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# Housing improvements for health and associated socio-economic outcomes

## PROTOCOL

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THE CAMPBELL COLLABORATION

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### **What's new**

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## **Abstract**

### **Background**

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# Plain language summary

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# Background

## Description of the condition

Hundreds of studies have investigated the health of populations and their housing conditions, resulting in a body of evidence which displays strong associations between poor health and poor housing ([Bonney 2003](#); [Fuller-Thomson 2000](#); [Holmes 2000](#); [Hopton 1996](#); [Humfrey 1996](#); [Hunt 1993](#); [Macintyre 2003](#); [Martin 1987](#); [Peat 1998](#); [Raw 1995](#); [Raw 2001](#); [Revie 1998](#); [Wilkinson 1998](#); [Wilkinson 1999](#)). Despite this, there remains some ambiguity about the strength of evidence and also the nature of the link between poor housing and poor health ([Dunn 2000](#); [Howden-Chapman 2002](#); [Thiele 2002](#)). This may be largely explained by the inextricable links between poor housing and other determinants of poor health such as poverty and pre-existing poor health. For example, vulnerable groups such as the sick, the elderly, and the unemployed are among those most likely to live in poor housing and who also tend to spend long periods of time indoors exposed to potentially hazardous environments ([BMA 2003](#)).

Poor housing conditions may comprise a number of factors and the prevalence and relevance of specific factors may vary according to context. For example, temperature control is related to health. In colder countries there is a need to provide adequate, affordable warmth while in warmer countries the emphasis may be on keeping occupants cool in hot summers.

The aspects of poor housing which are most commonly linked to adverse health outcomes ([Raw 2001](#)) are detailed in Box 1 (UK data).

**Box 1. Most significant housing hazards associated with health effects \* (Box 1a) plus type of health effects commonly linked to poor housing (Box 1b)**

<b>Box 1a</b>	<b>Box 1b</b>
Air quality (particles and fibres causing death among the very ill)	<i>Respiratory symptoms, asthma, lung cancer</i>
Hygrothermal conditions (warmth and humidity)	<i>Depression and anxiety</i>
Radon	<i>Injury or death from accidents and fires</i>
Slips, trips and falls	<i>Hypothermia</i>
Noise	<i>Skin and eye irritation</i>
House dust mites	<i>General physical symptoms</i>
Environmental tobacco smoke	

Fires	
*seriousness of hazard assessed and ranked by number of people affected, seriousness of effect and strength of evidence	

## Description of the intervention

Poor housing is both an indicator of poverty and a common target for interventions to improve public health and reduce health inequalities ([Gauldie 1974](#)). For example the WHO Knowledge Network on Urban Settings and the WHO Commission on the Social Determinants of Health have highlighted the need to create healthy housing and healthy neighbourhoods for future health ([Kjellstrom 2007](#)). Within public health more generally, housing policy is regularly cited as both a determinant of health and health inequalities ([Shaw 2004](#); [Thiele 2002](#)) and a means by which inequalities may be tackled ([Best 1999](#); [Howden-Chapman 2002](#)).

Interventions to improve housing conditions may involve changes to the physical fabric of the housing, equipment and educational interventions to reduce exposure to hazards, in particular air pollutants and allergens, and to reduce domestic injury. This review will focus on interventions to improve the physical fabric of housing. These interventions vary and may comprise demolition of substandard slum housing and rehousing of occupants to new build housing with modern facilities; refurbishment of existing housing; remediation of damp or mould problems; provision, repair or upgrading of heating or energy efficiency measures such as insulation.

## How the intervention might work

The well-established associations between poor housing and poor health suggest that housing improvement may well be justified on health grounds alone. Interventions to upgrade the housing fabric typically involve substantial changes to housing and may affect, intentionally or not, exposure to a range of potential hazards. For example, energy efficiency measures may result in improved warmth, elimination or containment of mould or damp, and improved air quality as well as reduced fuel costs. It is hypothesised that reduction in exposure to housing conditions associated with poor health will result in health improvement, although the timescale for the impact on health is not clear and may not be immediate. In addition, associated socio-economic factors may mediate between the potential for health improvement and housing improvement. Thus improved housing conditions may be regarded as an intervention which can tackle the complex dynamic between poverty and poor health.

## Why it is important to do this review

Much of the existing research investigating the links between housing and health has been cross-sectional. These studies have often demonstrated strong independent associations between housing conditions and health; however the lack of control for confounders means that their results remain open to debate and interpretation ([Wilkinson 1999](#)). In addition, reports of links between poor housing, deprivation and ill health may have only a limited role in informing specific policy decisions around the nature of investment or housing improvement required to improve health ([Maclennan 1999](#); [Thunhurst 1993](#)).

Experimental studies of the health impacts of housing would provide stronger evidence. However, the experimental approach to housing research has been criticised for being reductionist and ignoring the multi-factorial nature of causality in housing, deprivation and health ([Hunt 1993](#)). In addition to this objection there are substantial methodological, pragmatic and ethical obstacles to the conduct of trials in this field. The key issues are outlined below. Principles of social justice dictate that it would be unethical to withhold an available benefit, such as improved housing, from those deemed eligible simply for the purposes of research. Randomisation may only be justifiable where there is a natural delay or waiting list in distributing the housing improvement to eligible participants ([Thomson 2004](#)). Such studies are rare. It is most often impossible to blind participants or assessors to the allocation to intervention group or control group, resulting in high levels of recall bias ([Rothman 1998](#)). In cases where randomisation is not possible, identifying a suitable control group which is similar both socio-demographically and in terms of eligibility for a housing improvement is difficult. There may be a time delay between exposure to a housing hazard and emergence of the health effect. Furthermore, housing improvements are often accompanied by wider neighbourhood improvements and it is therefore difficult to attribute changes in outcomes to housing improvement alone.

Although experimental and quasi-experimental trials of housing improvement may still be possible, the issues raised above may partly explain why trials of housing improvements, randomised or not, have rarely been conducted. In light of these problems and the current lack of data from randomised trials, it would appear that data from small uncontrolled studies may be considered valuable to establish the nature and extent of possible health impacts following housing improvement.

### Previous reviews on this topic

A number of reviews have examined the strength of association between housing specific hazards and health ([Institute of Medicine 2004](#); [Peat 1998](#); [Rauh 2008](#); [Raw 2001](#); [Revie 1998](#); [Wilkinson 1999](#)). A recent review of reviews identified nine systematic reviews of housing related interventions which had examined impacts on health outcomes and health inequalities ([Bambra 2008](#);

[Bambra 2010](#)). Three of these reviews were of measures (including equipment and exercise regimes) to reduce falls at home amongst the elderly ([Chang 2004](#); [Gillespie 2003](#); [McClure 2005](#)); two reviews involved community and housing based interventions to reduce community and domestic injury ([Nilsen 2004](#)) and firearm injury ([Hahn 2005](#)); two reviews were of rental assistance programmes ([Acevedo-Garcia 2004](#); [Anderson 2003](#)); one review examined UK investment in area based renewal, some of which included housing-led renewal ([Thomson 2006](#)); and one review assessed the health impacts of physical improvements to the housing fabric ([Thomson 2001](#)). These latter two reviews ([Thomson 2001](#); [Thomson 2006](#)) were conducted by the authors of this protocol.

Other systematic reviews of housing interventions for health which we have identified, including Cochrane reviews, have focussed on equipment or behavioural interventions, or both, to reduce exposure to allergens amongst asthmatics ([Gøtzsche 2008](#)) and to reduce domestic injury and fires ([DiGuseppi 2000](#); [Kendrick 2007](#)[Lyons 2006](#)). Two further reviews conducted in the USA have been identified ([Jacobs 2009](#); [Saegert 2003](#)). Neither of these reviews were systematic reviews, and they focussed on interventions aimed at minimising exposure to specific hazards, for example pest management, cleaning treatments, dehumidifiers and behavioural interventions to reduce domestic injury. In addition, both these reviews were limited to US literature.

The 2001 review by Thomson et al is the only international systematic review of improvements to the physical fabric of housing which has been identified to date ([Thomson 2001](#)). The review, conducted in 2000, included all quantitative studies of housing improvement, of any design, which took a measure of health, illness or wellbeing; 18 completed studies and 14 ongoing studies were identified. Of the 18 completed studies, eight were identified from electronic databases including databases of unpublished literature. The remaining 10 studies were identified through personal communication, conference attendance and handsearching bibliographies of books. Of these 10 studies, eight were conducted in the UK and two in the USA ([Thomson 2002](#)). Although this distribution between studies in the UK and the USA reflects the distribution of study locations identified through the electronic databases, it is possible that unpublished studies from beyond the UK were missed and this may have introduced some bias into the review. Many of the ongoing studies identified are now due for completion and an update to this review is required. Extra efforts to identify unpublished studies carried out beyond the UK will be made.

A Cochrane review of remediation of damp and mould to buildings, including housing, is also currently underway.

## Objectives

To assess the health and social impacts on residents following improvements to the physical fabric of housing.

## **Methods**

### **Criteria for considering studies for this review**

#### ***Types of studies***

Prospective, retrospective, controlled, uncontrolled, randomised (including cluster randomised trials) and non-randomised studies of the health and social effects of housing improvements will be included in the review. Cross-sectional studies that do not investigate the effects of housing improvement will not be included, that is cross-sectional surveys reporting associations between housing conditions and health. Intervention studies reporting both quantitative and qualitative data will be reported in the review. The study designs and names used to describe study designs are defined in [Appendix 1](#).

#### ***Types of participants***

The review will not exclude any participants on the basis of family type, socio-economic status or other equity indicators such as race or ethnicity, occupation, education or religion. Studies from any region of the world and from both industrialised and non-industrialised countries will be included. Outcomes for both adults and children will be included in the review.

Included participants will be in receipt of a discrete programme of rehousing or housing improvement. Where households experience a change of housing conditions as an indirect result of some other life event, for example employment relocation, natural disaster, and the housing improvement is not part of a discrete programme these participants and the studies will not be included.

#### ***Types of interventions***

All physical house types which are static (that is not caravans or house boats) will be included, this may include residential establishments providing permanent accommodation and sheltered housing. Housing interventions will be defined as rehousing and any physical change to housing infrastructure, for example heating installation, insulation, double glazing and general refurbishment where aspects of the housing fabric is improved. Physical improvements tailored to meet the needs of the resident will be included, for example medical priority housing. Where these improvements are limited to provision of indoor furniture or equipment, such as vacuuming, mattresses and air purifiers, these will be excluded. Studies which provide no specific information on the nature or extent of the physical housing improvement or focus on non-physical aspects of being rehoused will be excluded. For example, a study may report on the health effects of former residents of supported living quarters being relocated to live

independently. It may be mentioned that the physical quality of the new housing is superior to previous accommodation but details of what the actual physical improvements are may be omitted as the intervention of interest to such a study is primarily the move to independent living. Such a study would be excluded.

Studies will be included if they have investigated changes in health, illness or wellbeing related outcomes among the residents following the delivery of a discrete housing improvement programme which has been delivered following and as a consequence of a natural disaster or labor migration. It is possible that following and as a consequence of such an event some of the population will live in improved housing. However studies will not be included where the study investigates the health and socio-economic effects of an event such as a natural disaster or economic migration but where no discrete programme of housing improvement has been delivered to the population.

Environmental studies of the adverse effects of lead, urea formaldehyde foam, air quality, allergens or radon will not be included. These studies assess the impact of exposure to the potential hazard rather than any impact of housing improvement. In addition, evidence of the harmful effects of radon, lead and asbestos are now accepted ([Wilkinson 1999](#)).

## **Types of outcome measures**

### **Primary outcomes**

Outcome measures will include any measure which can be interpreted as a direct measure of health, or mental and physical illness, general measures of self-reported wellbeing and quality of life measures.

Data on health service use will be extracted and reported but will not be included in the final synthesis. Studies which only report health service use outcomes will be reported to ensure provision of a comprehensive list of studies which have assessed health related outcomes. These studies will not be included in the final synthesis.

There will be no minimum follow-up period to assess health effects. Where a study reports on health impacts at multiple time points all impacts will be extracted and reported. The final impact will be used as the study's findings. In the case where synthesis across more than one study is possible, the outcomes from the most similar time point across the studies will be used.

### **Secondary outcomes**

Additional social and socio-economic outcomes which can be interpreted as determinants of health will be extracted, where reported; for example fuel costs, household income, measures of social contact, social exclusion, education, employment, time off work.

## Search methods for identification of studies

### Electronic searches

The following electronic bibliographic databases will be searched with no restriction on language. The following includes databases considered to be relevant to the issue of health equity.

- ASSIA (1987 to present) (CSA)
- Sociological Abstracts (1963 to present) (CSA)
- International Bibliography of the Social Sciences (1951 to present) (BIDS)
- Cochrane Central Register of Controlled Trials (*The Cochrane Library* current Issue) ([www3.interscience.wiley.com/cgi-bin/mrwhome/106568753/HOME](http://www3.interscience.wiley.com/cgi-bin/mrwhome/106568753/HOME))
- Campbell Collaboration Social, Psychological, Educational and Criminological Trials Register (C2-SPECTR) (1950 to present) (<http://geb9101.gse.upenn.edu/RIS/RISWEB.ISA>)
- MEDLINE (1966 to present) (Ovid)
- CINAHL (1982 to present) (Ovid)
- EMBASE (1980 to present) (Ovid)
- PsycINFO (1872 to present) (Ovid)
- MEDLINE In-Process and Other Non-Indexed Citations (Ovid)
- Social Science Citations Index (1981 to present) (ISI Web of Knowledge)
- CAB Abstracts (1973 to present) (Ovid)
- PAIS International (Public Affairs Information Service) (1976 to present) (Dialog)
- ICONDA International Construction (1976 to present) (Dialog)
- Architecture (1987 to present) (Dialog)
- DH-DATA: Health Admin, Medical Toxicology and Environmental Health (1983 to present) (Datastar)
- Global Health (1973 to present) (Ovid)
- Science Citations Index expanded (1981 to present) (ISI Web of Knowledge)
- SIGLE (GB records only) British Library in-house interface (with thanks to British Library staff)
- Avery Index to Architectural periodicals
- RIBA (Royal Institute of British Architects) library catalogue

An example of the search strategy illustrating the search terms to be used is available in [Appendix 2](#). The strategy and combination of terms used will be amended as required for each database. The search strategy will not be limited with respect to population characteristics such as age, gender, language, or race. The search strategy will include terms relating to public provision of housing aimed at low-income populations.

### Searching other resources

Bibliographies of screened papers and identified reviews will be searched for eligible studies. Efforts to identify relevant grey literature will include contacting experts, searching SIGLE/COPAC, handsearching IDOX (formerly PLANEX), and searching relevant websites both within the UK and beyond.

A list of experts from the lead review authors' own contacts and authors of housing studies will be contacted by e-mail to request any information about completed or ongoing studies which might be relevant to the review.

## **Data collection and analysis**

### ***Selection of studies***

The results of the searches will be screened independently by two review authors to identify studies which meet the review's inclusion criteria. The initial screening will be based on study title and abstract. Where there is disagreement or ambiguity about inclusion the full reference will be obtained to allow further scrutiny of the eligibility of the study. The review authors will meet to discuss studies where there is disagreement over inclusion or exclusion of a study.

### ***Data extraction and management***

Citations will be stored in EndNote (bibliographic software). Assessment of risk of bias will be conducted by two review authors independently and disagreements resolved by discussion. The reported findings from each study will be extracted by one review author and checked by a second review author with disagreements or inaccuracies to be discussed between the authors. All data will be entered into an Access database and checked by a second review author. The final agreed data extraction will be entered into RevMan by one review author. A list of data extraction fields is available in [Appendix 3](#).

The data extraction will include extraction of intervention context and the socio-demographic characteristics of the study sample, such as gender, race, age, and socio-economic status.

## ***Assessment of risk of bias in included studies***

### **Quantitative studies**

We will complete the Cochrane risk of bias tool for each study. It is expected that this tool will not be sensitive to the variations in study quality across the various study designs included in this review, such as non-randomised studies and uncontrolled studies. For this reason, studies will also be assessed for risk of bias using a critical appraisal tool developed by a group of systematic reviewers in Hamilton, Canada (Hamilton Assessment Tool) ([Thomas](#)) and that has been recommended by the Cochrane Public Health Group for use in reviews of public health interventions where non-randomised studies are included ([Armstrong 2008](#)). We propose amending the Hamilton Assessment Tool to ensure that it is

appropriate to studies of housing interventions, for example by including an assessment of key confounders accounted for beyond socio-demographics, such as eligibility for housing improvement and housing condition at baseline. Also, the Hamilton Assessment Tool (HAT) does not differentiate between prospective controlled study designs and other non-randomised study designs; we propose to amend the tool to allow distinctions between controlled and uncontrolled study designs. Our proposed amended HAT to assess risk of bias is presented in [Appendix 4](#). Using this tool, each study will be assessed for the extent of bias introduced to the study with regard to selection of study population, study design, control for confounding, data collection measures and methods, blinding of assessor and participants, and withdrawals by final follow up. Each of these potential areas of bias will be graded as A, B, or C (A indicating minimal potential bias and C indicating considerable potential for bias) according to the criteria outlined in [Appendix 4](#).

The quality assessment for each study will be carried out by two independent review authors and entered onto a Microsoft Access® database. Disagreements in any one of the six points of assessment (selection, study design, confounding, data collection, blinding, withdrawals) will be resolved through discussion between the two review authors.

Each study will be assigned to a summary category (A, B, or C) indicating the overall potential for bias. The criteria for this summary category are outlined in [Appendix 4](#).

### **Qualitative studies**

Qualitative studies, including studies reporting qualitative data supplementary to quantitative data, will be included in the review. There is much unresolved debate about appropriateness of assessments of the quality of qualitative studies and their data. Despite this, it is important to present details of the study design, sample, and data collection methods, as well as an indication of the review authors' appraisal of the validity of the reported findings and their interpretation.

Data on the study aims and methods, including sampling details and data collection methods, will be extracted and tabulated to provide an overview of the study design and methods. In addition, a critical appraisal tool developed for qualitative studies and previously recommended for use in systematic reviews will be used, such as the tool developed by the Joanna Briggs Institute.

### **Intervention implementation and performance bias**

Variation in the ways in which an intervention is implemented may introduce bias and explain variance in the reported effects within a study (Type III error) ([Dobson 1980](#)). This may be referred to as performance bias.

It cannot be assumed that the housing improvements were implemented as originally planned, or that all recipients of the intervention used the intervention in the same way. Variation in intervention implementation may result in variation in exposure to the critical changes that the intervention aims to affect and will result in variation in the potential to benefit within a study. For example, the extent of housing improvement may be tailored according to individual household need and will vary, therefore, by the housing condition at baseline. In addition, delivery of a housing improvement may not result in exposure to improved housing conditions. For example, fear of costly fuel bills may prevent use of a new central heating system, or if an intervention is implemented without assessment of need there may be households where the potential to improve housing conditions is limited if housing conditions are satisfactory at baseline.

Included studies will be assessed for within study heterogeneity with respect to intervention implementation and also for heterogeneity in the extent of improvement in housing conditions actually experienced by participants (see [Appendix 4](#)).

### ***Measures of treatment effect***

Comprehensive Meta-Analysis (CMA) software© will be used to calculate standardised effect sizes for all health outcomes from controlled studies which report necessary data. These outcomes will be likely to include continuous and dichotomous variables and the standardised effect will be reported as an odds ratio (OR) and 95% confidence Interval (CI).

### ***Unit of analysis issues***

Housing interventions are allocated and implemented at a household level either to individually targeted households meeting pre-specified eligibility criteria or to all households within a targeted geographical area. However, health outcomes are assessed at an individual level.

In some studies health outcomes are only assessed for one occupant, and in others health outcomes are assessed for more than one or for all occupants (though these assessments may be made on behalf of other occupants by a nominated occupant). The sample type is likely to vary across the identified studies. We will extract all reported health and socio-economic outcomes for however many occupants have been included in the study. Where a study presents different data for different occupant types, the categories are likely to be adult or child; adult: gender, diagnosed with specific illness for example asthma or not diagnosed. Other subgroups of interest with respect to equity indicators are race or ethnicity, occupation, socio-economic status, education, religion. For the main analysis child and adult data will be reported and analysed separately. Data and analysis on other subgroups mentioned above, in particular

those with equity implications, will be extracted, reported and synthesised where there are sufficient, similar data.

### ***Dealing with missing data***

We will contact authors of studies to obtain missing data. We will report withdrawals and levels of attrition for each study and incorporate these into the overall indication of study quality. CMA will be used to calculate standardised effect sizes for controlled studies which report the necessary data.

### ***Assessment of heterogeneity***

Statistical heterogeneity will be assessed using the  $\text{Chi}^2$  and  $I^2$  statistics. If appropriate, a meta-analysis of effect sizes will be conducted using a fixed-effect model, otherwise a random-effects model will be considered.

Heterogeneity within and between the studies will be investigated and reported with respect to study design, study quality, intervention, context, and implementation of the intervention. See the section on 'subgroup analysis and investigation of heterogeneity' for a more detailed description of how heterogeneity between the studies will be dealt with, and also 'intervention implementation and performance bias' for details of how heterogeneity with respect to implementation and performance bias will be assessed.

### ***Assessment of reporting biases***

We will investigate the impact of publication bias by preparing a funnel plot and calculating Egger's test if there are sufficient studies which report standard errors for the effect sizes.

### ***Data synthesis***

#### **Quantitative data**

Data from the better quality studies will be synthesised and the final synthesis will reflect the relative weight of evidence within each group of studies.

It is anticipated that there will be extreme levels of heterogeneity within the collection of studies identified. It has previously been recommended that measures to overcome heterogeneity should be taken, where possible, to facilitate a meta-analysis. These measures include calculation of standardised effect sizes, grouping of studies appropriately with respect to interventions and outcomes, and use of a random-effects model ([Ioannidis 2008](#)). Effect sizes from studies will be calculated for all prospective controlled studies identified using Comprehensive Meta-Analysis software©. Where data for similar outcomes following similar housing improvements (outcomes and interventions grouped as outlined in 'subgroup analysis and investigation of heterogeneity') are available these effect sizes will be pooled. Where the same continuous outcome is reported across more than one study a standardised mean

difference will be computed (using the Comprehensive Meta-Analysis© software). These will be combined in a meta-analysis to calculate a weighted mean difference. Where a dichotomous variable is reported, an odds ratio will be calculated. Heterogeneity will be assessed using  $\text{Chi}^2$  and  $I^2$  statistics. If appropriate, a meta-analysis of effect sizes will be conducted using a fixed-effect model, otherwise a random-effects model will be considered for use. Where effect sizes are not amenable to pooling due to extreme heterogeneity, a forest plot will be used to illustrate the range of effect sizes. Where the outcomes within a category are similar but not the same, for example different measures of respiratory health, and they are a mix of continuous and dichotomous variables we will present effect sizes as odds ratios and present them in a forest plot to allow all the effect sizes to be shown together. The raw data from both dichotomous and continuous outcomes will be transformed using the Comprehensive Meta-Analysis© software. Results of experimental and quasi-experimental studies will be analysed separately.

For groups of studies where a statistical synthesis of the data is not appropriate the data will be synthesised narratively according to the ESRC guidance ([Popay 2006](#)). The main steps of the narrative synthesis will involve articulating a theory of how housing improvement might lead to health impacts (see 'How the intervention might work'), conducting a preliminary synthesis to test the theory, exploring the relationships in the data (within and between similar studies), and assessing the robustness of the synthesis. The data from each study will be tabulated to provide a textual as well as a visual summary of the data; this will help facilitate the synthesis by illustrating emerging patterns with respect to reported impacts and study characteristics as well as improving the transparency of the synthesis.

To present a clear demonstration of what studies have been identified, how these studies compare to better quality studies, and why they have not been included in the final assessment, a narrative description of all studies, regardless of study design, will be included in the final review with an accompanying tabulation of all studies and reported impact data. This information will provide a definitive map of intervention research in this field and help to point to how future research design could be improved within this field.

### **Qualitative data**

The synthesis of data from multiple qualitative studies has been contested as contrary to the qualitative methodological approach and epistemology. It has been argued that essential differences between studies with respect to theoretical and methodological foundations means that to synthesise data will overlook the strengths and values of the data that emphasise the importance of specific contexts, individual experiences and attached meanings. However, others argue that qualitative data can uncover impacts not predicted or

detectable by quantitative studies and also shed light on important confounding factors and pathways which may help explain the variance in predicted health impacts. Importantly, these data may be generalisable to other similar contexts, populations and interventions.

It is expected that the qualitative studies will be heterogeneous with respect to intervention, context and population, as well as methodology and study quality. For these reasons we propose to conduct a narrative synthesis of the qualitative data in accordance with the ESRC guidance on narrative synthesis ([Popay 2006](#)). The findings from each study will be tabulated to provide a textual summary of the data. This will facilitate a thematic analysis and examination of emerging themes with respect to reported impacts, mediating factors, and pathways affecting health impacts.

### ***Subgroup analysis and investigation of heterogeneity***

We will assess heterogeneity for different aspects including statistical heterogeneity. With respect to heterogeneity of interventions, the synthesis will be carried out for groups of studies which include similar interventions, as described below.

#### **Study heterogeneity: methods, intervention, population, context, and outcomes**

The broad scope of this review will inevitably mean that the identified studies will display extreme variance in the methods used, the interventions being assessed, the study populations, and contexts in which the intervention is being implemented; and the potential range of illness, health and wellbeing outcomes being assessed. In addition to details of the intervention, study sample and study methods, and details of the local context such as rurality, slum conditions will be extracted, where available. We propose to group the interventions into broad categories of the type of housing intervention and, if appropriate and there is a sufficient number of studies, according to the context and population of the study. For example, studies relating to modern day housing interventions in wealthy countries, such as insulation and central heating, will be grouped and analysed separately from older studies of rehousing from slum neighbourhoods. The reported outcomes will also be grouped into broad categories, for example respiratory health, general health and mental health.

Data will be presented according to the appropriate groups for intervention, context and population, where possible. In addition, the reported data will be accompanied by an indication of study design, overall study quality, different aspects of potential for bias, and also an indication of intervention integrity (see above 'Intervention implementation and performance bias').

#### **Statistical heterogeneity**

Where there are substantial levels of statistical heterogeneity (> 50%) the data will be checked for accuracy. If statistical heterogeneity persists the data will be meta-analysed using a random-effects model. If substantial heterogeneity persists the standardised effect data will be presented on a forest plot but a meta-analysis will not be performed.

### **Investigation of equity and differential impacts across population subgroups**

The studies in this review are expected to focus largely on low-income populations living in poor quality housing, including publicly provided housing. Knowledge of impacts on low-income populations is important with respect to improving the health of the worst off and may indicate the potential for housing improvements to impact on health inequalities. However, assessments of and data on variations in impact across different socio-economic groups are needed to confirm whether or not an intervention is likely to impact on the gap in health status between high and low income groups.

Where available, data for specific population subgroups will be extracted and reported separately, for example where impacts are reported by gender, socio-economic status, educational status, or religion. Where sufficient, similar data on specific subgroups are available we will consider synthesising and presenting these data separately to illustrate the differential effects for different subgroups.

### ***Sensitivity analysis***

Before making decisions about which studies should be included in the final syntheses, a sensitivity analysis will be conducted to examine variation in reported effects by study characteristics. This will be done for each outcome category (see 'subgroup analysis and investigation of heterogeneity') where data are available. The sensitivity analysis will require using the standardised effect measure and examination of heterogeneity and variation in heterogeneity when only studies with minimal risk of bias are included in the meta-analysis. The key study characteristics used for the sensitivity analysis will be the six potential sources of bias (study design, confounding, selection, data collection, blinding, and withdrawals) as well as the overall study quality grade assigned. Some other study characteristics may be identified for inclusion in the sensitivity analysis during the course of the review.

## **Results**

### **Description of studies**

#### ***Results of the search***

#### ***Included studies***

***Excluded studies***

**Risk of bias in included studies**

***Allocation***

***Blinding***

***Incomplete outcome data***

***Selective reporting***

***Other potential sources of bias***

**Effects of interventions**

## **Discussion**

**Summary of main results**

**Overall completeness and applicability of evidence**

**Quality of the evidence**

**Potential biases in the review process**

**Agreements and disagreements with other studies or reviews**

## **Authors' conclusions**

**Implications for practice**

**Implications for research**

## **Acknowledgements**

We would like to thank the referees and editors from both the Cochrane and Campbell Collaborations for helpful comments on earlier drafts of this protocol. We would also like to thank NHS Centre for Reviews & Dissemination, York, UK for help in developing the initial search strategy.

## **Contributions of authors**

HT is the lead review author and will lead all aspects of the review. ST and ES are co-reviewers and will screen, critically appraise, extract data, and approve the final synthesis for the review. MP will advise on the methods of the review.

## **Declarations of interest**

HT and MP have previously conducted a systematic review of housing improvement (Thomson H, Petticrew M, Morrison D. Health effects of housing improvement: systematic review of intervention studies. *BMJ* 2001;323(7306):187-90). MP is an editor on the Cochrane Public Health Group (but not involved in the editorial approval of this review).

## **Differences between protocol and review**

### **Published notes**

A previous protocol for this review had been peer reviewed and approved by the Campbell Collaboration: Thomson H, Petticrew M. Assessing the health and social effects on residents following housing improvement: a protocol for a systematic review of intervention studies. International Campbell Collaboration approved protocol ([www.campbellcollaboration.org/doc-pdf/housingimpprot.pdf](http://www.campbellcollaboration.org/doc-pdf/housingimpprot.pdf)), 2004. This current, modified protocol is now co-registered within the Campbell Collaboration.

## **Characteristics of studies**

### **Characteristics of included studies**

*Footnotes*

### **Characteristics of excluded studies**

*Footnotes*

### **Characteristics of studies awaiting classification**

*Footnotes*

### **Characteristics of ongoing studies**

*Footnotes*

## **Summary of findings tables**

### **Additional tables**

## **References to studies**

### **Included studies**

### **Excluded studies**

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**Classification pending references**

**Data and analyses**

**Figures**

**Sources of support**

**Internal sources**

- Chief Scientist Office, Health Department, Scottish Government, UK

**External sources**

- Nordic Campbell Collaboration (NC2), Norway One month funding for co-reviewer, Norway

**Feedback**

**Appendices**

**1 Definition of study design names used in the review**

***Experimental, randomised study designs***

Randomised controlled trial (RCT): the study sample comprises all those eligible for the intervention. The intervention is delivered to those selected at random to receive the intervention during the study period, this group is the intervention group; those who do not receive the intervention act as a suitable comparison or control group. Key outcomes are assessed before and after delivery of the intervention in both the intervention and the control groups. Changes in the key outcomes are analysed comparing changes among the intervention group and the control group.

Cluster randomised controlled trial: this design is similar to the above design (RCT) but instead of individuals being randomised to receive the intervention the unit of randomisation is a group for example a school, a neighbourhood, or a street.

***Observational study designs, non-randomised study designs***

Prospective controlled study: the intervention is not randomised. The key outcome is assessed among the study population before and after receipt of the intervention. The change in outcome is compared with the same outcome measurements and changes in a suitable comparison group acting as a control group who have not received the intervention. It is likely that there will be systematic differences in eligibility for the intervention between the intervention and the control group. The key outcome is assessed at the same time points in the intervention and the control group. This design may be referred to as a quasi-experimental design and may also be known as a controlled before and after study (CBA) or a controlled prospective cohort study.

Prospective uncontrolled study: the key outcome is assessed among the study population before and after receipt of the intervention but there is no comparison or control group. This design may also be known as an uncontrolled before and after study or an uncontrolled prospective cohort study.

Retrospective controlled study: changes in the key outcome since delivery of the intervention are assessed retrospectively and the study population is identified after the intervention has been delivered. The key outcome may be assessed using data collected before the intervention for another purpose for example routine data, or relying on recall of baseline status before receipt of the intervention. Retrospective changes in the key outcome are assessed and compared with similar measurements in a suitable comparison group. For the purposes of this review, and to distinguish retrospective controlled studies from case-control studies, the intervention group will comprise those in receipt of housing improvements which are part of a discrete programme of housing improvement or rehousing delivered at a similar time point.

Retrospective uncontrolled study: this design is similar to a retrospective controlled study but there is no comparison or control group. For the purposes of this review the intervention group will comprise those in receipt of housing improvements that are part of a discrete programme of housing improvement or rehousing delivered at a similar time point.

## **2 Example search strategy for MEDLINE**

1. housing/
2. housing for the elderly/
3. public housing/
4. ((renovat\$ or repair\$) adj3 (home or homes or house or houses or housing)).ti,ab.
5. ((mite or mites or rat or rats or mouse or mice or cockroach\$ or vermin or flea or fleas or infest\$) adj3 (home or homes or house or houses or housing)).ti,ab.

6. ((sanitation or sanitary) adj3 (home or homes or house or houses or housing)).ti,ab.
7. ((mold or mould or moldy or mouldy) adj3 (home or homes or house or houses or housing)).ti,ab.
8. ((damp\$ or humid\$) adj3 (home or homes or house or houses or housing)).ti,ab.
9. (heating adj3 (home or homes or house or houses or housing)).ti,ab.
10. ((retrofit\$ or retro fit\$) adj3 (home or homes or house or houses or housing)).ti,ab.
11. (ventilation adj3 (home or homes or house or houses or housing)).ti,ab.
12. (insulat\$ adj3 (home or homes or house or houses or housing)).ti,ab.
13. (refurbish\$ adj3 (home or homes or house or houses or housing)).ti,ab.
14. ((crowd\$ or overcrowd\$) adj3 (home or homes or house or houses or housing)).ti,ab.
15. (double glaz\$ adj3 (home or homes or house or houses or housing)).ti,ab.
16. ((draft\$ or draught\$) adj3 (home or homes or house or houses or housing)).ti,ab.
17. (allergen\$ adj3 (home or homes or house or houses or housing)).ti,ab.
18. Air Pollution, Indoor/
19. indoor air qualit\$.ti,ab.
20. (towerblock\$ or tower block\$).ti,ab.
21. apartment\$.ti,ab.
22. (bedsit\$ or bed sit\$).ti,ab.
23. (highrise\$ or high rise\$).ti,ab.
24. (multistor\$ or multi stor\$).ti,ab.
25. (bungalow\$ or flats).ti,ab.
26. landlord\$.ti,ab.
27. rehaus\$.ti,ab.
28. (homeowner\$ or home owner\$ or tenant\$ or owner\$ occup\$).ti,ab.
29. dwellings.ti,ab.
30. squatter\$.ti,ab.
31. or/1-30

32. (reduc\$ or increas\$ or decreas\$ or evaluat\$ or change\$ or changing or intervention\$ or grow\$).ti,ab.
33. (improv\$ or better or worse\$ or effect\$ or achieve\$ or comfort or morale or harmful or impact\$ or gain\$).ti,ab.
34. 32 or 33
35. ((reduc\$ or increas\$ or decreas\$ or evaluat\$ or change\$ or changing or intervention\$ or grow\$ or (improv\$ or better or worse\$ or effect\$ or achieve\$ or comfort or morale or harmful or impact\$ or gain\$)) adj3 housing).ti,ab.
36. 31 and 34
37. 35 or 36
38. homeless\$.ti,ab.
39. exp homeless persons/
40. animal housing/
41. or/38-40
42. 37 not 41
43. exp research/
44. exp public policy/
45. exp evaluation studies/
46. exp epidemiologic study characteristics/
47. exp clinical trials/
48. (trial or trials or random\$ or controlled or study or studies or intervention\$).ti,ab.
49. (program or programs or programme or programmes or research or policy or policies).ti,ab.
50. quasi experimental.ti,ab.
51. longitudinal\$.ti,ab.
52. prospective.ti,ab.
53. randomized controlled trial.pt.
54. clinical trial.pt.
55. or/43-54
56. 42 and 55
57. animal/
58. human/

59. 57 not (57 and 58)

60. 56 not 59

### **3 Data extraction fields for quantitative and qualitative studies**

#### **List of data extraction fields for quantitative studies**

Unique number

Author

Paper title

Intervention category

Reviewer

Publication Year

Country

Title

Include/Exclude

Individuals likely to be representative of target population?

% and number of selected individuals

How were participants selected?

Summary of Selection Bias

What was the study design?

Was the study described as randomised?

Method of randomisation

Was the method of randomisation described?

Was method appropriate?

Summary of Study Design

Important differences between groups prior to intervention?

Specify differences

Which key confounders were controlled for in main analysis?

How were control and intervention group matched?

In what respects cont/int group can be considered similar?

Summary of confounders

Was outcome assessor aware of intervention or exposure status?

Assessor/participants blinded?

Were study participants aware of research question?

Summary of Blinding

Were validated measures of health used?

Were data collection methods/tools piloted?

Specify methods

Summary of data collection

Withdrawals and drop-outs reported: numbers and reasons per group

Indicate % of participants completing study- final response rate

Specify reasons for drop outs

Are there differences reported between participants and drop-out?

Differences between responders and non-responders?

Summary of Withdrawals and drop outs

What % of participants received allocated intervention or exposure?

Date of Intervention

Intervention: Geographical Location

Setting of intervention

Description of Intervention

Intervention category

Further details

Intervention summary

Details of potential confounding factors

Is it likely that subjects received an unintended intervention?

Variation in type of intervention delivered across study sample?

Any other comments on intervention?

Was the consistency of the intervention measured?

Describe heterogeneity of intervention

Heterogeneity intervention delivered

Heterogeneity in improvement experienced

Details of consultation and implementation of the intervention

Distinct intervention group for reported results

Integrity of intervention summary  
Frequency and timing of follow up  
Total duration of follow-up  
Follow-up details  
Summary of follow-up  
Indicate the unit of allocation  
Indicate unit of analysis  
Are the statistical methods appropriate for study design?  
What attempts were made in analysis to control for key confounders?  
Analysis performed by intervention allocation status or other?  
Final Sample Size  
Baseline/final sample size  
Summary final sample  
What kinds of statistical analysis were used?  
Population characteristics/context  
List all health measures used  
List all housing measures used  
Health improvement (2-3 sentence summary)  
Housing improvement (2-3 sentence summary)  
Health outcomes reported  
Results: Briefly summarise  
Cost data (Yes or No- brief description)  
Sub-group analysis performed?  
Less than 50% households probably at baseline  
Change data for health outcomes  
Data for validated health outcomes reported?  
Control group health data  
Study design re health data presented  
Distinct intervention group  
Authors conclusions  
Sufficient data to validate results

Limitations of study  
Reviewers Comments  
Overall study quality (A/B/C)  
Is there any discrepancy between the two reviewers?  
If yes indicate reason  
Response rate for health data  
Qualitative data (Yes/No)

### **List of database fields for qualitative analysis**

ID  
Author  
Country  
Endnote ref  
Publication year  
Year of interviews  
Intervention Category  
Title  
Supplementary to quantitative study  
Sample size  
Sample selection  
Data collection method  
Data recorded and transcribed  
Aim of qualitative data  
Details of analysis  
Overview of findings  
Short findings  
Intervention  
Time since intervention

## **4 Assessment of risk of bias, overall study quality, and performance bias**

### **Selection bias**

Selected study sample very likely to represent population from  
A

target area AND 80 to 100% response at baseline

Selected study sample very likely to represent population from  
B

target area AND 60% to 79% response at baseline

Not representative of target population OR response less than  
60% C

**Routine data:** *Unless it is stated that individual data were taken from routine data specifically for the study population then studies using routine data labelled*

**Study design** *(based on design used to assess health outcomes, some studies use different designs to assess housing conditions)*

Prospective controlled  
study

A

Prospective uncontrolled, retrospective controlled study  
B

Retrospective uncontrolled/unclear how different groups in study  
C  
analysed

### **Confounders**

Demonstration that intervention and control group are matched for  
A

key confounders (housing quality, socio-economic status, health status, eligibility for improvement) or appropriate control for above key confounders in analysis

Control group matched for two of following: housing quality,  
B

socio-economic status, health status, eligibility for improvement

Analysis controlled for two of above key confounders

Inadequate control for confounders/no control group/control group  
C

very different to intervention  
group

### **Blinding**

Assessor AND participant blind to intervention  
status A

Assessor or participant completely blind to intervention status  
B

Partial/no blinding/unclear/not  
reported C

### **Data collection**

Objective health outcome measure used- validated measures such as  
A

GHQ, HADS, SF-36, or self-reported health (not other self-reported  
symptoms such as asthma), or routine morbidity or mortality data  
Gathered from census of health records e.g. clinic attendance for diarrhoea  
(not health service use) PLUS clear description of established data  
Collection method e.g. postal or interviewer administered questionnaire,  
Interview, telephone interview

Objective health outcome measure (as described above) but unclear  
B

Description of data collection method OR other direct measure of  
Health e.g. self-reported symptoms and clear description of established  
Data collection method (see above)

Direct measure of health BUT Inadequate description of data collection C

Method OR indirect measure of health as main outcome e.g. health service use or prescribing data

**Withdrawals (same as Hamilton)**

80% to 100% of original sample in final sample  
A

60% to 79% of original sample in final sample  
B

Less than 60%/not reported/retrospective study/cannot tell  
C

*(If using routine data which is not linked to individuals or not panel data at end point then C- unless panel data has final response of >60%)*

**Criteria for assignment of overall study quality**

**Overall methodological quality grade (based on above six criteria)**

Prospective controlled study (including RCTs) AND assessed as A or B  
A

in at least two of the remaining three critical appraisal items (sample selection, control of confounding, and withdrawals).

Prospective controlled study AND assessed as A or B in one remaining  
B

critical appraisal criteria (sample selection, control of confounding and withdrawals). Prospective uncontrolled study OR Retrospective controlled study AND assessed as B in two remaining critical appraisal criteria (sample selection, control of confounding & withdrawals).

Prospective controlled study AND assessed to be C in all remaining  
C

critical appraisal criteria (sample selection, control of confounding & withdrawals). Prospective uncontrolled study OR Retrospective controlled study AND assessed as C in two or more remaining critical appraisal criteria (sample selection, control of confounding & withdrawals).

### **Performance bias**

This assessment is based on the level of variance or heterogeneity in the implementation of the intervention across the study population, and also on the variance or heterogeneity in the amount of change in housing conditions experienced across the study population.

#### *Heterogeneity of intervention implementation*

Based on assessment of intervention type and information provided about variation in extent of intervention delivered to residents within the same study

**None/minimal**

**Some**

**Considerable/Not reported/Unclear**

#### *Heterogeneity of change in housing conditions experienced*

**Minimal:** > 80% final sample reported similar type improvements in housing conditions

**Some:** 60% to 80% final sample reported similar type/level of improvement in housing conditions

**Considerable:** < 60% final reported similar type/level of improvement in housing conditions

### **Overall assessment of performance bias**

Minimal heterogeneity in both intervention delivered +  
reported

A

improvement

Minimal heterogeneity in EITHER intervention delivered or  
B

reported improvement AND some heterogeneity in EITHER  
intervention delivered or reported improvement or BOTH  
intervention delivered or reported improvement

Some heterogeneity in EITHER intervention delivered or  
reported C

improvement AND Considerable/Not reported levels of heterogeneity  
in intervention delivered and/or reported improvement