

# Protocol: Improving access to health and social services for individuals experiencing, or at risk of experiencing, homelessness: A Systematic Review

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## Background

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### ***The problem, condition or issue***

Homelessness is a multifaceted issue with outcomes that are as complex and unique as the individual who is experiencing life without stable housing. Those people who are currently experiencing homelessness have a much greater risk of poorer physical and mental health than the general population (Homeless link, 2014), and so the requirement to access health and social care (HSC) services is increased.

Accessing HSC services when homeless is extremely difficult for a myriad of reasons. First, there are many countries in the world without a free health care system and homeless individuals will need to prioritise food and shelter over their basic HSC needs. Second, there can be difficulties associated with registering for HSC services due to practical issues such as providing documentation or a current address or telephone number. Third, issues with the

location of the HSC services may be an additional barrier to those without access to transport. Fourth, it may be that there is a lack of HSC services to meet an individual's needs, or if not, there may be a waiting list that delays a person's access to the service they require. Fifth, the individual may be someone who has multiple HSC needs and might find it impossible to access all the services they require as the disordered nature of life can make managing appointments problematic. Finally, people experiencing homelessness experience high levels of prejudice and discrimination (Weng & Clark, 2018) and so fear of these pervasive attitudes and behaviours, coupled with low confidence and self-esteem may ensure that those who require HSC services avoid accessing them.

### ***The intervention***

Individuals experiencing homelessness have multiple needs requiring support from a range of HSC services. These services may be provided by professionals such as general practitioners, hospital staff, community nurses, dentists, pharmacists, social workers, and staff working in housing, employment or education services. As outlined previously, a person experiencing homelessness may face seemingly impenetrable barriers to accessing these services and professionals. To improve access to HSC services, interventions must address the barriers which exist. This systematic review will include all relevant studies which assess the effectiveness of interventions which *influence* access to HSC services for individuals experiencing, or at risk of experiencing homelessness. Although related, this systematic review is not concerned with those studies which assess the effectiveness or implementation of the HSC services themselves.

Interventions which may influence access to HSC services and are suitable for inclusion in this review may range from those which focus on improvements to assessment procedures and referrals perhaps through primary care initiatives to those which coordinate required support for the service user perhaps through community-based initiatives.

### ***How the intervention might work***

Facilitating access to HSC services help those individuals experiencing homelessness lead more independent, healthy and happy lives while retaining autonomy over their health and social care choices. When a service that someone requires and is entitled to is inaccessible, they cannot receive the treatment and support available, meaning it has failed to meet their needs.

The United Kingdom's National Health Service (NHS) recognises the need to improve access to services especially to underprivileged groups as outlined in principle one of the NHS constitution.

“The NHS provides a comprehensive service, available to all...it has a duty to each and every individual that it serves and must respect their human rights. At the same time, it has a wider social duty to promote equality through the services it provides and to pay particular attention to groups or sections of society where improvements in health and life expectancy are not keeping pace with the rest of the population.”

Considerations for improving access to HSC services include how the service is advertised, the physical environment of the service, staff levels of awareness, empathy and understanding, methods of communication and how information is presented to service users.

### ***Why it is important to do the review***

Globally homelessness is rising and there is a significant need to identify and combine all relevant interventions which aim to improve access to HSC services. To ensure that policymakers avail of the most robust and rigorous evidence to date, a Systematic Review of the literature is required.

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 EGMs identified various systematic reviews which assess the effectiveness of interventions to

identified various systematic reviews which assess the effectiveness of interventions to improve both physical and mental health in homeless populations (Hwang, Tolomiczenko, Kouyoumdjian & Garner, 2005; Speirs, Johnson & Jirojwong, 2013; Thomas, Gray & McGinty, 2011) and reducing homelessness (Fitzpatrick-Lewis, et. al., 2011; Munthe-Kaas, Berg & Blaasvæ, 2018) but fewer focus on those interventions which seek to improve access to HSC services. The author will outline those systematic reviews which synthesise the literature around interventions to improve access to HSC services and how they are different from the proposed review.

### *Restricted by intervention*

Three reviews have included only those interventions which use social networking sites to improve access to HIV prevention services. First, A systematic review by Capurro and colleagues (2014) identified 73 studies. However, as they focussed on a general population of participants described as ‘difficult to reach’ only two studies which focussed on homeless youth were included (Rice, Tulbert, Cederbaum, Barman Adhikari & Milburn, 2012; Young & Rice, 2011). Similarly, a second systematic review (Lim, Wright, Carrotte & Pedrana, 2016) of 47 studies found only one which included a homeless population (Rice et al., 2012). Thirdly, in a systematic review which located 58 social network based interventions (Ghosh et al., 2017) five were on homeless men and youths. However, as the review included other outcomes such as drug adherence and patient retention, of the five studies included on the population of interest to this review, none were associated with access to services.

Another systematic review identified by the map did centre on a homeless population (McInnes, Li & Hogan, 2013). However, it focussed on their access to information technologies such as mobile phones and the internet. The authors do conclude that this access to technology will improve access to HSC, but this was not tested within the review.

### *Restricted by population*

Three reviews have included only specific subsets of the homeless population. First, a review by Hudson and Colleagues (2016) included nine qualitative studies which focussed on access to services of those individuals requiring palliative care, while another systematic review of 62 studies (Brown, Rice, Rickwood & Parker, 2016) focussed on those individuals requiring mental-health care only. Third, a systematic review of 12 studies conducted within the European Union (de Vries et al., 2017) focussed on access to diagnostic and treatment services for tuberculosis patients.

### *Restricted by outcome*

Finally, although a systematic review of five studies exists which has similar objectives to the current review (Health Quality Ontario, 2016), one of the inclusion criteria is more limited than the current review. Authors retrieved only those interventions that would improve access to a primary care provider (a physician, a nurse, or a nurse practitioner). This review will be wider in scope and seek to improve access to all HSC services, not just primary care.

## **Objectives**

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1. What is the effect of interventions to improve access to health and social services on outcomes for individuals experiencing or at risk of experiencing homelessness?
2. Who do access interventions work best for?
  - a. Young people/older adults?
  - b. Males/Females?
  - c. 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?

## **Methodology**

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## ***Criteria for including and excluding studies***

### *Types of study designs*

Only studies which use a control group design will be eligible for inclusion. These groups (intervention and control) can be assigned randomly or non-randomly. If non-random, only a rigorous matched-comparison group design will be accepted for inclusion in the analysis. A matched group design consists of a treatment and control group that share similar baseline characteristics. This design allows greater confidence that observed group differences are due to the intervention rather than baseline differences. In a matched-comparison design, the interventionists ensure equivalence between the two groups by collecting data on potential confounding variables at pre-test. Non-random studies will be judged in relation to how suitably matched the two groups were at pre-test and whether attempts were made to control for any pre-test differences.

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 subgroup analyses and any significant influences will be controlled through meta-regression.

Control groups can include various types, such as; alternative treatment, placebo, no treatment, waitlist, or usual treatment (standard care). Any study which includes one group pre-test/post-test or in which a treatment group is compared to another treatment group without a control arm will not be eligible for inclusion.

Studies with no control or comparison group, unmatched controls or cross-national comparisons with no attempt to control for relevant covariates will not be included. Case studies, opinion pieces or editorials will not be included. Finally, A person cannot serve as their own control, but instead must be compared against a group of untreated participants.

### *Types of participants*

This systematic review on access to HSC services will focus on all individuals who are currently experiencing, or at risk of experiencing homelessness irrespective of age or gender. The included studies will include populations from high-income countries. Homelessness is defined as those individuals who are sleeping 'rough' (sometimes defined as street homeless), those in temporary accommodation (such as shelters and hostels), those in insecure accommodation (such as those facing eviction or in abusive or unsafe environments), and those in inadequate accommodation (environments which are unhygienic and/or overcrowded).

### *Types of interventions*

Interventions that will be included within this systematic review will be those with an explicit objective of improving access to HSC services, we are not concerned with the services themselves. HSC services will vary immensely according to factors such as resources available in each jurisdiction and/or the specific needs of the individual experiencing homelessness. Some examples of interventions may include:

- those which seek to improve access or rate of referral to a GP or nurse,
- interventions which seek to improve collaboration between statutory, community and voluntary organisations offering HSC services
- those which improve the timeliness of access to all HSC services,
- interventions which clearly inform individuals on the services available,
- interventions which seek to educate HSC professionals on improving access for individuals experiencing, or at risk of experiencing, homelessness
- Those interventions which adapt methods of communication and how information is presented to service users.

Comparison conditions will include services as usual or alternative services/intervention.

### *Types of outcome measures*

This review will primarily address how interventions can improve access to HSC services of those individuals experiencing, or at risk of experiencing, homelessness.

Secondary outcomes include:

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

### *Duration of follow-up*

It is anticipated that the included interventions will report effects at multiple follow-up periods after implementation of the intervention. In instances where this is the case, data relating to multiple points of follow up will be extracted in their entirety. This will allow us to conduct analysis on effect sizes related to similar time points and when outcomes are similar across various timepoints then an average effect size will be calculated to estimate effectiveness.

### *Types of settings*

Settings where these interventions take place may be varied and can include community-based settings, vocational settings, treatment centres, clinical settings and the individual's temporary accommodation.

### **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)

### **Description of methods used in primary research**

Trials measuring the effectiveness of interventions to improve access to HSC services against a control group or well matched comparison group will be included.

### **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 be 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 variance of interest a

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.

### ***Details of study coding categories***

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.

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.

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 x

### ***Statistical procedures and conventions***

All analyses will be conducted using the R program. A random-effects analysis (REM) is chosen as the hierarchical linear model. This decision to employ a REM was made for two reasons. Firstly, the true effect would vary from study to study due to the distribution of effects. These variances may include: the setting of the intervention, the training of the person delivering the program or the dosage of the intervention. Secondly, under the random-effects model the weights assigned to each individual study are more reasonable as it considers that the effect observed within each study are based on a sample from a population with an unknown mean.

Meta-analysis will be conducted to test effectiveness of interventions to improve HSC access across various domains relating to homelessness. The outcomes related to homelessness are continuous and so the effect size metric chosen is Hedges' *g*, many studies will need to be recalculated into a Standardised Mean Difference (SMD) with a 95% confidence interval to allow appropriate summary of effect sizes across the included studies. SMD will be calculated from means and standard deviations in the first instance, however, if a study does not provide this raw data, authors will be contacted, and this information will be requested. Failing this, many papers have been published to assist authors in calculating the SMD from primary research (Rosnow & Rosenthal, 1996; Rosnow, Rosenthal & Rubin, 2000), and have enabled authors to transform many statistical tests of significance such as t-tests, F tests, and chi square values to a metric which allows comprehension of the magnitude of the

can square values to a metric which allows comprehension of the magnitude of the intervention effect. A very useful online calculator has also been developed, this allows authors to choose the type of raw data available, and the calculator will automatically transform this to various effect size types, including the SMD (Lipsey & Wilson, 2000).

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.

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.

If it transpires that there is substantial heterogeneity between studies, authors understand it is not suitable to combine these in a meta-analysis as the experimental effects are more different than one would expect based on chance alone. Statistical heterogeneity or lack thereof will be checked in several ways. Firstly, visually using forest plots and checking for overlap of confidence intervals. Secondly, using tests such as the Cochran Q test (Chi-Square or  $\chi^2$ ), percentage of total variation across studies ( $I^2$ ) and the Tau-squared statistic ( $\tau^2$  or Tau2).

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 were 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.

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).

Two subgroup analyses will then be conducted in relation to each of the three factors above (age and gender) 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 each other in all other respects, such as whether 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.

### ***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 HSC 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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## Review authors

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## **Roles and responsibilities**

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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 Ciara Keenan will be responsible for the day to day operation of the review. This review will be supported by specialist input from Dr Jennifer Hanratty alongside research support from two full time research assistants.

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.

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 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.

Alan Maddock is a Lecturer in Social Work at Queen's University, Belfast. Alan is a qualified social worker who worked within homeless services in Dublin, Ireland from 2009-2015. Alan recently completed his PhD in Psychology with Trinity College, Dublin. This PhD was funded by the Health Research Board as part of the SPHeRE scholarship programme. This research focussed on how mindfulness interventions work using innovative mixed methodologies. Alan has published research on the effectiveness of mindfulness interventions on important mental health and addiction outcomes for homeless service users. Alan has a strong research interest in homelessness, specifically; the effectiveness of psychosocial interventions with marginalised groups, and improving access to evidence based supports for people who are marginalised.

- Content: AM
- Systematic review methods: CK, SM, JH
- Statistical analysis: CK, SM, JH
- Information retrieval: CK, SM, JH

### **Sources of support**

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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.

### **Declarations of interest**

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Please declare any potential conflicts of interest. For example, have any of the authors been involved in the development of relevant interventions, primary research, or prior published reviews on the topic?

### **Preliminary timeframe**

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Approximate date for submission of the systematic review.

- Date you plan to submit a draft protocol: 31 Jan 2019
- Date you plan to submit a draft review: 27 Sep 2019

### **Plans for updating the review**

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Reviews should include in the protocol specifications for how the review, once completed, will be updated. This should include, at a minimum, information on who will be responsible and the frequency with which updates can be expected.

### **AUTHOR DECLARATION**

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#### ***Authors' responsibilities***

By completing this form, you accept responsibility for preparing, maintaining and updating the review in accordance with Campbell Collaboration policy. Campbell 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 Campbell.

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. Campbell 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 Cochrane 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: Ciara Keenan Date: 28/03/19**