THE CAMPBELL COLLABORATION

Protocol for a Systematic Review: Discharge programmes for individuals experiencing, or at risk of experiencing, homelessness: A systematic review.

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Submitted to the Coordinating Group of:

	Crime and Justice
	Education
	Disability
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X	Social Welfare
	Other:

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1 Background

1.1 THE PROBLEM, CONDITION OR ISSUE

Provide a description of the problem, condition or issue that the intervention under review is aiming to address. (You may provide citations of relevant papers. Use APA style for referencing.)

People who have spent time in an institutional setting, such as prison or in-patient health services, may be at risk of homelessness upon discharge from the institution (Tsai & Rosenheck, 2015; Winkler et al., 2016). People experiencing homelessness have higher rates of ill health and low quality of life (Fazel, Geddes, & Kushel, 2014) and are between 2 and 6 times more likely to die than those who are not homeless (Thomas, 2012), with men at particularly high risk. Those leaving institutional settings are likely to have existing challenges to their health and wellbeing. This population is especially at risk of poor outcomes if discharged into homelessness or into unstable housing or accommodation that is no longer suitable for their needs.

Extent of the problem & associated problems

Those who have been residing in an institutional setting are known to be at higher risk of homelessness upon discharge (Gulliver, 2015). Definitions of homelessness vary across the world, making cross-national comparisons on the extent and nature of homelessness challenging (Amore, Baker, & Howden-Chapman, 2011). Further, without a reasonably accurate picture of the scale of the problem and the needs of the population who are homeless, or are at risk of becoming homeless, effective policymaking is hindered. For example, defining homelessness as number of people accessing homeless services ignores those 'hidden homeless' who do not access homeless services but live in unstable or unsuitable accommodation. Despite these measurement issues, estimates do suggest that people leaving institutional settings have substantially higher risk of becoming homeless than the general population. For example, in the US, between 31% and 46% of youth aging out of foster care had been homeless at least once by age 26, compared to just 4% of the general population. A Canadian study of discharge from psychiatric hospital found that 10.5% of people were discharged into homelessness (Forchuk, Russell, Kingston−MacClure, Turner, & Dill, 2006).

Of course, depending on the institutional setting people have been residing in different groups of people are likely to have different needs. For example, those discharged from in-patient addiction treatment are likely to need a stable, drug free living environment. Whereas youth aging out of care may need structured practical tapering support to enable them to become self-sufficient adults. There will also be many individuals with multiple risk factors and comorbid conditions placing them at even higher risk of homeless and associated negative outcomes. Discharge into shelter accommodation where overcrowding, violence drug use and lack of privacy, is not a suitable place for a person trying to recover from ill health. After discharge from institutions, unstable or unsuitable living conditions can contribute to relapse, recidivism, deterioration in health and readmission to hospital (Gulliver, 2015).

The Intervention

Define the intervention and its components. Define all terms and intervention components clearly and try to set a tone that does not pre-judge the value of the intervention. Provision of examples of the intervention and its components here will help the reader gain a better understanding of the intervention under review. Outline possible variations of the intervention. What is given, by whom, and for how long?

Discharge programmes involve the coordination and provision of services, including accommodation, for people upon discharge from institutions. These programmes aim to avoid discharging people into homelessness and to reduce the risk of subsequently becoming homeless, with the overarching goal of trying to prevent people entering into a costly cycle of unsafe discharge, readmission, relapse or recidivism and widening health inequalities (Whiteford & Cornes, 2019). Discharge programmes may be offered to people in a diverse set of circumstances including people; leaving military service; released from prison; being discharged from hospitals, mental health services, addiction treatment or other in-patient health care services; young people ageing out of care. Supporting a person to establish suitable stable housing may in turn improve their chances of recovery from illness or addiction, reduce the risk of relapse or recidivism, and improve quality of life.

The programmes currently in use in high income countries adopt a variety of approaches with different levels of complexity. Programmes primarily seek to address housing needs, either through maintaining previous housing arrangements prior to entry into the institution or to seek new suitable accommodation. Programmes may also offer continued support prior to and following on from discharge, to ensure the persons housing situation is suitable and sustainable. This could be in the form of paying rent for the individual, facilitating family/partner contact to maintain relationships during time away from home. For example, one simple intervention in a prison context is supporting contact with family to maintain relationships so the person has a home to return to on release. Other, more complex models, involve the coordination of multiple agencies to enhance the continuity of care

and support a person to access services. For example, Critical Time Intervention (Herman et al., 2011), offers care coordination along with direct emotional and practical support over nine months during the critical discharge period. Another example is a 'transition of care' model, where hospital settings work together with community health and social care colleagues, housing organisations and voluntary sector to plan for a person's discharge and effectively communicate with each other to facilitate a smooth transition with the goal of reducing the need for re-admission.

How the Intervention Might Work

In this section, briefly identify the theoretical underpinnings and refer to literature that identifies a potential pathway of effect between intervention and outcomes. Describe the mechanisms by which the intervention is expected to bring about the expected changes in the outcomes. You might wish to include a logic model here which shows the connections between the intervention and outcomes. A logic model should define the intervention of interest and its components, specify important outcomes, and indicate intermediate outcomes or pathways through which the intervention is intended to affect the outcomes. The logic model may also be used to provide a logical rationale for why only a component of an intervention is being reviewed (and point to where other reviews may need to be carried out to complete the evidence picture).

Generally, discharge programs aim to prevent people being discharged into homelessness, or to reduce the risk that they will become homeless due to unsuitable or unsustainable housing. The range of possible approaches is broad but generally they seek to achieve this aim through assessing individual needs, planning for discharge in advance, establishing communication and coordination between the institution and relevant statutory and voluntary agencies such as social services, housing agencies, parole office, community health teams to ensure that a person is discharged into suitable accommodation. Some interventions will also provide ongoing support to help each person to access appropriate health and/or social care services to reduce the risk of readmission and support their reintegration into the community. By improving access to suitable accommodation and support services there is improved opportunity for complete recovery from both physical and mental illness, substance use and reducing the risk of recidivism.

1.2 WHY IT IS IMPORTANT TO DO THIS REVIEW

Clearly describe the justification for doing the review. Why is the review needed? This section should include two main components.

First, you should include a discussion here of existing and ongoing primary research, narrative and systematic reviews, and meta-analyses on the topic, to highlight what has been learned from past efforts as well as to point out any inconsistencies, methodological strengths and weaknesses, and evidence "gaps" that still remain. The contribution of your planned review should be emphasized by clearly stating the unresolved questions and controversies that will be addressed.

Second, to instruct the end-user on the potential application of review findings, include a brief statement on how this could inform practice or policy decisions.

In high income countries homelessness is rising and there is a significant need to identify and implement effective policies and interventions, and discontinue ineffective practices in order to reduce homelessness. In the UK, for example the government has pledged to end homelessness in England by 2027 (*Rough Sleeping Strategy*, 2018), People who are approaching the transition from an institutional setting may be particularly at risk of homelessness on discharge. To ensure that policymakers avail of the most robust and rigorous evidence to date a Systematic Review of the literature on interventions aimed at reducing risk and/or incidence of homelessness for this vulnerable population is needed.

This systematic review will be based on evidence already identified in two existing evidence and gap maps (EGMs) commissioned by the Centre for Homelessness Impact (CHI) and built by White, Saran, Teixeira, Fitzpatrick & Portas (2018). The EGMs present studies on the effectiveness and implementation of interventions aimed at people experiencing, or at risk of experiencing, homelessness. The EGMs were constructed using a comprehensive search strategy including a search of Campbell, PROSPERO and Cochrane databases. The map identified one systematic review

relevant to discharge interventions (Kyle & Dunn, 2008). However, this review is focused primarily on people with severe mental illness and is not a review of the effectiveness of discharge programmes. One other possibly overlapping review is by Chambers et al (Chambers et al., 2018), on housing interventions for 'vulnerable adults'. While there may be some overlap, our review is distinct in its focus on discharge programs specifically and including any individuals at risk of homelessness and not limited to adults only. Our proposed review is also unique in that we will include evidence on both effectiveness and implementation, including qualitative data, to develop a comprehensive synthesis of which programmes can work, for whom, under what circumstances alongside a synthesis of the common barriers and facilitators for effective implementation.

1.3 OBJECTIVES

Briefly outline the objectives of the proposed review. Systematic reviews can be undertaken for a number of reasons. For example, reviews can be conducted to (a) produce general statements about relationships and treatment effects through the synthesis of individual study results, (b) find reasons for conflicting evidence, (c) answer questions, using variations in studies, that could not have been answered in the individual component studies, (d) explain variations in practice, (e) review the evidence on the subjective experience of an intervention, and/or (f) build connections between related areas of research. While Campbell systematic reviews might be motivated by any of these and other reasons, their overarching aim should be to gather, summarize and integrate empirical research so as to help people understand the evidence.

In setting out the objectives, reviewers should keep in mind that Campbell systematic reviews should help people make practical decisions about social and behavioural interventions and public policy. This has important implications for deciding whether and how to undertake a Campbell systematic review, how to formulate the problem that a review will address, how to develop the protocol and how to present the results of the review. The objectives of a review should address the choices (practical options) people face when deciding about whether or not to adopt a policy or practice. Reviews should address outcomes that are meaningful to people making decisions about public policy.

1. What is the effect of discharge programmes on outcomes for individuals experiencing or at risk of experiencing homelessness?
2. Who do discharge programmes work best for?
    i) Young people/older adults

    ii) Men and women

    iii) People discharged from different institutions e.g. prison, hospital, substance abuse treatment

    iiii) Other sub groups or populations

3. What implementation and process factors act as barriers or facilitators to intervention delivery?
4. Is implementation fidelity related to the effectiveness of the intervention?

1.4 METHODS

1.4.1 Types of studies

Briefly describe the types of primary study designs that will be included and excluded in this review.

We will include all study designs where a comparison group is used. This includes Randomised controlled trials, quasi-experimental designs, matched comparisons, other study designs that attempt to isolate the impact of the intervention on homelessness using appropriate statistical modelling techniques.

As randomised control trials are accepted as more rigorous than non-randomised studies, the potential impact of non-random study design on effect sizes will be explored as part of the analysis of heterogeneity.

Studies must include an inactive comparison condition that could include;

- **No treatment**.
- **Treatment as usual** where pupils receive their normal level of support or intervention. Details of what this consists of will be extracted.
- **Waiting list** where schools or classrooms are randomly assigned to receive the intervention at a later date. Details of what happens to waitlisted participants will be extracted.
- **Attention control**, where participants receive some contact from researchers but both participants and researchers are aware that this is not an active intervention.
- **Placebo** where participants perceive that they are receiving an active intervention but the researchers regard the treatment as inactive.

Studies with no control or comparison group, unmatched controls or national comparisons with no attempt to control for relevant covariates will not be included. Case studies, opinion pieces or editorials will not be included.

1.4.2 Types of participants

Briefly describe the types of participants that will be included and excluded.

Persons experiencing, or at risk of experiencing, homelessness in institutions in high income countries. We will include people of all ages and in any institutional setting including but not limited to military service, social care, in-patient health care, residential treatment for addiction and prison.

1.4.3 Types of Intervention

Briefly describe the types of intervention(s) and comparator(s) that will be included and excluded.

We will include any intervention targeted at people being discharged from any institutional setting, that aims to avoid discharging into homelessness or reduce the risk of future homelessness through planning for suitable stable accommodation prior to discharge. Typically, interventions will involve advance planning prior to discharge and coordination between institutions and housing services. Some interventions will also provide ongoing support to people to enable them to access appropriate health and social care services to support their transition from an institutional setting.

The control or comparison condition can include no services/intervention, services as usual, attention control or waiting list (see study design section for more detail).

1.4.4 Types of outcome measures

Briefly describe the types of outcome measures that will be included and excluded.

Given the breath of possible outcomes and means of measurement we will extract all outcome data relating to seven broad domains.

1.4.4.1 Primary outcome domains

1) Housing stability

2) Health, including substance abuse, mortality, morbidity, readmission

1.4.4.2 Secondary outcome domains

3) Access to services, including appropriate ongoing community support for individual needs.

4) Crime/criminalization

5) Employment and income

6) Capabilities and wellbeing

7) Cost of intervention

These domains reflect six out of the seven outcome domains used in the Evidence and Gap map, with the addition of cost.

We will also pay particular attention to implementation and acceptability of interventions and will include an analysis of attrition rates or 'dropout' from interventions.

Duration of follow up

Briefly describe the duration(s) of follow-up that will be included and excluded.

We will include studies with follow-up of any duration and data relating to all follow-up points will be extracted. We will conduct separate analysis for each follow up period as follows; up to one month, six months, one year, two years, more than two years post discharge.

Types of settings

Briefly describe the types of settings that will be included and excluded.

Relevant institutional settings will include but not be limited to military service, social care, in-patient health care, residential treatment for addiction and prison. Settings to which individuals are discharged may include, but not be limited to, respite care, temporary housing, shelter/hostel, their own home with modifications to make it suitable for current needs, permanent housing.

1.4.5 Search methods for identification of studies

Briefly describe the anticipated search strategy.

This systematic review will be based on evidence already identified in two existing evidence and gap maps (EGMs) commissioned by the Centre for Homelessness Impact (CHI) and built by White, Saran, Teixeira, Fitzpatrick and Portas (2018). The EGMs present studies on the effectiveness and implementation of interventions aimed at people experiencing, or at risk of experiencing, homelessness in high income countries. The maps used a comprehensive three stage search and mapping process. Stage one was to map the included studies in an existing Campbell review on homelessness (Munthe-Kass, Berg, & Blaasvær, 2018), stage two was a comprehensive search of 17 academic databases, three evidence and gap map databases, and eight systematic review databases for primary studies and systematic reviews. Finally stage three included web searches for grey literature, scanning reference lists of included studies and consultation with experts to identify additional literature. Sample search terms can be found in the protocol (White et al., 2018)

1.4.5.1 Data collection and analysis

1.4.5.1.1 Description of methods used in primary research

Interventions will include any trial measuring the effectiveness of discharge programmes compared to a control group or well matched comparison group.

1.4.5.1.2 Criteria for determination of independent findings

It is important to ensure that the effects of an individual intervention are only counted once and the following conventions will therefore apply.

Where there are **multiple measures reported for the same outcome**, this will be dealt with by calculating an average effect size within each study for each outcome. A simple average effect size will be calculated by first calculating the effect size for each measure of a given outcome and then averaging these effect sizes within each study. The exception will be any treatment inherent measures of the outcome of interest, these measurements will be discarded as they risk overestimating the treatment effect.

Where the **same outcome construct is measured but across multiple time domains**, such as through the collection of both post-test and further follow-up data, the analysis will conducted and reported separately for different time points (see above).

Studies comparing **multiple treatment and control arms** will be discussed with the full author team to decide if eligible intervention arms are similar enough to combine and compare as if they are one intervention group. If not, each intervention arm will contribute separate effect sizes to the meta-analysis and the control group sample size will be split by the number of intervention arms included to avoid double counting of control participants.

In the case of **multiple cohorts** appearing in one study we will calculate a simple average, as described above, for the omnibus meta-analysis. If different cohorts in a study fall into different subgroups then they will be considered separately in subgroup analysis but no overall summary of effect will be calculated combining subgroups in those cases. If there are sufficient eligible studies

reporting multiple and dependent effect sizes (i.e. occurring in more than 20 eligible studies) then robust variance estimation will be employed. This technique calculates the variance between effect sizes to give the variable of interest a quantifiable standard error. It has been shown to calculate correct results with a minimum of 20-30 individual studies (Hedges, Tipton, & Johnson, 2010) although it performs better with an increased quantity of studies.

1.4.5.2 Selection of studies

The studies contained within the exiting evidence and gap maps will be screened against the inclusion criteria for eligibility by two independent screeners.

1.4.5.3 Data extraction and management

Once eligible studies have been found, we will undertake dual data extraction, where two authors will both complete data extraction and risk of bias assessments independently for each study. Coding will be carried out by trained researchers. Any discrepancies in screening or coding will be discussed with senior authors until a consensus is reached.

1.4.5.3.1 Details of study coding categories

A coding framework will be developed and piloted prior to undertaking data extraction for all included studies using EPPI Reviewer software. At a minimum we will extract the following data: Publication details, Geographical location of study, Intervention details including setting, dosage and implementation, Delivery personnel, Descriptions of the outcomes of interest including instruments used to measure, Design and type of trial, Sample size of treatment and control groups, Data required to calculate Hedge's g effect sizes, Quality assessment. It is anticipated that we will also extract more detailed information on the interventions such as: Duration and intensity of the programme, Timing of delivery, Key programme components (as described by study authors). Alongside extracting data on programme components, descriptive information for each of the studies will be extracted and coded to allow for sensitivity and subgroup analysis. This will include information regarding:

- the study characteristics in relation to: design, sample sizes, measures and attrition rates, who funded the study, and whether the study was conducted by a research team associated with the programme or an independent team.
- the stage of programme development, for example whether it is a new programme being piloted or an established programme being replicated or scaled-up.
- the extent to which the programme was delivered as intended (fidelity)
- Demographic variables relating to the participants including age, gender, and other relevant population characteristics.
- Setting, which type of institutional setting(s) are study participants transitioning from?

Quantitative data will be extracted to allow for calculation of effect sizes (such as mean change scores and standard error or pre and post means and standard deviations). Data will be extracted for the intervention and control group on the relevant outcomes measured in order to assess the intervention effects.

1.4.5.4 Assessment of risk of bias in included studies

Assessment of methodological quality and potential for bias will be conducted using The Cochrane Risk of Bias tool for Randomised controlled trials. This is a standard tool, which takes the forms of a series of questions about the randomisation procedures and blinding. Non-randomised studies will be coded using XXXXX

1.4.5.5 Measures of treatment effect

It is anticipated that most outcomes reported will be based upon continuous variables and so the main effect size metric to be used for the purposes of the meta-analyses will be the standardized mean difference, with its 95% confidence interval. Within this, Hedges' g will be used to correct for any small sample bias. Where other effect sizes have been reported, such as Cohen's d or risk ratios (for dichotomous outcomes) these will be converted to Hedges' g for the purposes of the meta-analysis using formulae provided in the Cochrane Handbook (Higgins & Green, 2011).

using formulae provided in the Cochrane Handbook (Higgins & Green, 2011).

1.4.5.6 Unit of analysis issues

If studies involve group-level allocation, where possible, data will be included which have been adjusted to account for the effects of clustering, typically through the use of multilevel modelling or adjusting estimates using the intra-cluster correlation coefficient (ICC). Where the effects of clustering have not been taken into account, estimates of effect size will be adjusted following guidance in the Cochrane Handbook. If ICC is not reported external estimates will be obtained from studies that provide the best match on outcome measures and types of clusters from existing databases of ICCs (Ukoumunne, Gulliford, Chinn, Sterne, & Burney, 1999) or other similar studies within the review.

1.4.5.7 Dealing with missing data

If study reports do not contain sufficient data to allow calculation of effect size estimates authors will be contacted to obtain necessary summary data, such as means and standard deviations or standard errors. If no information is forthcoming the study cannot be included in meta-analysis and will instead be included in a narrative synthesis.

1.4.5.8 Assessment of heterogeneity

Heterogeneity will be assessed first, through visual inspection of the forest plot and checking for overlap of confidence intervals and second through the Q, I2 and Tau2 statistics.

1.4.5.9 Assessment of reporting biases

A funnel plot and Egger's linear regression test will be included to check for publication bias across included studies (Stern & Egger, 2005). Where the funnel plot is asymmetrical this indicates either publication bias or bias which relates to smaller studies showing larger treatment effects. The trim and fill method will be used where the funnel plot is asymmetrical (Higgins & Green, 2011), this is a nonparametric technique which removes the smaller studies causing irregularity until there is a new symmetrical pooled estimate, the studies which were eliminated where then filled back in to reflect the new estimate.
To ensure robustness of the review and to account for individual studies that appear to exert an undue influence on findings, process sensitivity analysis will also be carried out on domains relating to the quality of the included studies.

1.4.6 Data synthesis

2. Who do discharge programmes work best for?
    i) Young people/older adults

    ii) Men and women

    iii) People discharged from different institutions e.g. prison, hospital, substance abuse treatment

    iiii) Other sub groups or populations

3. What implementation and process factors act as barriers or facilitators to intervention delivery?
4. Is implementation fidelity related to the effectiveness of the intervention?

1.4.6.1 Approach to meta-analysis

Given the diverse range of interventions that this review is likely to find, random effects models, using inverse-variance estimation, will be used as the basis for meta-analysis. The analysis will be conducted using Stata 14 and the range of commands externally developed to conduct meta-analysis with Stata such as metan and metareg (Palmer & Sterne, 2016).

1.4.6.1.1 Main effects (Objective 1)

The main effects analysis, synthesising the evidence in relation to the effects of discharge programs in general, will be undertaken using the approach to meta-analysis outlined above for each primary and secondary outcome in turn, with separate analysis for follow-up of different duration (see *duration of follow-up*).

1.4.6.2

1.4.6.2

Sensitivity analysis

For each outcome, the following sensitivity analyses will also be undertaken to assess whether there are potential influences relating to:

1. Studies that appear to exert an undue influence on findings.

2. Study quality (Studies with a "high" or "unclear" risk of bias on 3 or more of the 7 risk of bias domains in the Cochrane Risk of Bias assessment will be coded as low quality).

In relation to studies that appear to exert an undue influence, a further meta-analysis will be conducted for each outcome that omits these studies to assess whether their inclusion exerts an influence on the findings.

1.4.6.3

Subgroup analysis and investigation of heterogeneity

Assessment of differential effectiveness in relation to age, gender, institutional setting or other subgroups/populations identified in included studies (Objective 2)

Eligible studies will be coded in terms of:

- The age of the participants falls into child (under 12), adolescent or young adult (age 13-25), adult (26-65), older adult (age 65plus).

- The effects for males and females, where reported separately. In such cases, each study will generate two studies for the purposes of the subgroup analyses to follow (one based upon the sample for boys only, and the other for girls only). We anticipate that few, if any, studies will assess the effectiveness of discharge programs for trans or non-binary people but if sufficient studies so report this information we will include this third group in our analysis.

- The institutional setting people have been residing in.

Three subgroup analyses will then be conducted in relation to each of the three factors above (age, gender and institution) for each of the primary and secondary outcomes. The subgroup analyses (based upon random-effects models), will group studies by sub-category and estimate overall effects sizes for each. Subgroup analyses will only be carried out where studies included in the subgroup analysis are sufficiently similar to eachother in all other respects, such as weather the interventions delivered to younger and older people are similar enough to be confident that the subgroup analysis reflects differences in the effectiveness for different populations rather than different intervention effects.

1.4.6.4 Treatment of qualitative research

Qualitative research included in this review is based on an existing evidence and gap map (EGM) commissioned by the Centre for Homelessness Impact (CHI) and built by White, Saran, Teixeira, Fitzpatrick and Portas (2018). The EGM presents 246 qualitative evaluations on the implementation issues of homelessness interventions.

The implementation issues categories included in the EGM were developed through an iterative process. Initially categories were based on the implementation science framework (Aarons, Hurlburt & Horwitz, 2011). These categories were then independently piloted against process evaluations and agreement was reached by researchers in the Campbell Collaboration, Queen's University Belfast, and Heriot-Watt University. The five broad categories of implementation issues agreed are: contextual factors, policy makers / funders, programme managers / implementing agency, staff / case workers, and recipients.

There are many process evaluations of accommodation based- interventions identified by the EGM and they will be included in the synthesis of qualitative data. We will appraise the quality of the studies using a tool developed by White and Keenan (2018) and will narratively synthesise the barriers and facilitators described in the included process evaluations.

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ADVISORY GROUP

1.5 CONTRIBUTIONS OF AUTHORS

The review will be undertaken by systematic review specialists within the Campbell UK & Ireland Centre. Dr Sarah Miller will be the Principal Investigator (PI) of the project and will have overall responsibility for its conduct and delivery. Dr Jennifer Hanratty will be responsible for the day to day operation of the reviews. This review will be supported by specialist input from Dr Ciara Keenan alongside research support from two full time research assistants.

Dr Jennifer Hanratty has worked in evidence synthesis since 2012 and published reviews with Campbell, Cochrane and NIHR Health Technology Assessment amongst others. Jennifer is associate Editor with Campbell Education Co-ordinating group, on the editorial board of the Campbell Knowledge Translation and Implementation Group, and represents Campbell UK & Ireland on the advisory board for Evidence Synthesis Ireland.

Dr Sarah Miller is the Deputy Director of Campbell UK & Ireland. She is co-chair and co-editor of the Campbell Education Coordinating Group and also Deputy Director of the Centre for Evidence and Social Innovation, within which she leads the What Works in Schools programme of research. She has considerable methodological and statistical expertise, which includes the conduct and analysis of randomised controlled trials as well as systematic reviews and meta-analyses.

Dr Ciara Keenan has acquired six years' experience working across 15 Systematic Reviews. Ciara is currently co-convenor of the Campbell Collaboration's Information Scientist Network; methods editor for the Campbell Collaboration's Education coordinating group; and founder and editor of the meta-evidence blog.

Please note that this is the *recommended optimal* review team composition.

- Content: Mackie, Fitzpatrick, Cowman
- Systematic review methods: Hanratty, Keenan & Miller
- Statistical analysis: Hanratty, Keenan & Miller
- Information retrieval: Keenan & Hanratty

1.6 FUNDING

Do you receive any financial support, and if so, from where? What are your deliverable deadlines for the review? If not, are you planning to apply for funding, and if so, from where?

This review is funded by the Centre for Homelessness Impact. The review is due to be submitted to the coordinating group by the end of September 2019.

1.7 POTENTIAL CONFLICTS OF INTEREST

Please read the Campbell conflict of interest policy (October 2013). Ask each of your co-authors to fill in a conflict of interest form (available in the policy), then describe any potential conflicts here. For example, have any of the authors been involved in the development of relevant interventions, primary research, or prior published reviews on the topic? Please submit your forms with the title registration form.

1.8 PRELIMINARY TIMEFRAME

Note, if the protocol or review is not submitted within six months and 18 months of title registration, respectively, the review area is opened up for other authors.

- Date you plan to submit a draft protocol: January 31st 2019
- Date you plan to submit a draft review: 27th September 2019

PLANS FOR UPDATING THE REVIEW

Review authors will seek opportunities for further funding to update the review ever 5 years.

AUTHOR DECLARATION

Authors' responsibilities

By completing this form, you accept responsibility for preparing, maintaining and updating the review in accordance with Campbell Collaboration policy. The Campbell Collaboration will provide as much support as possible to assist with the preparation of the review.

A draft review must be submitted to the relevant Coordinating Group within two years of protocol publication. If drafts are not submitted before the agreed deadlines, or if we are unable to contact you for an extended period, the relevant Coordinating Group has the right to de-register the title or transfer

the title to alternative authors. The Coordinating Group also has the right to de-register or transfer the title if it does not meet the standards of the Coordinating Group and/or the Campbell Collaboration.

You accept responsibility for maintaining the review in light of new evidence, comments and criticisms, and other developments, and updating the review at least once every five years, or, if requested, transferring responsibility for maintaining the review to others as agreed with the Coordinating Group.

Publication in the Campbell Library

The support of the Coordinating Group in preparing your review is conditional upon your agreement to publish the protocol, finished review, and subsequent updates in the Campbell Library. The Campbell Collaboration places no restrictions on publication of the findings of a Campbell systematic review in a more abbreviated form as a journal article either before or after the publication of the monograph version in *Campbell Systematic Reviews*. Some journals, however, have restrictions that preclude publication of findings that have been, or will be, reported elsewhere and authors considering publication in such a journal should be aware of possible conflict with publication of the monograph version in *Campbell Systematic Reviews*. Publication in a journal after publication or in press status in *Campbell Systematic Reviews* should acknowledge the Campbell version and include a citation to it. Note that systematic reviews published in *Campbell Systematic Reviews* and co-registered with the Cochrane Collaboration may have additional requirements or restrictions for co-publication. Review authors accept responsibility for meeting any co-publication requirements.

I understand the commitment required to undertake a Campbell review, and agree to publish in the Campbell Library. Signed on behalf of the authors:

Form completed by: Jennifer Hanratty **Date:** Feb 2019