 mo)
Holland 2005[55] Fair	Enrolled in a Medicare managed care plan with one or more chronic diseases IG: 255; CG: 249 73 years	12 months	Assessment: Repeated every 6 months Management: Individually tailored + 12 newsletters	No Yes	ADL/IADL (NS) (12 mo) Hospitalizations (NS) (12 mo)
Alkema 2007[56] Fair	Enrolled in a Medicare managed care plan and high health care utilization in the previous year IG: 2976; CG: 2824 83 years	12 months	Assessment: Once Management: 12 calls	Yes No	None
Mahoney 2007[57] Fair	High risk community-dwelling older adults IG: 174; CG: 175 80.0 years	12 months	Assessment: Once Management: 12 contacts (1 combined with assessment) + 11 calls	No Yes	ADL (NS) (12 mo)
Shumway-Cook 2007[58] Good	Community-dwelling older adults IG: 226; CG: 227 75.6 years	12 months	Assessment: Once Management: 156 exercise classes + 6 education classes (6 months)	No No	Fallers (NS) (12 mo)
Non-U.S. Trials					
Hendriksen 1984[59] Fair Denmark	Community-dwelling older adults IG: 300; CG: 300 78.4 years	36 months	Assessment: Repeated every 3 months Management: 12 contacts (combined with assessment)	No NR	Hospitalizations (NS) (12 mo); (+) (36 mo) Institutionalizations (NS) (12, 36 mo)

Appendix B Table 2. Evidence Table: Population and Intervention Details for Trials With a Primary Purpose Other Than Preventing Functional Decline

Trial author, year, and quality	Population target, # randomized, and age	Intervention duration	Intervention frequency	Intervention comprehensive, geriatric expertise	Outcomes
Vetter 1992[60] Fair Wales	Community-dwelling older adults IG: 350; CG: 324 70+ years	48 months	Assessment: Repeated annually Management: 4 contacts (combined with assessment)	Yes No	Fallers (NS) (48 mo)
Gallagher 1996[61] Fair Canada	Older adults who had fallen in the previous 3 months IG: 50; CG: 50 74.6 years	2 weeks	Assessment: Once (over 3 visits, combined with management) Management: 3 contacts (combined with assessment)	No NR	IADL (NS) (6 mo) QOL (NS) (6 mo)
Bernabei 1998[62] Fair Italy	Frail community-dwelling older adults receiving home health services or home assistance programs IG: 100; CG: 100 81.0 years	12 months	Assessment: Repeated every 2 months Management: Individually tailored	Yes Yes	ADL (+) (12 mo) IADL (+) (12 mo) Hospitalizations (+) (12 mo) Institutionalizations (NS) (12 mo)
Close 1999[63] Fair United Kingdom	Community-dwelling older adults presenting to A&E or ED with a fall IG: 184; CG: 213 78.2 years	12 months	Assessment: Once Management: 1 contact	Yes NR	ADL (+) (12 mo) Hospitalizations (NS) (12 mo) Fallers (+) (12 mo)
Dalby 2000[64] Fair Canada	Frail community-dwelling older adults reporting functional impairment and admission to hospital or bereavement in previous 6 months IG: 73; CG: 69 78.6 years	14 months	Assessment: Once Management: Individually tailored	Yes NR	Institutionalizations (NS) (14 mo)
Hogan 2001[65] Fair Canada	Older adults who had fallen within previous 3 months IG: 79; CG: 84 77.7 years	Unclear (limited)	Assessment: Once Management: At least once	No Yes	Institutionalizations (NS) (12 mo) Fallers (NS) (12 mo)
Newbury 2001[66] Fair Australia	Older adults living independently in their own homes IG: 50; CG: 50 78.5 years	None	Assessment: Once Management: None	No No	ADL (NS) (12 mo) Institutionalizations (NS) (12 mo) Fallers (NS) (12 mo)
Lightbody 2002[67] Fair United Kingdom	Older adults presenting to A&E with a fall IG: 171; CG: 177 75 years (median)	Unclear (limited)	Assessment: Once (combined with management) Management: 1 contact (combined with assessment)	No NR	ADL (+) (6 mo) Fallers (NS) (6 mo)
Yamada 2003[68] Fair Japan	Community-dwelling older adults dependent in IADLs and independent in ADLs IG: 184; CG: 184 78.7 years	18 months	Assessment: Once Management: 7 contacts	No No	QOL (NS) (18 mo)

Appendix B Table 2. Evidence Table: Population and Intervention Details for Trials With a Primary Purpose Other Than Preventing Functional Decline

Trial author, year, and quality	Population target, # randomized, and age	Intervention duration	Intervention frequency	Intervention comprehensive, geriatric expertise	Outcomes
Byles 2004[69] Fair Australia	Community-dwelling older veterans or war widows IG: 942; CG: 627 70+ years	36 months	Assessment: Repeated annually (groups 1 and 2); repeated twice annually (groups 3 and 4) Management: 3 calls (groups 1 and 2); 6 calls (groups 3 and 4)	No No	ADL (NS) (12 mo); (+) (36 mo) IADL (NS) (12, 36 mo) Physical QOL (+) (36 mo) Mental QOL (+) (36 mo) Hospitalizations (NS) (36 mo) Institutionalizations (-) (36 mo)
Caplan 2004[70] Fair Australia	Older adults sent home from the ED IG: 370; CG: 369 82.2 years	4 weeks	Assessment: Once Management: Individually tailored	Yes Yes	ADL (NS) (18 mo) Hospitalizations (+) (18 mo) Institutionalizations (NS) (18 mo)
Dyer 2004[17] Fair United Kingdom	Residential care home residents IG: 102; CG: 94 87.3 years	12–14 weeks	Assessment: Once Management: 37–43 classes + individually tailored	No Yes	Fallers (NS) (12 mo)
Davison 2005[71] Fair United Kingdom	Older adults, recurrent fallers, presenting to A&E with a fall or fall-related injury and had sustained at least 1 additional fall in the preceding year IG: 159; CG: 154 77 years	Unclear (limited)	Assessment: Once Management: 1 contact	Yes NR	Fallers (NS) (12 mo)
Lord 2005[72] Fair Australia	Community-dwelling older adults IG1: 210; IG2: 206; CG: 204 80.4 years	12 months	Assessment: Once Management: 1 contact	Yes NR	Fallers (NS) (12 mo)
Sahlen 2006[73] Fair Sweden	Healthy pensioners IG: 249; CG: 346 79 years	24 months	Assessment: Repeated every 6 months (combined with management) Management: 4 contacts	No No	None
Salminen 2009[74] Fair Finland	Community-dwelling older adults with a fall in previous 12 months IG: 293; CG: 298 73.0 (median for IG)	12 months	Assessment: Once Management: Individually tailored plus exercise	No Yes	Fallers (NS) (12 mo)
Thomas 2007[75] Fair Canada	Community-dwelling older adults living in their own homes or with friends or relatives and receiving informal care from a family member or peer IG1: 175; IG2: 170; CG: 175 80.6 years	48 months	Assessment: Repeated annually (combined with management) Management: 4 contacts (combined with assessment)	No No	Institutionalizations (NS) (48 mo)
Vaapio 2007[76] Fair Finland	Community-dwelling older adults who had fallen in the previous year IG: 293; CG: 298 72.0 (median for IG)	12 months	Assessment: Once Management: 51 contacts (1 combined with assessment)	No Yes	None

Appendix B Table 2. Evidence Table: Population and Intervention Details for Trials With a Primary Purpose Other Than Preventing Functional Decline

Trial author, year, and quality	Population target, # randomized, and age	Intervention duration	Intervention frequency	Intervention comprehensive, geriatric expertise	Outcomes
Elley 2008[77] Good New Zealand	Community-dwelling older adults who had fallen in the past year IG: 155; CG: 157 80.8 years	12 months (exercise) 2–4 weeks (other)	Assessment: Once Management: 6 contact (one combined with assessment)	No Yes	ADL (NS) (12 mo) IADL (NS) (12 mo) ADL/IADL (NS) (12 mo) Physical QOL (NS) (12 mo) Mental QOL (NS) (12 mo) Fallers (NS) (12 mo)
Harari 2008[78] Fair England	Functionally independent community-dwelling older adults IG:1240; CG: 1263 74.5 years	Unclear (limited)	Assessment: Once Management: 1 contact	No No	Hospitalizations (NS) (12 mo)
Hendriks 2008[79] Fair The Netherlands	Community-dwelling older adults who attended the ED after a fall IG: 166; CG: 167 74.9 years	Unclear (limited)	Assessment: Once (over 2 visits) Management: 2 contacts (combined with assessment)	No Yes	ADL (NS) (12 mo) ADL/IADL (NS) (12 mo) QOL (NS) (12 mo) Fallers (NS) (12 mo)
Peri 2008[18] Fair New Zealand	Low-level dependency residential care home residents IG: 73; CG: 76 85 years	6 months	Assessment: Once Management: 11 contacts	No Yes	ADL (NS) (6 mo) IADL (NS) (6 mo) Physical QOL (+) (6 mo) Mental QOL (NS) (6 mo) Fallers (NS) (6 mo)
Vind 2009[80] Good Denmark	Older adults treated for a fall IG: 196; CG: 196 74.4 years	12 months	Assessment: Once Management: Individually tailored	Yes Yes	ADL (NS) (Barthel); (+) (SF-36) (12 mo) ADL/IADL (NS) (12 mo) Fallers (NS) (12 mo)
Hogg 2009[81] Fair Canada	Older adults at risk for experiencing adverse health outcomes IG: 120; CG: 121 71.2 years	18 months	Assessment: Once Management: Individually tailored	No No	IADL (NS) (18 mo) Physical QOL (NS) (18 mo) Mental QOL (NS) (18 mo) Hospitalization (NS) (18 m)
Lam 2010[82] Fair Hong Kong	Community-dwelling older adults with mild dementia IG: 59; CG: 43 78.4 years	4 months	Assessment: Once Management: Individually tailored	Yes Yes	QOL (NS) (12 mo) Institutionalization (NS) (12 mo)
Logan 2010[83] Good United Kingdom	Older adults who called emergency services for fall IG: 102; CG: 102 82.5 years	6 weeks	Assessment: Once Management: Individually tailored with minimum of 6 physical therapy sessions, home hazard modification (mean, 10 sessions), and 12 group sessions on falls prevention	No No	ADL (+) (12 mo) ADL/IADL (+) (12 mo) Fallers (+) (12 mo)

Abbreviations: A&E=Accident and Emergency Department (UK); ADL=activities of daily living; CG=control group; ED=emergency department; HMO=health maintenance organization; IADL=instrumental activities of daily living; IG=intervention group; NR=not reported; NS=not significant; PCP=primary care physician; QOL=quality of life; SF-36=Short-form 36-item Health Survey; VA=Veterans Affairs Medical Center; (+)=significant association in favor of the intervention group.

Appendix C. List of Excluded Studies

1. Anttila SK, Huhtala HS, Pekurinen MJ, Pitkäjärvi TK. Cost-effectiveness of an innovative four-year post-discharge programme for elderly patients—prospective follow-up of hospital and nursing home use in project elderly and randomized controls. *Scand J Public Health*. 2000;28(1):41-6. **Study relevance.**
2. Avlund K, Vass M, Kvist K, Hendriksen C, Keiding N. Educational intervention toward preventive home visitors reduced functional decline in community-living older women. *J Clin Epidemiol.* 2007;60(9):954-62. **Study relevance.**
3. Balaban DJ, Goldfarb NI, Perkel RL, Carlson BL. Follow-up study of an urban family medicine home visit program. *J Fam Pract*. 1988;26(3):307-12. **Study quality.**
4. Bandinelli S, Lauretani F, Boscherini V, Gandi F, Pozzi M, Corsi AM, et al. A randomized, controlled trial of disability prevention in frail older patients screened in primary care: the FRASI study. Design and baseline evaluation. *Aging Clin Exp Res*. 2006;18(5):359-66. **Study design.**
5. Beck A, Scott J, Williams P, Robertson B, Jackson D, Gade G, et al. A randomized trial of group outpatient visits for chronically ill older HMO members: the Cooperative Health Care Clinic. *J Am Geriatr Soc.* 1997;45(5):543-9. **Study relevance.**
6. Béland F, Bergman H, Lebel P, Clarfield AM, Tousignant P, Contandriopoulos AP, et al. A system of integrated care for older persons with disabilities in Canada: results from a randomized controlled trial. *J Gerontol A Biol Sci Med Sci*. 2006;61(4):367-73. **Study relevance.**
7. Béland F, Bergman H, Lebel P, Dallaire L, Fletcher J, Contandriopoulos AP, et al. Integrated services for frail elders (SIPA): a trial of a model for Canada. *Can J Aging*. 2006;25(1):5-42. **Study relevance.**
8. Bergman H, Béland F, Lebel P, Contandriopoulos AP, Tousignant P, Brunelle Y, et al. Care for Canada's frail elderly population: fragmentation or integration? *CMAJ*. 1997;157(8):1116-21. **Study relevance.**
9. Beswick AD, Rees K, Dieppe P, Ayis S, Gooberman-Hill R, Horwood J, et al. Complex interventions to improve physical function and maintain independent living in elderly people: a systematic review and meta-analysis. *Lancet.* 2008;371(9614):725-35. **Study relevance.**
10. Bleijlevens MH, Hendriks MR, van Haastregt JC, van Rossum E, Kempen GI, Diederiks JP, et al. Process factors explaining the ineffectiveness of a multidisciplinary fall prevention programme: a process evaluation. *BMC Public Health*. 2008;8:332. **No relevant outcomes.**
11. Boult C, Reider L, Frey K, Leff B, Boyd CM, Wolff JL, et al. Early effects of "guided care" on the quality of health care for multimorbid older persons: a cluster-randomized controlled trial. *J Gerontol A Biol Sci Med Sci*. 2008;63(3):321-7. **Study relevance.**
12. Bouman A, van Rossum E, Nelemans P, Kempen GI, Knipschild P. Effects of intensive home visiting programs for older people with poor health status: a systematic review. *BMC Health Serv Res*. 2008;8:74. **Study relevance.**
13. Boyd CM, Boult C, Shadmi E, Leff B, Brager R, Dunbar L, et al. Guided care for multimorbid older adults. *Gerontologist*. 2007;47(5):697-704. **Study relevance.**
14. Burton LC, Paglia MJ, German PS, Shapiro S, Damiano AM. The effect among older persons of a general preventive visit on three health behaviors: smoking, excessive alcohol drinking, and sedentary lifestyle. *Prev Med*. 1995;24(5):492-7. **No relevant outcomes.**
15. Calver J, Wiltshire A, Holman CD, Hunter E, Garfield C, Rosman DL. Does health assessment improve health outcomes in indigenous people? An RCT with 13 years of follow-up. *Aust N Z J Public Health*. 2005;29(2):107-11. **Population.**

16. Challis D, Clarkson P, Williamson J, Hughes J, Venables D, Burns A, et al. The value of specialist clinical assessment of older people prior to entry to care homes. *Age Ageing*. 2004;33(1):25-34. **Study design.**
17. Ciaschini PM, Straus SE, Dolovich LR, Goeree RA, Leung KM, Woods CR, et al. Community-based intervention to optimise falls risk management: a randomised controlled trial. *Age Ageing*. 2009;38(6):724-30. **Study quality.**
18. Clark F, Azen SP, Carlson M, Mandel D, LaBree L, Hay J, et al. Embedding health-promoting changes into the daily lives of independent-living older adults: long-term follow-up of occupational therapy intervention. *J Gerontol B Psychol Sci Soc Sci*. 2001;56(1):60-3. **Study relevance.**
19. Clarke M, Clarke SJ, Jagger C. Social intervention and the elderly: a randomized controlled trial. *Am J Epidemiol*. 1992;136(12):1517-23. **Study relevance.**
20. Clemson L, Cumming RG, Kendig H, Swann M, Heard R, Taylor K. The effectiveness of a community-based program for reducing the incidence of falls in the elderly: a randomized trial. *J Am Geriatr Soc.* 2004;52(9):1487-94. **Study relevance.**
21. Counsell SR, Callahan CM, Buttar AB, Clark DO, Frank KI. Geriatric Resources for Assessment and Care of Elders (GRACE): a new model of primary care for low-income seniors. *J Am Geriatr Soc.* 2006;54(7):1136-41. **No relevant outcomes.**
22. Courtney M, Edwards H, Chang A, Parker A, Finlayson K, Hamilton K. Fewer emergency readmissions and better quality of life for older adults at risk of hospital readmission: a randomized controlled trial to determine the effectiveness of a 24-week exercise and telephone follow-up program. *J Am Geriatr Soc.* 2009;57(3):395-402. **No relevant outcomes.**
23. Crotty M, Whitehead C, Miller M, Gray S. Patient and caregiver outcomes 12 months after home-based therapy for hip fracture: a randomized controlled trial. *Arch Phys Med Rehabil*. 2003;84(8):1237-9. **Study relevance.**
24. Cunliffe AL, Gladman JR, Husbands SL, Miller P, Dewey ME, Harwood RH. Sooner and healthier: a randomised controlled trial and interview study of an early discharge rehabilitation service for older people. *Age Ageing*. 2004;33(3):246-52. **Setting.**
25. Cutchin MP, Coppola S, Talley V, Svihula J, Catellier D, Shank KH. Feasibility and effects of preventive home visits for at-risk older people: design of a randomized controlled trial. *BMC Geriatr*. 2009;9:54. **No relevant outcomes.**
26. Daniels R, van Rossum E, de Witte L, Kempen GI, van den Heuvel W. Interventions to prevent disability in frail community-dwelling elderly: a systematic review. *BMC Health Serv Res*. 2008;8:278. **Study relevance.**
27. Dunn RB, Lewis PA, Vetter NJ, Guy PM, Hardman CS, Jones RW. Health visitor intervention to reduce days of unplanned hospital re-admission in patients recently discharged from geriatric wards: the results of a randomised controlled study. *Arch Gerontol Geriatr*. 1994;18(1):15-23. **Setting.**
28. Edwards M. Hospital and home rehabilitation did not differ for functional competence in activities of daily living. *Evid Based Nurs*. 2009;12(3):84. **Study design.**
29. Eekhof J, De Bock G, Schaapveld K, Springer M. Effects of screening for disorders among the elderly: an intervention study in general practice. *Fam Pract*. 2000;17(4):329-33. **No relevant outcomes.**

30. Eklund K, Sjöstrand J, Dahlin-Ivanoff S. A randomized controlled trial of a health-promotion programme and its effect on ADL dependence and self-reported health problems for the elderly visually impaired. *Scand J Occup Ther*. 2008;15(2):68-74. **Study relevance.**
31. Engelhardt JB, Rizzo VM, Della Penna RD, Feigenbaum PA, Kirkland KA, Nicholson JS, et al. Effectiveness of care coordination and health counseling in advancing illness. *Am J Manag Care*. 2009;15(11):817-25. **Population.**
32. Fabacher D, Josephson K, Pietruszka F, Linderborn K, Morley JE, Rubenstein LZ. An in-home preventive assessment program for independent older adults: a randomized controlled trial. *J Am Geriatr Soc.* 1994;42(6):630-8. **Study quality.**
33. Fairhall N, Aggar C, Kurrle SE, Sherrington C, Lord S, Lockwood K, et al. Frailty Intervention Trial (FIT). *BMC Geriatr*. 2008;8:27. **No relevant outcomes.**
34. Finkelstein SM, Speedie SM, Zhou X, Ratner E, Potthoff S. VALUE: Virtual Assisted Living Umbrella for the Elderly—user patterns. *Conf Proc IEEE Eng Med Biol Soc*. 2006;1:3294-6. PMID: **Study relevance.**
35. Fletcher AE, Price GM, Ng ES, Stirling SL, Bulpitt CJ, Breeze E, et al. Population-based multidimensional assessment of older people in UK general practice: a cluster-randomised factorial trial. *Lancet.* 2004;364(9446):1667-77. **Study design.**
36. Fletcher AE, Jones DA, Bulpitt CJ, Tulloch AJ. The MRC trial of assessment and management of older people in the community: objectives, design and interventions. *BMC Health Serv Res*. 2002;2(1):21. **Study design.**
37. Forbes DA. An educational programme for primary healthcare providers improved functional ability in older people living in the community. *Evid Based Nurs*. 2005;8(4):122. **Study design.**
38. Ford AB, Katz S, Downs TD, Adams M. Results of long-term home nursing: the influence of disability. *J Chronic Dis*. 1971;24(9):591-6. **Study relevance.**
39. Fox PJ, Breuer W, Wright JA. Effects of a health promotion program on sustaining health behaviors in older adults. *Am J Prev Med*. 1997;13(4):257-64. **Study design.**
40. Friedman B, Wamsley BR, Liebel DV, Saad ZB, Eggert GM. Patient satisfaction, empowerment, and health and disability status effects of a disease management-health promotion nurse intervention among Medicare beneficiaries with disabilities. *Gerontologist*. 2009;49(6):778-92. **Study quality.**
41. Gates S, Fisher JD, Cooke MW, Carter YH, Lamb SE. Multifactorial assessment and targeted intervention for preventing falls and injuries among older people in community and emergency care settings: systematic review and meta-analysis. *BMJ*. 2008;336(7636):130-3. **Study relevance.**
42. German PS, Burton LC, Shapiro S, Steinwachs DM, Tsuji I, Paglia MJ, et al. Extended coverage for preventive services for the elderly: response and results in a demonstration population. *Am J Public Health*. 1995;85(3):379-86. **Study relevance.**
43. Gitlin LN, Hauck WW, Dennis MP, Winter L, Hodgson N, Schinfeld S. Long-term effect on mortality of a home intervention that reduces functional difficulties in older adults: results from a randomized trial. *J Am Geriatr Soc.* 2009;57(3):476-81. **Study relevance.**
44. Gitlin LN, Winter L, Dennis MP, Hauck WW. Variation in response to a home intervention to support daily function by age, race, sex, and education. *J Gerontol A Biol Sci Med Sci*. 2008;63(7):745-50. **Study relevance.**

45. Graves N, Courtney M, Edwards H, Chang A, Parker A, Finlayson K. Cost-effectiveness of an intervention to reduce emergency re-admissions to hospital among older patients. *PLoS One*. 2009;4(10):e7455. **Study relevance.**
46. Gunner-Svensson F, Ipsen J, Olsen J, Waldstrøm B. Prevention of relocation of the aged in nursing homes. *Scand J Prim Health Care*. 1984;2(2):49-56. **Study relevance.**
47. Hall N. Randomized trial of a health promotion program for frail elders. *Can J Aging*. 1992;11:72-91. **Study design.**
48. Hansen FR, Poulsen H, Sørensen KH. A model of regular geriatric follow-up by home visits to selected patients discharged from a geriatric ward: a randomized controlled trial. *Aging (Milano)*. 1995;7(3):202-6. **Study relevance.**
49. Hansen FR, Spedtsberg K, Schroll M. Geriatric follow-up by home visits after discharge from hospital: a randomized controlled trial. *Age Ageing*. 1992;21(6):445-50. **Study relevance.**
50. Hendriks MR, Evers SM, Bleijlevens MH, van Haastregt JC, Crebolder HF, van Eijk JT. Cost-effectiveness of a multidisciplinary fall prevention program in community-dwelling elderly people: a randomized controlled trial. *Int J Technol Assess Health Care*. 2008;24(2):193-202. **No relevant outcomes.**
51. Hopp F, Woodbridge P, Subramanian U, Copeland L, Smith D, Lowery J. Outcomes associated with a home care telehealth intervention. *Telemed J E Health*. 2006;12(3):297-307. **Study relevance.**
52. Hørdam B, Sabroe S, Pedersen PU, Mejdahl S, Søballe K. Nursing intervention by telephone interviews of patients aged over 65 years after total hip replacement improves health status: a randomised clinical trial. *Scand J Caring Sci*. 2010;24(1):94-100. **Study relevance.**
53. Hornbrook MC, Stevens VJ, Wingfield DJ, Hollis JF, Greenlick MR, Ory MG. Preventing falls among community-dwelling older persons: results from a randomized trial. *Gerontologist*. 1994;34(1):16-23. **Study relevance.**
54. Hughes SL, Weaver FM, Giobbie-Hurder A, Manheim L, Henderson W, Kubal JD, et al; Department of Veterans Affairs Cooperative Study Group on Home-Based Primary Care. Effectiveness of team-managed home-based primary care: a randomized multicenter trial. *JAMA*. 2000;284(22):2877-85. **Study relevance.**
55. Huss A, Stuck AE, Rubenstein LZ, Egger M, Clough-Gorr KM. Multidimensional preventive home visit programs for community-dwelling older adults: a systematic review and meta-analysis of randomized controlled trials. *J Gerontol A Biol Sci Med Sci*. 2008;63(3):298-307. **Study relevance.**
56. Jitapunkul S. A randomised controlled trial of regular surveillance in Thai elderly using a simple questionnaire administered by non-professional personnel. *J Med Assoc Thai*. 1998;81(5):352-6. **Setting.**
57. Kerse NM, Flicker L, Jolley D, Arroll B, Young D. Improving the health behaviours of elderly people: randomised controlled trial of a general practice education programme. *BMJ*. 1999;319(7211):683-7. **Study relevance.**
58. Kingston P, Jones M, Lally F, Crome P. Older people and falls: a randomized controlled trial of a health visitor (HV) intervention. *Rev Clin Gerontol*. 2001;11(3):209-14. **Study relevance.**
59. Kirchberger I, Meisinger C, Seidl H, Wende R, Kuch B, Holle R. Nurse-based case management for aged patients with myocardial infarction: study protocol of a randomized controlled trial. *BMC Geriatr*. 2010;10(1):29. **Population.**

60. Kircher TT, Wormstall H, Müller PH, Schwärzler F, Buchkremer G, Wild K, et al. A randomised trial of a geriatric evaluation and management consultation services in frail hospitalised patients. *Age Ageing.* 2007;36(1):36-42. **Setting.**
61. Kono A, Fujita T, Tsumura C, Kondo T, Kushiyama K, Rubenstein LZ. Preventive home visit model targeted to specific care needs of ambulatory frail elders: preliminary report of a randomized trial design. *Aging Clin Exp Res*. 2009;21(2):167-73. **No relevant outcomes.**
62. Kronborg C, Vass M, Lauridsen J, Avlund K. Cost effectiveness of preventive home visits to the elderly: economic evaluation alongside randomized controlled study. *Eur J Health Econ*. 2006;7(4):238-46. **Study design.**
63. Lamb SE. Multidisciplinary assessment of elderly people with a history of multiple falls reduces the risk of further falls. *Aust J Physiother*. 2009;55(2):139. **Study design.**
64. Latour CH, Bosmans JE, van Tulder MW, de Vos R, Huyse FJ, de Jonge P, et al. Cost-effectiveness of a nurse-led case management intervention in general medical outpatients compared with usual care: an economic evaluation alongside a randomized controlled trial. *J Psychosom Res*. 2007;62(3):363-70. **No relevant outcomes.**
65. Latour CH, de Vos R, Huyse FJ, de Jonge P, van Gemert LA, Stalman WA. Effectiveness of post-discharge case management in general-medical outpatients: a randomized, controlled trial. *Psychosomatics*. 2006;47(5):421-9. **No relevant outcomes.**
66. Leff B, Reider L, Frick KD, Scharfstein DO, Boyd CM, Frey K, et al. Guided care and the cost of complex healthcare: a preliminary report. *Am J Manag Care*. 2009;15(8):555-9. **Study relevance.**
67. Leung AC, Liu C, Chow NW, Chi I. Cost-benefit analysis of a case management project for the community-dwelling frail elderly in Hong Kong. *J Appl Gerontol*. 2004;23:70-85. **Study quality.**
68. Lewin G, Vandermeulen S. A non-randomised controlled trial of the Home Independence Program (HIP): an Australian restorative programme for older home-care clients. *Health Soc Care Community*. 2010;18(1):91-9. **Study design.**
69. Littbrand H, Lundin-Olsson L, Gustafson Y, Rosendahl E. The effect of a high-intensity functional exercise program on activities of daily living: a randomized controlled trial in residential care facilities. *J Am Geriatr Soc.* 2009;57(10):1741-9. **Setting.**
70. Martin F, Oyewole A, Moloney A. A randomized controlled trial of a high support hospital discharge team for elderly people. *Age Ageing*. 1994;23(3):228-34. **Setting.**
71. Melin AL, Bygren LO. Efficacy of the rehabilitation of elderly primary health care patients after short-stay hospital treatment. *Med Care*. 1992;30(11):1004-15. **Study design.**
72. Melin AL, Håkansson S, Bygren LO. The cost-effectiveness of rehabilitation in the home: a study of Swedish elderly. *Am J Public Health*. 1993;83(3):356-62. **Study design.**
73. Melis RJ, van Eijken MI, van Achterberg T, Teerenstra S, Vernooij-Dassen MJ, van de Lisdonk EH, et al. The effect on caregiver burden of a problem-based home visiting programme for frail older people. *Age Ageing.* 2009;38(5):542-7. **No relevant outcomes.**
74. Meng H, Friedman B, Wamsley BR, Mukamel D, Eggert GM. Effect of a consumer-directed voucher and a disease-management-health-promotion nurse intervention on home care use. *Gerontologist*. 2005;45(2):167-76. **No relevant outcomes.**
75. Meng H, Wamsley B, Liebel D, Dixon D, Eggert G, Van Nostrand J. Urban-rural differences in the effect of a Medicare health promotion and disease self-management program on physical function and health care expenditures. *Gerontologist*. 2009;49(3):407-17. **Study relevance.**

76. Naylor MD, Brooten D, Campbell R, Jacobsen BS, Mezey MD, Pauly MV, et al. Comprehensive discharge planning and home follow-up of hospitalized elders: a randomized clinical trial. *JAMA*. 1999;281(7):613-20. **Setting.**
77. Newcomer R, Maravilla V, Faculjak P, Graves MT. Outcomes of preventive case management among high-risk elderly in three medical groups: a randomized clinical trial. *Eval Health Prof*. 2004;27(4):323-48. **Study relevance.**
78. Nikolaus T, Specht-Leible N, Bach M, Oster P, Schlierf G. A randomized trial of comprehensive geriatric assessment and home intervention in the care of hospitalized patients. *Age Ageing*. 1999;28(6):543-50. **Setting.**
79. Nikolaus T, Bach M. Preventing falls in community-dwelling frail older people using a home intervention team (HIT): results from the randomized Falls-HIT trial. *J Am Geriatr Soc.* 2003;51(3):300-5. **Setting.**
80. Peeters GM, de Vries OJ, Elders PJ, Pluijm SM, Bouter LM, Lips P. Prevention of fall incidents in patients with a high risk of falling: design of a randomised controlled trial with an economic evaluation of the effect of multidisciplinary transmural care. *BMC Geriatr*. 2007;7:15. **No relevant outcomes.**
81. Rubenstein LV, Calkins DR, Young RT, Cleary PD, Fink A, Kosecoff J, et al. Improving patient function: a randomized trial of functional disability screening. *Ann Intern Med*. 1989;111(10):836-42. **Population.**
82. Rubenstein LZ, Alessi CA, Josephson KR, Trinidad Hoyl M, Harker JO, Pietruszka FM. A randomized trial of a screening, case finding, and referral system for older veterans in primary care. *J Am Geriatr Soc.* 2007;55(2):166-74. **Study design.**
83. Rubin CD, Sizemore MT, Loftis PA, de Mola NL. A randomized, controlled trial of outpatient geriatric evaluation and management in a large public hospital. *J Am Geriatr Soc.* 1993;41(10):1023-8. **Study relevance.**
84. Scholes D, LaCroix AZ, Wagner EH, Grothaus LC, Hecht JA. Tracking progress toward national health objectives in the elderly: what do restricted activity days signify? *Am J Public Health*. 1991;81(4):485-8. **No relevant outcomes.**
85. Scott JC, Conner DA, Venohr I, Gade G, McKenzie M, Kramer AM, et al. Effectiveness of a group outpatient visit model for chronically ill older health maintenance organization members: a 2-year randomized trial of the cooperative health care clinic. *J Am Geriatr Soc.* 2004;52(9):1463-70. **Study relevance.**
86. Shapiro A, Taylor M. Effects of a community-based early intervention program on the subjective well-being, institutionalization, and mortality of low-income elders. *Gerontologist*. 2002;42(3):334-41. **Study quality.**
87. Shaw FE, Bond J, Richardson DA, Dawson P, Steen IN, McKeith IG, et al. Multifactorial intervention after a fall in older people with cognitive impairment and dementia presenting to the accident and emergency department: randomised controlled trial. *BMJ*. 2003;326(7380):73. **Population.**
88. Shyu YI, Liang J, Wu CC, Su JY, Cheng HS, Chou SW, et al. A pilot investigation of the short-term effects of an interdisciplinary intervention program on elderly patients with hip fracture in Taiwan. *J Am Geriatr Soc.* 2005;53(5):811-8. **Setting.**
89. Sørensen KH, Sivertsen J. Follow-up three years after intervention to relieve unmet medical and social needs of old people. *Compr Gerontol B*. 1988;2(2):85-91. **Study quality.**

90. Spice CL, Morotti W, George S, Dent TH, Rose J, Harris S, et al. The Winchester falls project: a randomised controlled trial of secondary prevention of falls in older people. *Age Ageing.* 2009;38(1):33-40. **Study design.**
91. Steinberg M, Cartwright C, Peel N, Williams G. A sustainable programme to prevent falls and near falls in community dwelling older people: results of a randomised trial. *J Epidemiol Community Health*. 2000;54(3):227-32. **No relevant outcomes.**
92. Stewart S, Harvey I, Poland F, Lloyd-Smith W, Mugford M, Flood C. Are occupational therapists more effective than social workers when assessing frail older people? Results of CAMELOT, a randomised controlled trial. *Age Ageing*. 2005;34(1):41-6. **Study design.**
93. Stewart S, Pearson S, Luke CG, Horowitz JD. Effects of home-based intervention on unplanned readmissions and out-of-hospital deaths. *J Am Geriatr Soc.* 1998;46(2):174-80. **Study relevance.**
94. Stock R, Mahoney ER, Reece D, Cesario L. Developing a senior healthcare practice using the chronic care model: effect on physical function and health-related quality of life. *J Am Geriatr Soc.* 2008;56(7):1342-8. **Study design.**
95. Stuck AE, Siu AL, Wieland GD, Adams J, Rubenstein LZ. Comprehensive geriatric assessment: a meta-analysis of controlled trials. *Lancet.* 1993;342(8878):1032-6. **Study relevance.**
96. Stuck AE, Kharicha K, Dapp U, Anders J, von Renteln-Kruse W, Meier-Baumgartner HP, et al. Development, feasibility and performance of a health risk appraisal questionnaire for older persons. *BMC Med Res Methodol*. 2007;7:1. **Study relevance.**
97. Tinetti ME, Baker DI, Gottschalk M, Williams CS, Pollack D, Garrett P, et al. Home-based multicomponent rehabilitation program for older persons after hip fracture: a randomized trial. *Arch Phys Med Rehabil*. 1999;80(8):916-22. **Study relevance.**
98. Townsend J, Piper M, Frank AO, Dyer S, North WR, Meade TW. Reduction in hospital readmission stay of elderly patients by a community based hospital discharge scheme: a randomised controlled trial. *BMJ*. 1988;297(6647):544-7. **Study relevance.**
99. Trentini M, Semeraro S, Motta M. Effectiveness of geriatric evaluation and care: one-year results of a multicenter randomized clinical trial. *Aging (Milano).* 2001;13(5):395-405. **Study relevance.**
100. Tulloch AJ, Moore V. A randomized controlled trial of geriatric screening and surveillance in general practice. *J R Coll Gen Pract*. 1979;29(209):733-40. **Study quality.**
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Appendix D. Inclusion and Exclusion of Trials From Relevant Systematic Evidence Reviews

Trial Publications	Beswick 2008*[3]	Bouman 2008*[84]	Huss 2008*[6]	Stuck 2002[85]	Elkan 2001[86]	Van Haastregt 2000[87]	Stuck 1993*[7]	Status of article: Included (X) or Reason for Exclusion
Alkema 2007[56]								X
Allen 1986[88]							X	Intervention not conducted in primary care or other similar setting
Applegate 1990[89]							X	Intervention not conducted in primary care or other similar setting
Archbold 1995[90]					X			Design not RCT
Balaban 1988[91]	X				X			Problems with baseline comparability between groups
Beck 1997[92]	X							Not a clinical assessment and management intervention
Bernabei 1998[62]	X							X
Boult 2001[11]	X							X
Burns 1995[93] Burns 2000[33]	X							X
Burton 1995[94]	X							No relevant outcomes
Byles 2004[69]	X		X					X
Caplan 2004[70]	X							X
Carpenter 1990[36]	X		X	X		X	X	X
Chi 2006[14]								X
Clark 1997[95]	X							Not a clinical assessment and management intervention
Clarke 1992[96]	X			X				Not a clinical assessment and management intervention
Clemson 2004[97]	X							Not a clinical assessment and management intervention
Close 1999[63]	X							X
Cohen 2002[53] Phibbs 2006[98]								X
Coleman 1999[15]	X							X
Counsell 2007[16]								X
Crotty 2003[99]	X							Not a clinical assessment and management intervention
Cunliffe 2004[100]	X							Intervention not conducted in primary care or other similar setting
Dalby 2000[64]	X	X						X
Davison 2005[71]	X							X
Dunn 1994[101]	X				X			Intervention not conducted in primary care or other similar setting
Dyer 2004[17]								X
Eekhof 2000[102]	X							No relevant outcomes
Elley 2008[77]								X
Engelhardt 1996[103] Toseland 1996[29]	X							X
Epstein 1990[25]	X						X	X
Fabacher 1994[104]	X		X	X	X	X		Outcome assessment unblinded
Fletcher 2004[105]	X							Design not RCT
Ford 1971[106]	X							Requires integrated inpatient and outpatient approach
Fox 1997[107]	X							Design not RCT
Fretwell 1990[108]							X	Intervention not conducted in primary care or other similar setting

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Gagnon 1999[41]	X							X
Gallagher 1996[61]	X							X
Gayton 1987[109]							X	Design not RCT
Gilchrist 1988[110]							X	Intervention not conducted in primary care or other similar setting
Gill 2001[111] Gill 2002[34] Gill 2004[112]	X							X
Gunner-Svensson 1984[113]	X		X	X				Not a clinical assessment and management intervention
Hall 1992[114]	X	X			X	X		Design not RCT
Hansen 1992[115]	X				X		X	Not a clinical assessment and management intervention
Hansen 1995[116]	X							Requires integrated inpatient and outpatient approach
Harris 1991[117]							X	Intervention not conducted in primary care or other similar setting
Hebert 2001[45]	X		X	X				X
Hendriks 2008[79]								X
Hendriksen 1984[59]	X		X	X	X	X	X	X
Hogan 1987[118]							X	Intervention not conducted in primary care or other similar setting
Hogan 1990[119]							X	Intervention not conducted in primary care or other similar setting
Hogan 2001[65]	X							X
Hogg 2009[120] Dahrouge 2010[121]								X
Holland 2005[55]								X
Hornbrook 1994[122]	X							Not a clinical assessment and management intervention
Hughes 2000[123]	X							Requires integrated inpatient and outpatient approach
Jitapunkul 1998[124]	X							Intervention not conducted in primary care or other similar setting
Kennie 1988[125]							X	Intervention not conducted in primary care or other similar setting
Kerse 1999[126]	X							Not a clinical assessment and management intervention
Kono 2004[46]	X		X					X
Lam 2010[82]								X
Leung 2004[127]	X							Poor reporting
Leveille 1998[30] Phelan 2004[128]	X							X
Li 2010[129]								X
Lightbody 2002[67]	X							X
Logan 2010[130]								X
Lord 2005[72]	X							X
Luker 1981[131]						X		Design not RCT
Luker 1982[132]					X			Intervention not conducted in primary care or other similar setting
Mahoney 2007[57]			X					X
Martin 1994[133]	X							Intervention not conducted in primary care or other similar setting
Martin 2004[54]								X

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Trial Publications	Beswick 2008*[3]	Bouman 2008*[84]	Huss 2008*[6]	Stuck 2002[85]	Elkan 2001[86]	Van Haastregt 2000[87]	Stuck 1993*[7]	Status of article: Included (X) or Reason for Exclusion
McEwan 1990[37]	X			X	X	X		X
Melin 1992[134]	X						X	Design not RCT
Melis 2005[135] Melis 2008[47] Melis 2008[136]			X					X
Morrissey 1995[52]	X							X
Naylor 1999[137]	X							Intervention not conducted in primary care or other similar setting
Newbury 2001[66]	X			X				X
Newcomer 2004[138]	X							Not a clinical assessment and management intervention
Nicolaides-Bouman 2004[139] Bouman 2008[4] Bouman 2008[140]		X	X					X
Nikolaus 1999[141]	X							Intervention not conducted in primary care or other similar setting
Nikolaus 2003[142]	X							Intervention not conducted in primary care or other similar setting
Oktay 1990[143]					X			Design not RCT
Pathy 1992[38]	X		X	X	X	X	X	X
Peri 2008[18]								X
Phelan 2007[19]								X
Ploeg 2010[144]								X
Powell 1990[145]							X	Intervention not conducted in primary care or other similar setting
Reuben 1999[32]	X							X
Richardson 2008[48]								X
Rockwood 2000[42]	X							X
Rubenstein 1984[146]							X	Intervention not conducted in primary care or other similar setting
Rubenstein 1989[147]	X							Conducted in a population that does not have an average age of 65 years and older
Rubenstein 1994[148] Stuck 1995[28] Bula 1999[149]	X		X	X	X	X		X
Rubin 1992[150]							X	Intervention not conducted in primary care or other similar setting
Rubin 1993[151]	X							Requires integrated inpatient and outpatient approach
Sahlen 2006[73] Sahlen 2008[152]	X		X					X
Salminen 2009[74] Salminen 2009[153]								X
Schrijnemaekers 1995[40]	X							X
Scott 2004[154]	X							Not a clinical assessment and management intervention
Shapiro 2002[155]	X							High or differential attrition
Shumway-Cook 2007[58]								X
Silverman 1995[27]	X							X
Sommers 2000[20]	X							X
Sorensen 1988[156]	X			X		X	X	Poor reporting

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Trial Publications	Beswick 2008*[3]	Bouman 2008*[84]	Huss 2008*[6]	Stuck 2002[85]	Elkan 2001[86]	Van Haastregt 2000[87]	Stuck 1993*[7]	Status of article: Included (X) or Reason for Exclusion
Steinberg 2000[157]	X							No relevant outcomes
Stewart 1998[158]	X							Requires integrated inpatient and outpatient approach
Stewart 2005[159]	X							Design not RCT
Stuck 1995 [160] Stuck 2000[43]	X	X	X	X				X
Stuck 2007[161] Harari 2008[78]								X
Teasdale 1983[162]							X	Design not RCT
Thomas 1993[163]							X	Intervention not conducted in primary care or other similar setting
Thomas 2007[75]								X
Tinetti 1994[21]	X		X	X		X		X
Tinetti 1999[164]	X							Not a clinical assessment and management intervention
Townsend 1988[165]	X							Requires integrated inpatient and outpatient approach
Trentini 2001[166]	X							Requires integrated inpatient and outpatient approach
Tulloch 1979[167]	X						X	Poor reporting
Vaapio 2007[76] Sjösten 2007 [168] Sjösten 2007[169]			X					X
van Haastregt 2000[44]	X	X	X	X				X
van Hout 2005[170]		X						No relevant outcomes
van Hout 2010[51]								X
van Rossum 1993[39] van Rossum 1993[171]	X	X	X	X	X	X		X
Vass 2004[172]	X							Design not RCT
Vetter 1984[35]	X		X	X	X	X	X	X
Vetter 1992[60]	X		X	X		X	X	X
Vind 2009[173]								X
Wagner 1994[26]	X					X		X
Wallace 1998[31]	X							X
Williams 1987[174]	X						X	No relevant outcomes
Williams 1992[175]	X				X			Requires integrated inpatient and outpatient approach
Winograd 1991[176]							X	Intervention not conducted in primary care or other similar setting
Yamada 2003[68]		X						X
Yeo 1987[24]	X						X	X
Zimmer 1985[177]	X							Conducted in a population that is not comparable to primary care
Total Trials	**87**	**8**	**20**	**17**	**15**	**14**	**27**	**70**
Total Unique Trials	**34**	**1**	**0**	**0**	**3**	**1**	**15**	**23**

* Used as a source document

Abbreviations: RCT=randomized, controlled trial.

Appendix E. Appendix References

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