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The champion for improved delivery of care to older people in long-term care settings: effects on professional practice, quality of care and resident outcomes (Protocol)

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[Intervention Protocol]

The champion for improved delivery of care to older people in long-term care settings: effects on professional practice, quality of care and resident outcomes

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ABSTRACT

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

To assess the effectiveness of the champion on professional practice, quality of care and resident outcomes in long-term care for older people.

BACKGROUND

Description of the condition

How to deliver high-quality, affordable long-term care for the emerging population of older people who require it has become one of the defining challenges for policy makers and service providers worldwide.

In some developed countries (e.g. Scotland, Australia, Sweden and Denmark), government policy supports efforts towards 'ageing in place', which enable older people to remain independent as long as possible (Australian Government 2013; Genet 2009; Scottish Government 2014). However, this means that, in many cases, people are being admitted into long-term care at a more advanced age than in previous years, in the most frail and dependent conditions, and with complex healthcare needs (Katz 2009). This is creating a shift within the sector from a previous focus on social care to one where older people living in care homes are likely to suffer from multiple morbidities, often over an extended period (Bowman 2014).

Unlike acute settings, it is rare for long-term care facilities to be staffed by well-qualified interdisciplinary clinical personnel, and access to external healthcare provision is often limited. In two-thirds of the countries which responded to a recent World Health Organization/International Association of Gerontology and Geriatrics global survey, physicians are rarely involved in medical direction, and in one-sixth of the respondent countries they are never involved (Tolson 2013). In the United Kindgom (UK), although there are almost three times as many beds in long-term care as within the National Health Service (NHS) (Laing 2013), only 39% of these beds are in facilities with qualified nurses on site (Froggatt 2009). Staff retention is particularly challenging,

as working within this sector is afforded low status compared to the higher-valued health sector (Tolson 2011) and there is a lack of well-developed career progression pathways (Cavendish 2013). The impact of staffing models on quality of care has been examined in a number of reviews which focus on this sector (Hodgkinson 2011; Spilsbury 2011). Because of the concerns over extrapolating evidence from acute and clinical sectors to long-term care of older people, organisations such as the National Institute for Health and Care Excellence have recently begun to develop guidance and quality standards specifically for the social care sector (NICE 2013a) . Despite the increasing clinical skill requirements posed by the vulnerability of the care home population, long-term care of older people "remains a low-wage economy" (Scottish Government and COSLA 2014) and significant increases in education and training budgets, either within the mainly for-profit sector or from external clinical education providers, are unlikely in the short to medium term. Within the current funding climate, educating long-term care staff in the clinical areas of care relevant to this sector is not a feasible option (Lee 2009). In an attempt to address the need for quality improvement, other models of intervention have been trialled (Ouslander 2007), one of which is to designate an individual staff member to be a topic 'champion'.

Description of the intervention

In areas of care which often prove resistant to improvement (e.g. oral health (Bassim 2008), nutrition (Gaskill 2009), continence care (Ouslander 2007), delirium (Siddiqi 2008), and infection control (Damschroder 2009)) one model of intervention being increasingly adopted is that of the 'champion'.

Outside long-term care, the term 'champion' is often adopted to characterise a number of diverse categories of roles, groups or individuals whose goal is to effect positive change (Shaw 2012; Soo 2009) and often refers to influential and often charismatic individuals with high social and institutional status (Shetty 2013). Because of its positive connotations, it is now used in a wide range of settings (Clarkson 2009; Jenkins 2014; Temoka 2013) leading to a level of conceptual confusion. In long-term care, however, it is most commonly used to denote a care home staff member who has either been designated or who volunteers to take on an additional level of responsibility in a particular topic area. Although 'champion' is the commonly-used term for this role in the UK, others such as aide (Bassim 2008), organiser (de Visschere 2010), or coordinator (Pronych 2010) can and do reflect similar roles.

Studies suggest that having at least one on-site staff member (normally a care assistant, healthcare worker, nursing auxiliary or nurse) who has undertaken some form of supplementary training in a specific topic area will help improve the practice in that area, the quality of care and the health outcomes for the residents (Damschroder 2009, Gaskill 2009, Lee 2009, Nicol 2005, Shaw 2012, Siddiqi 2008, Wardh 2002, Wardh 2003). They are likely to be a key contact person within the care home for the topic area (e.g. the dental team). In their realist review, Goodman 2015 suggests that having a nominated champion on particular health topics among care home staff being allocated to work with external healthcare providers is an essential attribute to 'relational working'.

Having a nominated champion for a certain aspect of care may be part of a multi-factor intervention (e.g. training for other staff, on-going support from health provider organisation, liaison link with health teams (e.g. dieticians, dental professionals)) or be the single element of an intervention.

How the intervention might work

Evidence-based educational interventions which focus on treating or alleviating common health problems experienced by people in long-term care are necessary but often not sufficient to change staff behaviour (Coleman 2006; Ouslander 2007). Evaluations often show, at best, short-term gains (Boczko 2009; Ouslander 2007). Sustainable change, especially in areas which are perceived as low priority, often proves elusive (Simons 2000) or resource intensive (Nicol 2005).

Wardh and colleagues (Wardh 2002) suggest that designating one or more staff members within a long-term care facility as champions, and giving these individuals additional training and responsibility for a specific topic area, (in this case, oral health), will result in the specially-trained staff member gaining the competence necessary to manage topic-related issues, liaise with the primary care team and encourage appropriate referrals in addition to overseeing the practice of colleagues.

Financial and time pressures together with regulations requiring specific staffing ratios can discourage managers from releasing staff to undertake non-mandatory education. Having an internal, specially-trained staff member may lead to less disruptive scheduling of training delivery, and allow for improved monitoring of the daily activities of the care staff (MacEntee 2007). An Australian clinical trial looking at nutrition coordinators in long-term care (Gaskill 2009) also found that staff were more likely to attend in-service sessions than those sessions more formally arranged by external providers. Although the findings of this study (i.e. improvements in nutritional status) were not statistically significant, additional training, raised awareness, responsibility, contact and visits helped contribute to improvements.

Why it is important to do this review

Best-evidence guidance for providing care to older people in long-term care settings exists for dementia (NICE 2010; Scottish Government 2011), nutrition (Dietitians of Canada 2013), oral health (NHS Health Scotland 2013), and falls (NICE 2013b). However an evidence-based approach to the implementation of guidelines is often lacking, and in other settings studies have shown that in order to achieve sustained healthcare improvements, guid-

ance recommendations have to be embedded into routine clinical practice (Grimshaw 2004). One way of achieving this is through the appointment of a champion in a particular field of care to enable/facilitate implementation of guidelines (de Visschere 2010; Siddiqi 2008). Evaluations have been carried out on both standalone (Bassim 2008; Wardh 2003) and multi-component programmes (Siddiqi 2008) but little synthesis of the evidence for the effectiveness of champions for improving staff practice, quality of care and resident health outcomes within long-term care settings has been carried out.

OBJECTIVES

To assess the effectiveness of the champion on professional practice, quality of care and resident outcomes in long-term care for older people.

METHODS

Criteria for considering studies for this review

Types of studies

We will include randomised controlled trials (RCTs) and cluster RCTs as these are considered the gold-standard study design to assess the effectiveness of an intervention. However, due to the context in which the intervention may be implemented (over time, within long-term care settings and appointing existing staff as champions), randomisation may not be feasible. If there are not enough of these studies, we will include non-randomised trials (NRTs). We will include controlled before-after (CBA) studies that assess outcomes prior to and after implementation in the intervention group and in the control group (where no intervention takes place). We will include interrupted time series (ITS) studies where outcomes are assessed over time, prior to and after the implementation of the intervention, controlling for any underlying secular trends. We will include repeated measures studies (RMS) where outcomes are measured repeatedly on the same individuals over time, prior to and after the implementation of the intervention.

We will use the following criteria for inclusion of CBA studies: (i) duration of pre- and post-intervention period should be comparable, (ii) baseline characteristics of intervention and control groups should be similar, and (iii) there should be at least two control and two intervention sites.

We will use the following criteria for inclusion of ITS studies: (i) a clearly-defined point in time when the intervention occurred, and (ii) at least three data points before and after the intervention.

Types of participants

Participants in this review will be staff working in, and older people residing in, long-term care facilities designated as providing personal and/or nursing care for older people. We will include studies in which the majority of residents are aged 65 or over.

Types of interventions

We will consider all forms of interventions aimed at changing staff behaviour with regard to their practice, the quality of care and resident outcomes, which have as a component an identified champion (as described above). We will exclude studies where the designated champion (or other relevant terms) is not an internal staff member of the care facility, or where the role does not involve the described elements, i.e. supplementary training, assumes additional responsibility for a specific topic area, and/or acts as key contact person with external healthcare team.

We will also exclude any studies in which the intervention involves a 'champion' where the term is used to refer to an external, high-level, educationally-influential opinion leader such as those described in Flodgren 2011. Although the same term may be used to refer to such individuals, they are likely to be physicians and clinicians with higher social status, their leadership emerges rather than being a designated responsibility and their influence is considered to be related to influential communication networks and, as such, the role is not comparable to the care home champion as defined in this protocol.

We will compare the intervention to the following comparator groups:

1. No intervention (standard care), or
2. Single intervention (no champion), or
3. Multifaceted intervention (no champion).

Types of outcome measures

Primary outcomes

1. Staff adherence to recommended practice or guidelines
2. Staff change of behaviour associated with practice/ guidelines (e.g. twice daily teeth/denture cleaning)

Secondary outcomes

1. Resident objective health outcomes, depending on the focus of the intervention (e.g. delirium levels, continence, tissue viability, nutritional status, oral health status, emergency hospital admissions)
2. Resident quality of life (validated tools only)
3. Resident adverse health events (e.g. other than health outcome of interest- as a result of intervention focusing on one area of health, other areas of health are given lower priority, such

as delirium, dehydration, tissue viability, continence, nutritional status, oral health)

4. Resident satisfaction with care

Search methods for identification of studies

The review authors will develop a search strategy in collaboration with the Cochrane Effective Practice and Organisation of Care (EPOC) Group's Trials Search Coordinator (TSC).

Electronic searches

The TSC will search the Cochrane Database of Systematic Reviews and the Database of Abstracts of Reviews of Effects (DARE) for related systematic reviews, and the following databases for primary studies:

- Cochrane EPOC Group Specialised Register, Reference Manager
- Cochrane Central Register of Controlled Trials (CENTRAL) (*The Cochrane Library*), Wiley (Search date)
- NHS Economic Evaluation Database, HTA Database (*The Cochrane Library*), Wiley (Search date)
- MEDLINE and MEDLINE In-Process and other non-indexed citations, OvidSP (1946 - present)
- EMBASE, OvidSP (1974 - present)
- PsycINFO, OVIDSP (1967 - present)
- CINAHL (Cumulative Index to Nursing and Allied Health Literature), EbscoHost (1982 - present)
- Dissertations and Theses, ProQuest (Search date)
- Index to Theses (Search date)
- Science Citation Index Expanded, ISI Web of Knowledge (1945 - present)
- Conference Proceedings Citation Index - Science, ISI Web of Knowledge (1990)
- Health Management Information Consortium (HMIC), NHS Evidence (1979 - March 2013)
- Social Care Online (http://www.scie-socialcareonline.org.uk)
- World Health Organization - International Clinical Trials Registry Platform (Search date)
- US National Institutes of Health (clinicaltrials.gov)

We will adapt and translate the MEDLINE search strategy in Appendix 1 for other databases using appropriate syntax and vocabulary for those databases. The strategy includes Medical Subject Headings and synonyms for guidelines and implementation. Results will be limited by two methodological filters: the Cochrane Highly Sensitive Search Strategy (sensitivity- and precision-maximising version, 2008 revision) to identify randomised trials (Lefebvre 2011), and an EPOC methodology filter to identify non-RCT designs. There will be no language restrictions and studies will be included regardless of publication status.

Searching other resources

We will search reference lists of all papers and relevant reviews identified.

We will contact authors of relevant papers regarding any further published or unpublished work.

We will also contact authors of other reviews in the field of effective professional practice regarding relevant studies of which they may be aware.

We will conduct a grey literature search to identify studies not indexed in the databases listed above.

Data collection and analysis

Selection of studies

One review author (SW) will download all titles and abstracts retrieved by electronic searching to the reference management system EndNote (EndNote 2014), and remove duplicates. The remaining titles and abstracts will be independently examined by two review authors (AS and SW). We will directly exclude those studies which clearly do not meet the inclusion criteria and obtain full-text copies of potentially relevant references. Two review authors (AS, SW) will independently apply the inclusion criteria. Disagreements will be resolved by discussion, and where necessary arbitration by a third author. Eligible studies will be documented. Studies that are considered potentially eligible after the abstract reviewing stage, but are after scrutiny subsequently excluded, will be documented in the 'Characteristics of excluded studies' table (EPOC 2014a). We will produce a PRISMA flowchart to document the selection process (Moher 2009).

Data extraction and management

Two review authors (AS and SW) will independently extract details of study design, population, intervention, comparison group and outcome measures from each included study using a modified and piloted version of the 'Good practice data extraction form' (EPOC 2014b). Other information to be extracted includes: the setting, whether the intervention was a complex or multifaceted intervention, the theoretical model used to underpin training of the champion (i.e. type of behaviour targeted); how the champion was identified/appointed/nominated. Authors will be contacted by email in the first instance and by telephone to follow-up non-responders to retrieve any missing data or to provide clarification. Any disagreements between the two review authors will be adjudicated by a third author (GF).

Assessment of risk of bias in included studies

Two review authors (SW and AS) will assess the risk of bias independently using the 'Suggested risk of bias criteria' for EPOC

reviews (EPOC 2014b). We will refer any disagreements to a third party for adjudication (GF). We will follow the suggested 'Risk of bias criteria' for EPOC reviews for (i) studies with a separate control group: RCTs, NRCTs, CBA studies and (ii) ITS studies (EPOC 2014c).

For studies with a separate control group, we will use nine 'Risk of bias' criteria :

1. Adequate sequence generation;
2. Adequate allocation concealment;
3. Were baseline outcome measurements similar?
4. Were baseline characteristics similar?
5. Were incomplete outcome data adequately addressed?
6. Was knowledge of the allocated interventions adequately prevented during the study?
7. Was the study adequately protected against contamination?
8. Was the study free from selective outcome reporting?
9. Was the study free from other risks of bias?

For ITS studies we will use the following seven 'Risk of bias' criteria:

1. Was the intervention independent of other events?
2. Was the shape of the intervention effect pre-specified?
3. Was the intervention unlikely to affect/influence data collection?
4. Was knowledge of the allocated interventions adequately prevented during the study?
5. Were incomplete outcome data adequately addressed?
6. Was the study free from selective outcome reporting?
7. Was the study free from other risks of bias?

We will assign an overall assessment of the risk of bias (high, low, unclear) to each of the included studies using the approach suggested in Chapter 8.7 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). We will establish the relative importance of different domains with respect to the review question, and identify key domains on which to make our assessment. Studies with low risk of bias for all key domains or where it seems unlikely that bias has seriously affected the results will be considered to have a low risk of bias. Studies in which risk of bias in at least one key domain was unclear or judged to have some bias that could plausibly raise doubts about the conclusions will be considered to have an unclear risk of bias. Studies with a high risk of bias in at least one key domain or judged to have serious bias that decreases the certainty of the conclusions will be considered to have a high risk of bias. We will incorporate the results into a 'Risk of bias' summary table and graphs for RCT and CBA studies; and separately for ITS studies.

Measures of treatment effect

We will report dichotomous outcomes as a risk ratio (RR) (relative risk) and risk difference (RD) with 95% confidence intervals (CIs). For continuous outcomes, we will calculate the mean difference (MD) with 95% CIs when all studies use the same outcome measure. We will use the standardised mean difference (SMD) with 95% CIs when studies measure the same outcome but use different methods. For ITS studies, we will abstract the difference in slope and the difference in level pre- to post-intervention. The post- versus pre-intervention difference (adjusted for trends) at specific time-points (e.g. 3 and 12 months) will be reported. If the differences are not available in the primary reports, we will attempt re-analysis using data from graphs or tables based on the EPOC-specific guidance for analysis of ITS when the original study analysed the data inappropriately (EPOC 2014d).

Unit of analysis issues

If clustering has not been accounted for in the original analyses, we will use the intra-cluster correlation coefficient (ICC) to adjust the standard errors (or CIs) for the clustering. If the ICC is not published, we plan to estimate it from other similar studies, if they exist (Higgins 2011). However, sensitivity analyses will then be carried out to investigate the robustness of conclusions. For studies with unit of analysis errors, if there is insufficient information or data are not available, CIs and P values will not be reported.

Dealing with missing data

We will contact authors of studies to obtain data not available in the publication. We will deal with drop out of participants by using ntention-to-treat (ITT) analysis. We will use methods outlined by (Higgins 2011, Section 7.7.3) to estimate missing statistics such as standard deviations or correlation coefficients (regression coefficients) from CIs, standard errors, t values, P values, and F values. Otherwise we may use imputation methods, provided the number of statistics to be estimated is low compared to the total number of included studies. We will conduct sensitivity analyses in this case, testing changes in assumptions made.

Assessment of heterogeneity

We will use a random-effects model in any meta-analysis, as it is anticipated that in a complex intervention, the true effect may vary according to context. We will calculate the I^2 to explore statistical heterogeneity (I^2 >30% indicates moderate heterogeneity). If there is substantial methodological, statistical or clinical heterogeneity between studies, we will not proceed with meta-analysis (EPOC 2014d). We anticipate that due to the probability that included studies will have different interventions, and different outcome measures, substantial heterogeneity will exist, and therefore a narrative summary may be more appropriate. We will conduct a qualitative assessment of heterogeneity of the populations, settings and interventions from the included studies table before combining data.

Assessment of reporting biases

We will check the original protocols for studies (if they exist) to assess the level of reporting bias. We will assess the risk of publication bias based on the characteristics of included studies. If there is a sufficient number of studies, we will consider using funnel plots to assess the risk of publication bias.

Data synthesis

We will pool the results of studies if at least two studies are homogenous regarding the participants, interventions and outcomes. For dichotomous outcomes, we will use RR and RD. For continuous outcomes we will use MD or SMD. We will use a more conservative random-effects model if some heterogeneity is identified (I^2 > 30% indicates moderate heterogeneity). We will calculate the median effect size and the interquartile range (IQR) within each study that reports more than one primary outcome.

If it is not possible to pool the results across studies due to high heterogeneity and/or differences in intervention and outcome measures, we will describe the impact of the interventions on outcomes in tabular form and carry out a qualitative assessment of the effect of the studies (EPOC 2014e). We will produce a summary of findings table (one for each main comparison) using the GRADE approach to assess the level of certainty of the evidence for each outcome (Guyatt 2011). We intend to use the GRADE worksheets and/or GRADEpro software to aid in the production of the summary of findings tables (GRADEproGDT 2008).and the assessment of the quality of the body of evidence.

Subgroup analysis and investigation of heterogeneity

The effect of an intervention may vary across studies due to variation in (i) long-term care setting and type of resident, (ii) the intensity of training (if any) provided to the champion, (iii) the method used to appoint the champion, and (iv) the different mechanisms of change employed by the intervention. If there are sufficient studies with the same outcomes, we will investigate subgroups as described. These investigations will be observational and we will compare the magnitude of the effects in different subgroups rather than conduct statistical tests between subgroups. We may use meta-regression to explore heterogeneity when it does not make sense to calculate an average effect across settings; training intensities; champion selection methods, or mechanisms of change. We may use visual analysis of tables (including standardised measures of effect and key explanatory factors), bubble plots and/or box plots.

Sensitivity analysis

In order to determine how robust and consistent the results are, we will conduct sensitivity analyses, based upon study design (RCT versus other) or risk of bias in study (high, moderate, low).

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* *Indicates the major publication for the study*

APPENDICES

Appendix 1. MEDLINE search strategy

1.	(elder$ or geriatric or geriatrics or old or older or aged or senior or seniors or veteran$).mp. [mp=title, abstract, original title, name of substance word, subject heading word, keyword heading word, protocol supplementary concept, rare disease supplementary concept, unique identifier]			
2.	(champion or champions or coordinator$ or Facilitator$ or aide or aides or carer or "care staff" or "healthcare worker" or "certified nursing assistant" or ausculation).mp. [mp=title, abstract, original title, name of substance word, subject heading word, keyword heading word, protocol supplementary concept, rare disease supplementary concept, unique identifier]			
3.	1 and 2			

(*Continued*)

4.	("long term" or "long-term" or "care home$" or "nursing home$" or "care of the elderly" or "continuing care" or "sub acute" or "subacute" or "sub-acute" or "residential care").mp. [mp=title, abstract, original title, name of substance word, subject heading word, keyword heading word, protocol supplementary concept, rare disease supplementary concept, unique identifier]			
5.	1 and 2 and 4			
6.	("long term" or "long-term" or "care home$" or "nursing home$" or "continuing care" or "sub acute" or "subacute" or "sub-acute" or "residential care").mp. [mp=title, abstract, original title, name of substance word, subject heading word, keyword heading word, protocol supplementary concept, rare disease supplementary concept, unique identifier]			
7.	1 and 2 and 6			
8.	(champion or champions or coordinator$ or facilitator or aide or aides or "train the trainer").mp. [mp=title, abstract, original title, name of substance word, subject heading word, keyword heading word, protocol supplementary concept, rare disease supplementary concept, unique identifier]			
9.	1 and 6 and 8			

(*Continued*)

10.	(champion or champions or coordinator$ or facilitator or aide or aides).mp. [mp=title, abstract, original title, name of substance word, subject heading word, keyword heading word, protocol supplementary concept, rare disease supplementary concept, unique identifier]			
11.	1 and 6 and 10			
12.	1 and 10			

CONTRIBUTIONS OF AUTHORS

AS and SW conceived the review, drafted the protocol and developed the search strategy. GF reviewed the protocol.

DECLARATIONS OF INTEREST

None.

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Salary

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Salary

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