Housing improvements for health and associated socio-economic outcomes (Review)

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[Intervention Review]

Housing improvements for health and associated socio-economic outcomes

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ABSTRACT

Background

The well established links between poor housing and poor health indicate that housing improvement may be an important mechanism through which public investment can lead to health improvement. Intervention studies which have assessed the health impacts of housing improvements are an important data resource to test assumptions about the potential for health improvement. Evaluations may not detect long term health impacts due to limited follow-up periods. Impacts on socio-economic determinants of health may be a valuable proxy indication of the potential for longer term health impacts.

Objectives

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

Search methods

Twenty seven academic and grey literature bibliographic databases were searched for housing intervention studies from 1887 to July 2012 (ASSIA; Avery Index; CAB Abstracts; The Campbell Library; CINAHL; *The Cochrane Library*; COPAC; DH-DATA: Health Admin; EMBASE; Geobase; Global Health; IBSS; ICONDA; MEDLINE; MEDLINE In-Process & Other Non-Indexed Citations; NTIS; PAIS; PLANEX; PsycINFO; RIBA; SCIE; Sociological Abstracts; Social Science Citations Index; Science Citations Index expanded; SIGLE; SPECTR). Twelve Scandinavian grey literature and policy databases (Libris; SveMed+; Libris uppsök; DIVA; Artikelsök; NORART; DEFF; AKF; DSI; SBI; Statens Institut for Folkesundhed; Social.dk) and 23 relevant websites were searched. In addition, a request to topic experts was issued for details of relevant studies. Searches were not restricted by language or publication status.

Selection criteria

Studies which assessed change in any health outcome following housing improvement were included. This included experimental studies and uncontrolled studies. Cross-sectional studies were excluded as correlations are not able to shed light on changes in outcomes. Studies reporting only socio-economic outcomes or indirect measures of health, such as health service use, were excluded. All housing improvements which involved a physical improvement to the fabric of the house were included. Excluded interventions were improvements to mobile homes; modifications for mobility or medical reasons; air quality; lead removal; radon exposure reduction; allergen reduction or removal; and furniture or equipment. Where an improvement included one of these in addition to an included intervention the study was included in the review. Studies were not excluded on the basis of date, location, or language.

Data collection and analysis

Studies were independently screened and critically appraised by two review authors. Study quality was assessed using the risk of bias tool and the Hamilton tool to accommodate non-experimental and uncontrolled studies. Health and socio-economic impact data were extracted by one review author and checked by a second review author. Studies were grouped according to broad intervention categories, date, and context before synthesis. Where possible, standardized effect estimates were calculated and statistically pooled. Where meta-analysis was not appropriate the data were tabulated and synthesized narratively following a cross-study examination of reported impacts and study characteristics. Qualitative data were summarized using a logic model to map reported impacts and links to health impacts; quantitative data were incorporated into the model.

Main results

Thirty-nine studies which reported quantitative or qualitative data, or both, were included in the review. Thirty-three quantitative studies were identified. This included five randomised controlled trials (RCTs) and 10 non-experimental studies of warmth improvements, 12 non-experimental studies of rehousing or retrofitting, three non-experimental studies of provision of basic improvements in low or mIddle Income countries (LMIC), and three non-experimental historical studies of rehousing from slums. Fourteen quantitative studies (42.4%) were assessed to be poor quality and were not included in the synthesis. Twelve studies reporting qualitative data were identified. These were studies of warmth improvements (n = 7) and rehousing (n = 5). Three qualitative studies were excluded from the synthesis due to lack of clarity of methods. Six of the included qualitative studies also reported quantitative data which was included in the review.

Very little quantitative synthesis was possible as the data were not amenable to meta-analysis. This was largely due to extreme heterogeneity both methodologically as well as because of variations in the intervention, samples, context, and outcome; these variations remained even following grouping of interventions and outcomes. In addition, few studies reported data that were amenable to calculation of standardized effect sizes. The data were synthesised narratively.

Data from studies of warmth and energy efficiency interventions suggested that improvements in general health, respiratory health, and mental health are possible. Studies which targeted those with inadequate warmth and existing chronic respiratory disease were most likely to report health improvement. Impacts following housing-led neighbourhood renewal were less clear; these interventions targeted areas rather than individual households in most need. Two poorer quality LMIC studies reported unclear or small health improvements. One better quality study of rehousing from slums (pre-1960) reported some improvement in mental health. There were few reports of adverse health impacts following housing improvement. A small number of studies gathered data on social and socio-economic impacts associated with housing improvement. Warmth improvements were associated with increased usable space, increased privacy, and improved social relationships; absences from work or school due to illness were also reduced.

Very few studies reported differential impacts relevant to equity issues, and what data were reported were not amenable to synthesis.

Authors' conclusions

Housing investment which improves thermal comfort in the home can lead to health improvements, especially where the improvements are targeted at those with inadequate warmth and those with chronic respiratory disease. The health impacts of programmes which deliver improvements across areas and do not target according to levels of individual need were less clear, but reported impacts at an area level may conceal health improvements for those with the greatest potential to benefit. Best available evidence indicates that housing which is an appropriate size for the householders and is affordable to heat is linked to improved health and may promote improved social relationships within and beyond the household. In addition, there is some suggestion that provision of adequate, affordable warmth may reduce absences from school or work.

While many of the interventions were targeted at low income groups, a near absence of reporting differential impacts prevented analysis of the potential for housing improvement to impact on social and economic inequalities.

PLAIN LANGUAGE SUMMARY

Housing improvement as an investment to improve health

Poor housing is associated with poor health. This suggests that improving housing conditions might lead to improved health for residents. This review searched widely for studies from anywhere in the world which had investigated whether or not investment to improve housing conditions is linked with improvement in health. A huge amount of research on housing and health has been

published but very few studies have investigated if improved housing conditions impact on residents' health. Neighbourhood renewal programmes often include housing improvements but a key aim of these programmes is to improve the area by attracting new residents, often those who are better off. In these programmes, improvements in health statistics may simply reflect a change in the population living in an area and the original population may not have benefited from the improved living conditions. This review only looked at studies where changes in health for the original population were being investigated rather than changes for the area.

We identified 39 studies which assessed changes in health following housing improvement. The studies covered a wide range of housing improvements. The housing improvements in high income countries, and conducted in the past 30 years, included refurbishment, rehousing, relocation, installation of central heating and insulation. Studies from the developing world included provision of latrines. Older studies (pre-1965) examined changes in health following rehousing from slums. Overall, it would appear that improvements to housing conditions can lead to improvements in health. Improved health is most likely when the housing improvements are targeted at those with poor health and inadequate housing conditions, in particular inadequate warmth. Area based housing improvement programmes, for example programmes of housing-led neighbourhood renewal, which improve housing regardless of individual need may not lead to clear improvements in housing conditions for all the houses in a neighbourhood. This may explain why health improvements following these programmes are not always obvious.

Improvements in warmth and affordable warmth may be an important reason for improved health. Improved health may also lead to reduced absences from school or work. Improvements in energy efficiency and provision of affordable warmth may allow householders to heat more rooms in the house and increase the amount of usable space in the home. Greater usable living space may lead to more use of the home, allow increased levels of privacy, and help with relationships within the home. An overview of the best available research evidence suggests that housing which promotes good health needs to be an appropriate size to meet household needs, and be affordable to maintain a comfortable indoor temperature.

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 reports strong associations between poor health and poor housing (Bonnefoy 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 they 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)

Box 1a	*Box 1b*
Air quality (particles and fibres causing death among the very ill)	Respiratory symptoms, asthma, lung cancer
Hygrothermal conditions (warmth and humidity)	Depression and anxiety
	Injury or death from accidents and fires

(*Continued*)

Radon Slips, trips, and falls Noise House dust mites Environmental tobacco smoke Fires	Hypothermia Skin and eye irritation General physical symptoms	

** 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 a determinant of health and health inequalities (Shaw 2004; Thiele 2002) as well as having the potential to tackle health inequalities (Best 1999; Howden-Chapman 2002).

Interventions to improve housing conditions may involve changes to the physical fabric of the housing and providing equipment and educational interventions to reduce exposure to hazards, in particular air pollutants and allergens, and to reduce domestic injury. This review focused 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 newly built housing with modern facilities; refurbishment of existing housing; remediation of damp or mould problems; and 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 unclear and it may take years to emerge. 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 for 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. Moreover, most often it is impossible to blind participants or assessors to the allocation to intervention group or control group, resulting in high levels of recall bias in housing intervention studies regardless of study design (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 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 study 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). Two authors of this protocol conducted two of these reviews (Thomson 2001; Thomson 2006).

Other systematic reviews of housing interventions for health which we have identified, including Cochrane reviews, have focused on equipment or behavioural interventions, or both, to reduce exposure to allergens amongst asthmatics (Gøtzsche 2008; Singh 2002) and to reduce domestic injury and fires (DiGuiseppi 2000; Kendrick 2007; Lyons 2006). Two further reviews conducted in the USA have been identified (Jacobs 2009; Saegert 2003). Both these reviews were limited to studies from the US and focused on interventions aimed at minimising exposure to specific hazards, for example pest management, cleaning treatments, dehumidifiers, and behavioural interventions to reduce domestic injury. Moreover, the methods of these reviews were not transparent and it was unclear if or how study quality was considered in the final evidence synthesis. A Cochrane review of remediation of damp and mould in buildings has recently been completed (Sauni 2011). This review was not restricted to housing and included work and school buildings. The bibliography of the review was searched for eligible studies. A protocol for a systematic review of factors affecting the use of 'cleaner fuel' domestic cookstoves in low and middle income countries was identified (Puzzolo 2011). This protocol also pointed to an additional review of the effectiveness of household energy efficiency measures for improved air quality which is currently being conducted by the World Health Organization.

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 included 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 was required. Extra efforts to identify unpublished studies carried out beyond the UK were made.

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

Before and after, retrospective, controlled, uncontrolled, randomised (including cluster randomised), and non-randomised studies of the health and social effects of housing improvements were included in the review. Cross-sectional studies that did not investigate the effects of housing improvements were not included, that is cross-sectional surveys reporting associations between housing conditions or those in receipt of housing improvements and health, unless the outcome assessed was change in health. Intervention studies reporting quantitative or qualitative data, or both, were reported in the review. The study designs and the names used to describe study designs are defined in Appendix 1.

Types of participants

The review did 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 were eligible for inclusion. Outcomes for both adults and children were eligible for inclusion in the review. Included participants must have been in receipt of a discrete programme of rehousing or housing improvement. Where households experienced a change of housing conditions as an indirect result of some other life event, for example employment relocation or a natural disaster, and the housing improvement was not part of a discrete programme, these participants and the studies were not included.

Types of interventions

All physical house types which are static (that is not caravans or house boats) were eligible for inclusion. Mobile homes and house boats were not included. These housing types included exposure to a range of different housing conditions as well as being more likely to serve different purposes, which may also be related to exposure, for example recreational purposes. These house types were not considered a good comparison to permanent house types. Static permanent housing included residential establishments providing permanent accommodation and sheltered housing, or housing specifically for vulnerable adults where a manager or warden was available to facilitate independent living. Housing interventions were 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 were improved. Physical improvements tailored to meet the needs of the resident were eligible for inclusion, for example medical priority housing. Where residents were rehoused or received housing adaptations to accommodate changing mobility or care needs, or to alleviate mental health issues these studies were excluded unless it was clear that the majority of the recipients of the adaptations or rehousing experienced an improvement in housing condition and not simply an improvement in physical design or location. If these improvements were limited to provision of indoor furniture or equipment, such as vacuuming, mattresses, air purifiers and cookstoves, smoke alarms and other fire or injury prevention measures, they were excluded. Studies that did not provide specific information on the nature or extent of the physical housing improvement or focus on non-physical aspects of being rehoused were excluded. For example, a study may have reported the health effects of former residents of supported living quarters being relocated to live independently. Some studies mentioned that the physical quality of the new housing was superior to previous accommodation but details of the actual physical improvements were omitted as the intervention of interest to such a study was primarily the move to independent living. Such a study was excluded.

Studies were included if they investigated changes in health, illness, or well-being related outcomes among the residents following the delivery of a discrete housing improvement programme which was delivered following and as a consequence of a natural disaster or labor migration. It was possible that following, and as a consequence of, such an event some of the population lived in improved housing. However, studies were excluded where the study investigated 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 had been delivered to the population. The term 'discrete programmes' was used to describe a stand-alone project to deliver a defined housing improvement to a defined area or population by an agency. Retrospective analyses of changes in health following ad hoc home improvements initiated by the householder were not included.

Environmental studies of the adverse effects of lead, urea, formaldehyde, foam, air quality, allergens, or radon were not included. These studies assessed 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). Interventions to reduce or prevent exposure to lead, radon, urea, formaldehyde, allergens, or air pollutants were excluded. Lead, radon, urea, formaldehyde, asbestos, and other air pollutants are all now well established as stand-alone hazards to health and measures are available to limit exposure to these hazards. The focus of this review was to address the question around the extent to which general programmes of rehousing and housing improvement can lead to health improvement. Reviews of the health impacts following removal of these domestic hazards would be useful but were beyond the scope of this review. Similarly, there is a large body of evidence on the effectiveness on measures to reduce domestic fires and accidents as well as adaptations to promote mobility among the elderly. These topics merit a stand-alone review and indeed some reviews on these topics are available.

Housing improvements were included where they were delivered as part of a discrete programme of housing improvements. This meant that the nature of the housing improvement being delivered and eligibility for the improvement was pre-defined by the programme. Housing improvements initiated by householders may include improvements similar to those covered by this review. These interventions are susceptible to considerable levels of variation as they are not part of a discrete housing improvement programme. With little knowledge of the reasons for the improvement, the exact nature of the housing improvement implemented, changes in housing conditions, as well as changes in health outcomes it is unlikely that any identified studies would provide useful data on the health impacts of housing improvements.

The included interventions needed to meet one of each of the three criteria (A, B, C) listed in the left hand column of Table A.

Table A. Criteria for including excluding housing improvement interventions

Included	Excluded	
A House types: Permanent residences for independent living Sheltered housing Residential care homes	Mobile homes Boats	
B Housing programmes: Discrete housing programme, i. e. where a pre-defined housing improvement is delivered to a pre-defined population, either at area level or assessed according to individual eligibility	Diverse ad hoc improvements initiated by householder(s). Ad hoc provision of emergency housing following a natural disaster or due to migration	
C Housing interventions: Rehousing where clearly involves improvement in indoor housing conditions due to improved structure or housing fabric. This may include emergency rehousing, e.g. following natural disaster Medical Priority Rehousing - where there is physical improvement in housing condition delivered beyond adaptations to meet mobility, care, or mental health needs Structural improvements Warmth and energy efficiency improvements	Adaptations or rehousing to meet mobility, care, or mental health needs Housing design or layout Fire and accident prevention measures Measures to reduce or prevent exposure to: lead, radon, urea, formaldehyde, allergens, air pollutants, or asbestos Minor repairs, e.g. leaking pipes, broken windows	

Included interventions were allocated to the following groups.

- Warmth and energy efficiency improvements (post-1985).
- Rehousing or retrofitting ± neighbourhood renewal (post-1995).
- Provision of basic housing in low or middle income country (post-1990).
- Rehousing from slums (pre-1970).

Types of outcome measures

Primary outcomes

Outcome measures included any measure which could be interpreted as a direct measure of health or mental and physical illness, general measures of self-reported well-being, and quality of life measures.

Health service use was not included as a health outcome as this is not a direct measure of health or well-being. Studies only reporting changes in health service use were excluded from the review. Health service use cannot be considered a direct measure of health status as it is impossible to know whether an increase or decrease in health service use indicates an improvement or deterioration in

health. However, where included studies reported health outcomes and health service use, health service use data were extracted and reported (see Appendix 2) but were not included in the final synthesis of health impacts. Details of all the excluded studies and the interventions studied were extracted to provide a comprehensive list of studies which may have been considered eligible, for example by assessing a heath related outcome such as health service use (see list of excluded studies, Table 1).

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

Secondary outcomes

Additional social and socio-economic outcomes which could be interpreted as determinants of health were 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 were searched with no restriction on language. They were considered to be relevant to the issue of health equity.

- Cochrane Central Register of Controlled Trials (*The Cochrane Library* current Issue) (www3.interscience.wiley.com/cgi-bin/mrwhome/106568753/HOME).
- Cochrane Public Health Group Specialised Register (March 2012).
- Campbell Collaboration Social, Psychological, Educational and Criminological Trials Register (C2-SPECTR) (1950 to July 2012) (http://geb9101.gse.upenn.edu/RIS/RISWEB.ISA).
- MEDLINE (1966 to July 2012) (Ovid).
- CINAHL (1982 to July 2012) (Ovid).
- EMBASE (1980 to July 2012) (Ovid).
- PsycINFO (1872 to July 2012) (Ovid).
- MEDLINE In-Process and Other Non-Indexed Citations (Ovid) (September 2010).
- Social Science Citations Index (1981 to July 2012) (ISI Web of Knowledge).
- ASSIA (1987 to July 2012) (CSA).
- Sociological Abstracts (1963 to September 2010) (CSA).
- International Bibliography of the Social Sciences (1951 to September 2010) (BIDS).
- CAB Abstracts (1973 to July 2012) (Ovid).
- PAIS International (Public Affairs Information Service) (1976 to August 2010) (Dialog).
- ICONDA International Construction(1976 to September 2010) (Dialog).
- Architecture (1987 to June 2007) (Dialog).
- DH-DATA: Health Administration, Medical Toxicology and Environmental Health (1983 to February 2007) (Datastar).
- Global Health (1973 to September 2010) (Ovid).
- Science Citations Index expanded (1981 to August 2010) (ISI Web of Knowledge).
- SIGLE (GB records only) British Library in-house interface (with thanks to British Library staff) (to March 2005).
- COPAC (to July 2012).
- Avery Index to Architectural periodicals (1934 to August 2010).
- RIBA (Royal Institute of British Architects) library catalogue (July 2012).
- Social Care Institute for Excellence (SCIE) (July 2012).
- NTIS (National Technical Information Service) (July 2012).
- Geobase (to September 2010).
- Sociological Abstracts (to September 2010).
- Web of Science (to July 2012).
- PLANEX (1980 to July 2012).
- Libris (The union catalogue of Swedis libraries) (August 2006).
- SveMed+ (nordiska artiklar inom det medicinska området) (August 2006).
- Libris uppsök (examensarbeten och uppsatser i fulltext) (August 2006).
- DIVA (Digitala vetenskapliga arkivet) Artikelsök (Artiklar från svenska tidsskrifter) (August 2006).
- NORART (Norwegian and Nordic index to periodical articles) (August 2006).
- DEFF, Danmarks Elektroniske Fag - og Forskningsbibliotek (Denmark's Electronic Research Library) (August 2006).
- AKF, Amternes og kommunernes forskningsinstitut (Institute of Local Government Studies) (August 2006).
- DSI Institut for Sundhedsvæsen (Danish Institute for Health Services Research) (August 2006).
- SBI, Statens Byggeforskningsinstitut (Danish Building Research Institute) (August 2006).
- Statens Institut for Folkesundhed (National Institute of Public Health) (August 2006).
- Social.dk, Socialministeriet (Ministry of Social Affairs) (August 2006).
- Google Scholar (August 2006).

An example of the search strategy illustrating the search terms used is available in Appendix 3. The strategy and combination of terms used was amended as required for each database. The search strategy was not limited with respect to population characteristics such as age, gender, language, or race. The search strategy included

terms relating to public provision of housing aimed at low income populations.

Details of the full searches are provided in Appendix 4. The initial search was conducted in 2005 following approval of the protocol by The Campbell Collaboration in November 2004. The search was updated in 2007 and again in 2010 following submission of the completed review to The Campbell Collaboration and a decision to prepare the review as a joint review with The Campbell Collaboration and The Cochrane Collaboration, and a further updating of the search was conducted in July 2012. Due to changes in the databases and search facilities the updating of the searches could not be replicated but the 2007, 2010, and 2012 searches were devised to be more sensitive than the original search to ensure that studies were not lost by the changes in database search facilities. In addition to the searching of international databases, The Campbell Collaboration provided an information scientist to search Scandinavian databases with grey literature coverage for this region.

Searching other resources

Bibliographies of screened papers and identified reviews were searched for eligible studies. Efforts to identify relevant grey literature included contacting experts, searching SIGLE and COPAC, handsearching IDOX (formerly PLANEX), and searching relevant websites both within the UK and beyond. Details of the websites searched are provided in Appendix 4 (Section e). A list of experts from the lead review author's own contacts and authors of housing studies was drawn up, and these contacts were e-mailed to request any information about completed or ongoing studies which might be relevant to the review (see Appendix 4, Section d).

Data collection and analysis

Selection of studies

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

Data extraction and management

Citations were stored in EndNote© (bibliographic software). Assessment of risk of bias was conducted by two review authors independently and disagreements resolved by discussion. The reported findings from each study were extracted by one review author and checked by a second review author, with disagreements or inaccuracies discussed between the authors. All data were entered into an Access database. The final agreed data extraction was entered into RevMan by one review author. A list of data extraction fields is available in Appendix 5.

The data extraction included extraction of details 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 completed the Cochrane risk of bias tool for each included quantitative study. In addition to the standard risk of bias items we included three items recommended by the Cochrane Effective Practice and Organisation of Care (EPOC) Group. These were: similarity of outcomes across the intervention and control group at baseline, similarity of key characteristics at baseline, and contamination within the control group. The wording of the risk of bias tool was amended slightly to incorporate assessment of non-randomised studies. In addition, we attempted to incorporate items from the Hamilton tool (an additional tool designed to assess study quality in non-randomised studies, see next paragraph for more information) into the risk of bias tool. The additional items were: blinding of analysts, baseline response, and intervention implementation within the intervention group. Full details of the risk of bias tool and the additional items are reported in Appendix 6.

It was considered that the Cochrane risk of bias tool was not sensitive to the variations in study quality across the study designs included in this review, such as non-randomised studies and uncontrolled studies. For this reason, the quantitative studies were also assessed for risk of bias using a critical appraisal tool developed by a group of systematic reviewers in Hamilton, Canada (Hamilton Assessment Tool) (Thomas 1998). This tool 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 amended the Hamilton Assessment Tool to ensure that it was 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 controlled before and after study designs and other non-randomised study designs; we amended the tool to allow distinctions between controlled and uncontrolled study designs. Our amended HAT to assess risk of bias is presented in Appendix 7. Using this tool, each study was 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 was 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 7.

The quality assessment for each study was 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) were resolved through discussion between the two review authors.

Each study was assigned to an overall summary category (A, B, or C) indicating the overall potential for bias, this was based on the Hamilton tool. The criteria for this summary category are outlined in Appendix 7.

Table B below lists and compares the two quality assessment tools used with respect to the elements of bias assessed. The upper half of the table reports the elements of bias used to assess overall study quality based on the Hamilton tool.

Table B. Comparison of risk of bias (RoB) tool and Hamilton tool assessing aspects of bias in quantitative studies (bracketed text indicates source of item: Cochrane RoB - essential Cochrane risk of bias items; EPOC - additional Cochrane risk of bias items recommended by the EPOC group; Hamilton - Hamilton tool amended by the review authors)

Type of bias assessed	Cochrane risk of bias (RoB)	Hamilton Assessment Tool	Comment
Bias items used in assessment of overall study quality for the review			
Selection	Sequence generation (Cochrane RoB)		Not applicable to NRS
Selection	Allocation concealment (Cochrane)	Study design (Hamilton)	
Confounding	Baseline outcome characteristics similar (EPOC)	Control for confounding through analysis or design (Hamilton)	
Confounding	Baseline characteristics similar (EPOC)		
Baseline response	Baseline response (Hamilton)	Selection (Hamilton)	
Attrition	Incomplete outcome data (Cochrane RoB)	Withdrawals at follow-up (Hamilton)	
Bias items not used in assessment of overall study quality for the review			
Contamination	Contamination (EPOC)		
Reporting	Selective reporting (Cochrane RoB)		
Performance	Blinding - participants (Cochrane RoB)	Blinding - participants and assessors (combined) (Hamilton)	Rarely applicable to housing improvement studies - no studies blinded participants
Detection	Blinding - assessors (Cochrane RoB)		
	Blinding - analysts (Hamilton)		

(*Continued*)

Outcome measure		Data collection (Hamilton)	Designed to indicate appropriate data collection tools and outcomes
Performance	Intervention implementation: within study variation of exposure to intervention (Review authors)	Heterogeneity of exposure to intervention and potential to benefit from intervention (Review authors)	This measure was developed by the authors

Qualitative studies

Qualitative studies, including studies reporting qualitative data supplementary to quantitative data, were included in the review. There is much unresolved debate about appropriateness of quality assessments 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, were 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 was used. The qualitative data identified following the searches varied in terms of depth of enquiry and methods of data collection and analysis. Rather than perform a detailed synthesis of the qualitative data our plans for the qualitative data were to map out what was available with the possibility of illuminating additional unintended impacts associated with housing improvement. Following examination of some appraisal tools for qualitative research it was agreed that a brief tool to enable a systematic and independent assessment by two reviewers of study quality which allowed for diverse methods and study approaches was required. We adapted a series of prompts (Appendix 8) used in a previous review of tobacco control (Thomas 2008). The tool was developed by a team in the ESRC Research Methods Programme following extensive discussion within a multi-disciplinary team and evaluation of two existing appraisal tools (Dixon-Woods 2004).

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 sample. For example, the extent of housing improvement may be tailored according to individual household need and so the level of exposure to the intervention will vary across the study sample. 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 were assessed for within-study heterogeneity with respect to intervention implementation as well as for heterogeneity in the extent of improvement in housing conditions actually experienced by participants (see Appendix 7).

Measures of treatment effect

Comprehensive Meta-Analysis (CMA) software© was used to calculate standardized effect sizes for all health outcomes from controlled studies which reported necessary data. These outcomes included continuous and dichotomous variables and the standardized effect was reported as an odds ratio (OR) and 95% confidence interval (CI).

Unit of analysis issues

Housing interventions were allocated and implemented at a household level, either to individually targeted households meeting prespecified eligibility criteria or to all households within a targeted geographical area. Health outcomes were assessed at an individual level.

In some studies health outcomes were only assessed for one occupant, and in others health outcomes were assessed for more than one or for all occupants (these assessments were sometimes made on behalf of other occupants by a nominated occupant). The sample type varied across the identified studies. We extracted all re-

ported health and socio-economic outcomes for all occupants included in the study. Where a study presented different data for different occupant types, the categories included: adult or child; adult; gender; diagnosed with or without specific illness, for example asthma. Data for other subgroups of interest with respect to equity indicators were also extracted, including data on race or ethnicity, occupation, socio-economic status, education, religion. For the main analysis child and adult data were reported and analysed separately. Data and analysis on other subgroups mentioned above, in particular those with equity implications, were extracted, reported, and synthesised where there were sufficient similar data.

Dealing with missing data

We contacted authors of studies to obtain missing data. We reported withdrawals and levels of attrition for each study and incorporated these into the overall indication of study quality. CMA was used to calculate standardized effect sizes for controlled studies which reported the necessary data.

Assessment of heterogeneity

Statistical heterogeneity was assessed using the Chi^2 and I^2 statistics. If appropriate, a meta-analysis of effect sizes was conducted using a fixed-effect model, otherwise a random-effects model was considered. Heterogeneity within and between the studies was investigated and reported with respect to study design, study quality, intervention, context, and implementation of the intervention. A decision to use a random-effects model for meta-analysis took into consideration the level of statistical heterogeneity as well as heterogeneity of study characteristics. Where there were close similarities across the studies with respect to study sample, specific outcome type, time of follow-up, context, and the intervention, and where there was limited statistical heterogeneity, a fixed-effect model was used. Where there was variation in one of more of these characteristics a random-effects model was used regardless of statistical heterogeneity.

See the section on 'subgroup analysis and investigation of heterogeneity' (below) for a more detailed description of how heterogeneity between the studies was dealt with, and also 'intervention implementation and performance bias' (above) for details of how heterogeneity with respect to implementation and performance bias was assessed.

Assessment of reporting biases

We planned to investigate the impact of publication bias by preparing a funnel plot and calculating Egger's test if there were sufficient studies which reported standard errors for the effect sizes. However, there was an insufficient number of studies reporting the required data.

Data synthesis

Quantitative data

Data from the better quality studies were synthesized and the final synthesis reflected the relative weight of evidence within each group of studies. Results of experimental and quasi-experimental studies were analysed separately. Data from the poorer quality studies were also synthesized separately but these data were not incorporated into the conclusions of the synthesis.

As anticipated there were 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 standardized effect sizes, grouping of studies appropriately with respect to interventions and outcomes, and use of a random-effects model (Ioannidis 2008). Where data were available, standardized effect sizes for all controlled studies identified were calculated using Comprehensive Meta-Analysis software© (CMA). Where the outcomes within a category were similar but not the same, for example different measures of respiratory health, and they included a mix of continuous and dichotomous variables, we presented effect sizes as odds ratios. These were presented in a forest plot to allow all the effect sizes to be shown together even where meta-analysis was not performed. The outcomes presented were predominantly dichotomous and use of CMA software facilitated transformation of standardized mean differences from continuous variables into odds ratios to allow the presentation of all standardized effect sizes for each outcome category on a single forest plot. Where data for similar outcomes following similar housing improvements (outcomes and interventions grouped as outlined in 'subgroup analysis and investigation of heterogeneity') were available these effect sizes were pooled. Heterogeneity was assessed using Chi^2 and I^2 statistics. Only two studies reported data suitable for meta-analysis. Due to high levels of heterogeneity a random-effects model was used.

For groups of studies where a statistical synthesis of the data was not appropriate the data were synthesised narratively using the Economic and Social Research Council (ESRC) guidance (Popay 2006). The main steps of the narrative synthesis involved 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 were tabulated to provide a textual as well as a visual summary of the data using an effect direction plot. This allowed presentation of all studies whether or not standardized effect sizes were available. The visual tabulation of reported effect direction facilitated 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 were identified and how the poorer quality studies which were excluded from the synthesis compared to the better quality studies, a narrative description of all studies, regardless of study design, was included in the final review. Data from all eligible studies, regardless of study quality, were tabulated.

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 overlooks 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 was expected that the qualitative studies would be heterogeneous with respect to intervention, context, and population as well as methodology and study quality. For these reasons we conducted a narrative synthesis of the qualitative data using the ESRC guidance on narrative synthesis (Popay 2006). The findings from each study were tabulated to provide a textual summary of the data. This facilitated a thematic analysis and the examination of emerging themes with respect to reported impacts, mediating factors, and pathways affecting health impacts.

The qualitative studies were grouped according to the intervention categories developed for the quantitative studies, reflecting intervention type, context, and time period. Following agreement between the two review authors regarding the quality assessment and data extraction for the qualitative studies, two review authors (ST and HT) independently prepared a logic model mapping the impacts and links between impacts reported in the qualitative data. The two logic models were then compared. Following discussion to resolve differences between the two logic models, a final logic model was prepared to represent the nature of the impacts and links between impacts emerging from the qualitative data.

Incorporation of qualitative and quantitative data

Following preparation of a logic model mapping the findings of the qualitative data, the nature and direction of impacts reported in the better quality (Overall Grade A and B) quantitative studies were also mapped onto a logic model. This was added to the logic model of qualitative data to produce a one page summary of the reported health impacts and mechanisms for health impacts reported in the better quality studies. A logic model was prepared for 'warmth and energy efficiency improvements' (post-1985) and 'rehousing or retrofitting ± neighbourhood renewal' (post-1995) where there was a body of better quality studies. Finally, a logic model combining data from both groups was prepared to provide an empirically based model of the nature of, and mechanisms for, reported health impacts following housing improvement.

Subgroup analysis and investigation of heterogeneity

We assessed the studies and data according to different aspects of heterogeneity, including statistical heterogeneity. With respect to heterogeneity of interventions, the synthesis was carried out for groups of studies which included similar interventions, as described below.

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

The broad scope of this review inevitably meant that there was extreme variance in the methods used, the interventions being assessed, the study populations, and contexts in which the intervention was implemented; and the potential range of illness, health, and well-being 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 were extracted where available. Interventions were grouped into broad categories of the type of housing intervention and according to the context and population of the study. The groups were as follows.

- Warmth and energy efficiency improvements (post-1985).
- Rehousing or retrofitting ± neighbourhood renewal (post-1995).
- Provision of basic housing in low or middle income country (post-1990).
- Rehousing from slums (pre-1970).

The reported outcomes were grouped into broad categories: general health, respiratory health, mental health, and other illnesses or symptoms. The reported data were 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 were substantial levels of statistical heterogeneity (> 50%) the data were checked for accuracy. Where statistical heterogeneity persisted the data were meta-analysed using a random-effects model. Where substantial heterogeneity persisted the standardized effect data were presented on a forest plot but a meta-analysis was not performed. The lack of studies reporting standardized effect size prevented meta-analysis for all but two studies.

Investigation of equity and differential impacts across population subgroups

The studies in this review focused 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 were extracted and reported separately, for example where impacts were reported by gender, socio-economic status, educational status, or religion. Where sufficient similar data on specific subgroups were available we considered synthesizing and presenting these data separately to illustrate the differential effects for different subgroups. The lack of data on subgroups prevented separate meta-analysis by subgroup and the subgroup data were reported narratively as part of the narrative synthesis.

Sensitivity analysis

Before making decisions about which studies to include in the final syntheses, a sensitivity analysis was considered to examine variation in reported effects by study characteristics. The ability to perform a formal sensitivity analysis was limited due to the small number of studies and outcomes amenable to calculation of a standardized effect size and a meta-analysis. A less formal sensitivity analysis was also limited due to the small number of studies in any single intervention category reporting similar outcomes which were also similar with respect to specific study characteristics, such as study design and other markers of internal validity, as well as study population. An investigation of variation in reported impacts for each outcome category and relationship to study characteristics was carried out by examining the full data for groups of studies in the Access database and the visual summaries of reported effect directions. Variations in reported effect directions and statistically significant results were examined according to study design, study quality, and sample size, as well as by intervention and context.

RESULTS

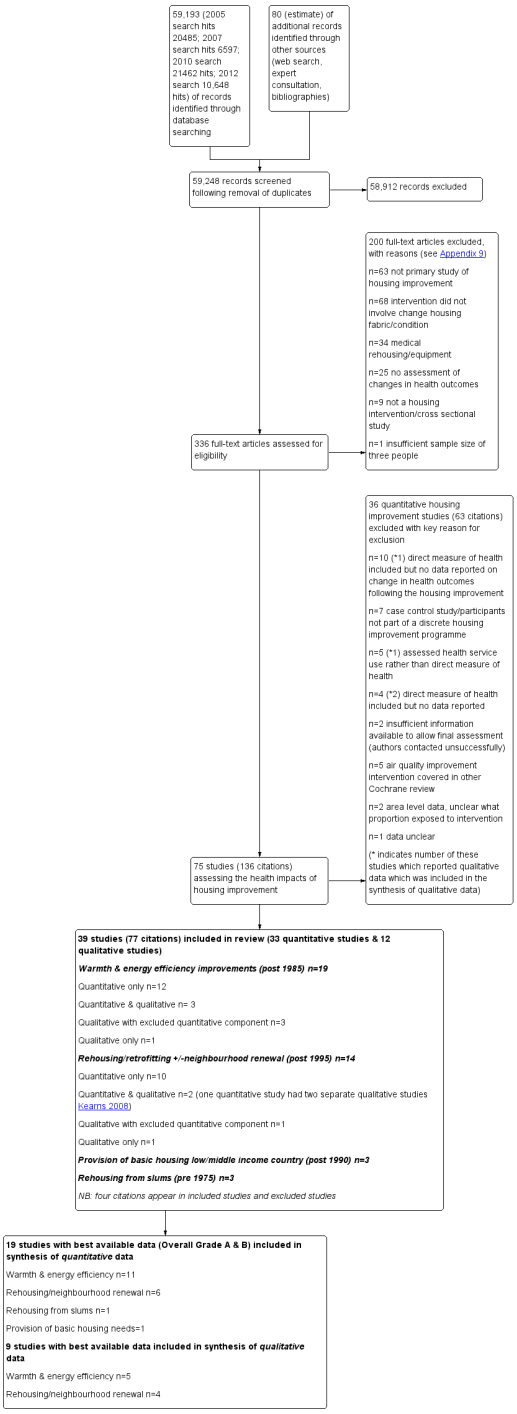
Description of studies

See: Characteristics of included studies; Characteristics of excluded studies; Characteristics of studies awaiting classification; Characteristics of ongoing studies.

Results of the search

Following the searches 59,193 citations were identified. Details of the number of hits identified by each database are available in Appendix 4. The sensitive nature of the searches resulted in a large number of potentially relevant citations being identified. Initially a selection of obviously irrelevant citations were examined to determine whether the search could be made more specific. However, on examination it was clear that key words which were an important component of the search were appearing in these obviously irrelevant citations, supporting the need for the sensitive search. A flow chart reports the numbers of citations excluded on the basis of title and abstract, and those citations which were screened using the full text (Figure 1).

Figure 1. Study flow diagram.



Included studies

Seventy-seven citations (Figure 1) were identified as meeting the inclusion criteria. These represented 39 separate studies; 28 studies reported only quantitative health impact data (Allen 2005; Ambrose 2000; Aziz 1990; Barnes 2003; Blackman 2001; Braubach 2008; Breysse 2011; Chapin 1938; CHARISMA 2011; Critchley 2004; Evans 2000; Halpern 1995; Health Action Kirklees; Hopton 1996; Howden-Chapman 2007; Howden-Chapman 2008; Iversen 1986; Lloyd 2008; McGonigle 1936; Molnar 2010; Osman 2010; Platt 2007; Rojas de Arias 1999; Somerville 2000; Spiegel 2003; Thomson 2007; Wells 2000; Wilner 1960), five studies reported both quantitative and qualitative data on health impacts (Allen 2005a; Barton 2007; Kearns 2008; Shortt 2007; Thomas 2005), two studies reported only qualitative data (Decent Homes 2012; Ellaway 2000). One study reported conducting a qualitative study in addition to the quantitative survey, but the data from the qualitative investigation comprised additional detailed quantitative data largely around changes in housing costs. The data were extracted and reported alongside the quantitative data (Ambrose 2000). A further four studies reported qualitative data on health impacts which was supplementary to a quantitative assessment of changes in health, but no quantitative data on changes in direct health outcomes were reported so the quantitative components of the studies were excluded from the review (Caldwell 2001; Heyman 2011; Jackson 2011; Warm Front 2008). Those studies which reported quantitative data on health impacts are listed under the 'Included studies' section with those papers marked as including qualitative data. In addition, a full list of the studies and references to the included qualitative studies is provided in Table 2 and Table 3.

Included studies by intervention category

The included studies were grouped into one of four categories according to the intervention type and, where appropriate, according to distinct contexts with respect to time or a historical period, low or middle income countries, and high income countries. The four categories were as follows.

- Warmth and energy efficiency improvements (post-1985) (n = 19). Quantitative studies: Allen 2005; Allen 2005a; Barton 2007; Braubach 2008; CHARISMA 2011; Health Action Kirklees; Hopton 1996; Howden-Chapman 2007; Howden-Chapman 2008; Iversen 1986; Lloyd 2008; Osman 2010; Platt 2007; Shortt 2007; Somerville 2000. Additional qualitative studies with no included quantitative data: Caldwell 2001; Decent Homes 2012; Heyman 2011; Warm Front 2008. In one of the included warmth improvement studies only a small subgroup of the intervention group received a warmth improvement but the data were still included (CHARISMA 2011).
- Rehousing or retrofitting ± neighbourhood renewal (post-1995) (n = 14). Quantitative studies: Ambrose 2000; Barnes 2003; Blackman 2001; Breysse 2011; Critchley 2004; Evans 2000; Halpern 1995; Kearns 2008; Molnar 2010; Thomas 2005; Thomson 2007; Wells 2000. Additional qualitative studies with no included quantitative data: Ellaway 2000; Jackson 2011.
- Provision of basic housing in low or middle income country (post-1990) (n = 3). Quantitative studies: Aziz 1990; Rojas de Arias 1999; Spiegel 2003.
- Rehousing from slums (pre-1970) (n = 3). Quantitative studies: Chapin 1938; McGonigle 1936; Wilner 1960.

The largest group of studies was 'warmth and energy efficiency improvements' (post-1985), and the smallest groups of studies were 'Provision of basic housing in low or middle income country' (post-1990) and 'rehousing from slums' (pre-1970). No qualitative data were identified from studies conducted in low or middle income countries or studies of rehousing from slums. The details of each intervention are provided in Table 4; Table 5; Table 6; Table 7.

Study designs

The 33 included quantitative studies used various study designs. Five were randomised controlled trials (RCTs), including one study which used a stepped-wedge design (Barton 2007; CHARISMA 2011; Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010). All the RCTs were studies of 'warmth and energy efficiency improvements'. Fourteen studies were quasi-experimental studies or controlled before and after (CBA) studies which assessed health outcomes in a cohort of people before and after the intervention and included a comparison or control group (Barnes 2003; Braubach 2008; Critchley 2004; Evans 2000; Hopton 1996; Iversen 1986; Kearns 2008; Lloyd 2008; Platt 2007; Rojas de Arias 1999; Shortt 2007; Thomas 2005; Thomson 2007; Wilner 1960). Three studies were cross-sectional controlled before and after (XCBA) studies (Aziz 1990; McGonigle 1936; Spiegel 2003). These studies assessed health outcomes in a neighbourhood undergoing housing investment before and after the intervention and included a comparison group. However, it was not specified that the sample population were followed as a cohort throughout the study, rather the assessment of health outcomes before and after relied on cross-sectional surveys, and there was no indication that the target population had changed over the course of the study. The remaining 10 studies had no control group; eight of these were uncontrolled before and after studies (UBA) (Allen 2005; Allen 2005a; Ambrose 2000; Blackman 2001; Chapin 1938; Molnar 2010; Somerville 2000; Wells 2000); two studies had a retrospective uncontrolled design (Breysse 2011; Health Action Kirklees) which assessed changes in health outcomes retrospectively; Halpern 1995 did not report data for a control group or a cohort of participants before and after

the intervention, rather this study only reported cross-sectional data for the intervention area so this study was labelled as a cross-sectional uncontrolled before abd after study (XUBA).

Excluded studies

Due to the sensitive nature of the searches the majority of identified citations could be excluded on the basis of title and abstract (n = 58,912). The full texts of 336 citations were examined. From these, 200 citations (Figure 1) clearly did not meet the review's inclusion criteria. A list of these excluded studies with the key reason for exclusion is provided in Appendix 9.

One hundred and thirty-five citations appeared to meet the review inclusion criteria, as they were evaluations of the health impacts of housing improvement. However, on further examination 63 citations were excluded representing 36 Excluded studies (Table 1). Nine of the excluded studies assessed the health impacts of warmth and energy efficiency improvements (Caldwell 2001; Eick 2011; El Ansari 2008; Green 1999; Heyman 2011; Roder 2008; Telfar-Barnard 2011; Warm Front 2008; Winder 2003); three assessed the health impacts of rehousing or retrofitting (Jackson 2011; Walker 1999; Woodin 1996); nine were of interventions to provide basic housing facilities often in low or middle income countries (Sedky 2001; Aiga 2002; Bailie 2012; Cattaneo 2007; Choudhary 2002; Pholeros 1993; Vyas 1998; Westaway 2007; Wolff 2001); two were of rehousing from slum conditions (Ferguson 1954; Wambem 1973); five were of improved air quality (Allen 2011; Burr 2007; Kovesi 2009; Warner 2000; Wright 2009); one study was of medical priority rehousing (Smith 1997); and seven were case control studies where there was no discrete programme of housing improvement and the reported improvements were diverse (Butala 2010; Coggon 1991; Jones 1999; Kahlmeier 2001; Keatinge 1989; Marsh 1999; Meddings 2004).

The most common reason for exclusion was the lack of data on changes in direct health outcomes. This was either due to study design (n = 10) (Sedky 2001; Aiga 2002; Cattaneo 2007; Choudhary 2002; Ferguson 1954; Green 1999; Smith 1997; Telfar-Barnard 2011; Warm Front 2008; Wolff 2001), or where a study assessed changes in a direct health outcome but did not report any data to support reported findings (n = 4) (Caldwell 2001; Heyman 2011; Roder 2008; Winder 2003), or where changes in health service use were assessed but there was no assessment of changes in a direct health outcome (n = 5) (Jackson 2011; Pholeros 1993; Walker 1999; Wambem 1973; Woodin 1996). Seven case control studies were excluded (Butala 2010; Coggon 1991; Jones 1999; Kahlmeier 2001; Keatinge 1989; Marsh 1999; Meddings 2004), these studies reported health outcomes retrospectively among a sample of people who had received housing improvement but where the improvements had not been part of a discrete programme of housing improvement. In two studies, only a small proportion of the study sample received the intervention and the analysis did not distinguish the intervention group from those who had not received the intervention (Bailie 2012; El Ansari 2008). Two studies (Vyas 1998; Westaway 2007) provided insufficient information to confirm whether the study met the review inclusion criteria. We attempted to contact the authors of these studies but without success. One small and poorly conducted RCT of warmth improvements was excluded due to poorly reported data which were difficult to interpret (Eick 2011); some data were available following the installation of mechanical ventilation heat recovery but this intervention was excluded from the review (see below). A further five studies which assessed changes in direct health outcomes following installation of ventilation improvements were excluded. Two studies that assessed the impact of air filters (Allen 2011; Burr 2007) and three studies that assessed the health impacts among asthmatic people following installation of a mechanical ventilation heat recovery (MVHR) housing improvement intervention were excluded (Kovesi 2009; Warner 2000; Wright 2009). These studies did assess changes in direct health outcomes but while MVHR may result in small improvements in domestic warmth, MVHR is primarily aimed at improving air quality. Two earlier Cochrane reviews (Gøtzsche 2008; Singh 2002) have focused on the health impacts of allergen reduction and air quality improvement among atopic and asthmatic groups and for these reasons this intervention was excluded from this review. One study assessing the impacts of mould removal and installation of a fan in the home was included as one of the intervention groups also received central heating (CHARISMA 2011).

Four of the excluded quantitative studies included a qualitative component which was assessed and included in the review (Caldwell 2001; Heyman 2011 (Harrington et al 2005); Jackson 2011 (Bullen et al 2008; Clinton et al 2006); Warm Front 2008 (Gilbertson et al 2006)) (relevant reference for the qualitative study indicated beside quantitative study link where the data were reported separately); for qualitative data see Table 2; Table 3. The quantitative elements of these studies were excluded because of study design (Warm Front 2008) or because only health service use outcomes were reported (Jackson 2011) or no data were reported (Caldwell 2001; Heyman 2011).

Ongoing studies

Three ongoing studies were identified as potentially eligible for inclusion but no findings were yet publicly available. One study is an RCT of warmth subsidies in New Zealand (WHEZ). Two longitudinal studies of major housing-led neighbourhood regeneration in social housing areas are underway in the UK, one in Glasgow, Scotland (GoWell) and one in Carmarthenshire in Wales (Lyons 2011).

Risk of bias in included studies

Quantitative studies

A summary of 'risk of bias' (RoB) for each study and comparative data across the studies is reported in Figure 2 and Figure 3 (see also Characteristics of included studies). The Cochrane RoB items were assessed using the criteria in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). We made minor amendments to the criteria to incorporate elements appropriate to our included studies (Appendix 6). Additional RoB items were included to supplement the standard items (see Assessment of risk of bias in included studies) and a total of 12 RoB items were completed for each quantitative study. As assessed by RoB, study quality was poor across the included studies. None of the studies were rated as 'low risk of bias' across all RoB domains. Only one study had no items which were rated to be at a 'high risk of bias' (Howden-Chapman 2008). It is apparent that for many studies poor reporting meant that the RoB was 'unclear'. The range for the number of unclear RoB items was 2 to 10 out of a possible 12 RoB items. Five RCTs were identified, each of these was within the 'warmth and energy efficiency' intervention category. With the exception of the 'Allocation' RoB domain there was little variation in the RoB items with respect to intervention category. The frequency of 'High' RoB items was largely explained by the small number of RCTs and the inclusion of uncontrolled studies. The two items relating to randomisation (sequence generation, allocation concealment) were the most common items to be rated as 'High' RoB. In addition, the inclusion of uncontrolled studies (n = 10) meant that these were all rated as 'High' RoB for the three blinding items, the two EPOC items comparing baseline characteristics, and contamination. Only one of the uncontrolled studies was included in the final synthesis (Somerville 2000). The items most likely to be rated as 'Low' RoB (in more than six studies) were 'Baseline outcome characteristics similar', 'Baseline characteristics similar', 'Baseline response', and 'Implementation of intervention'.

Figure 2. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.

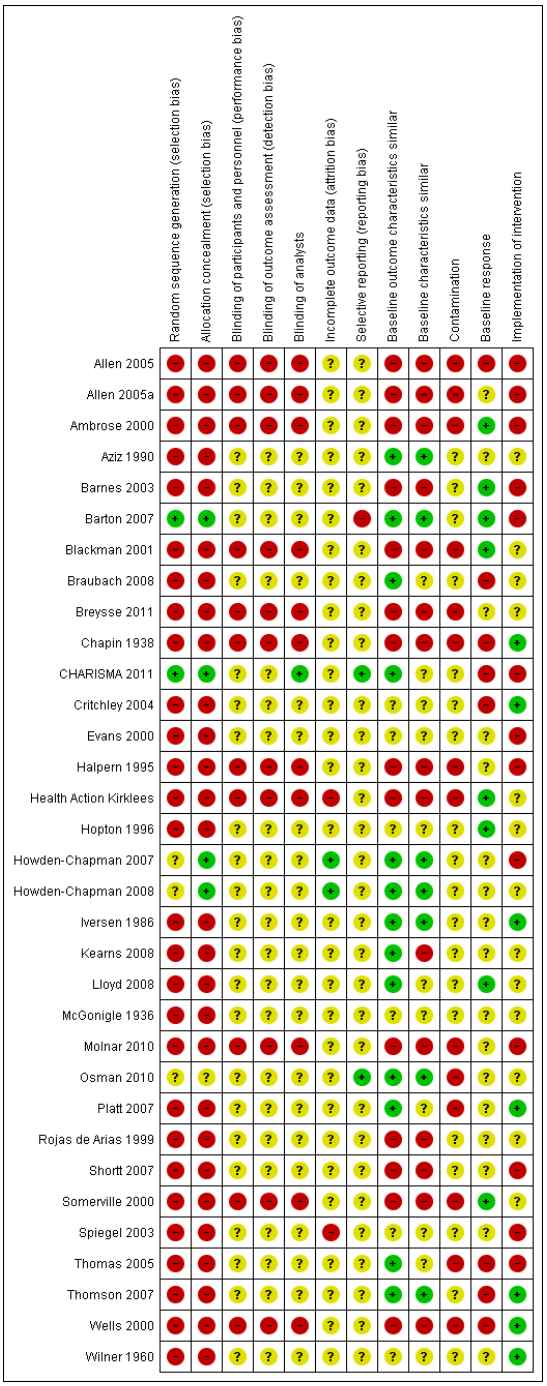
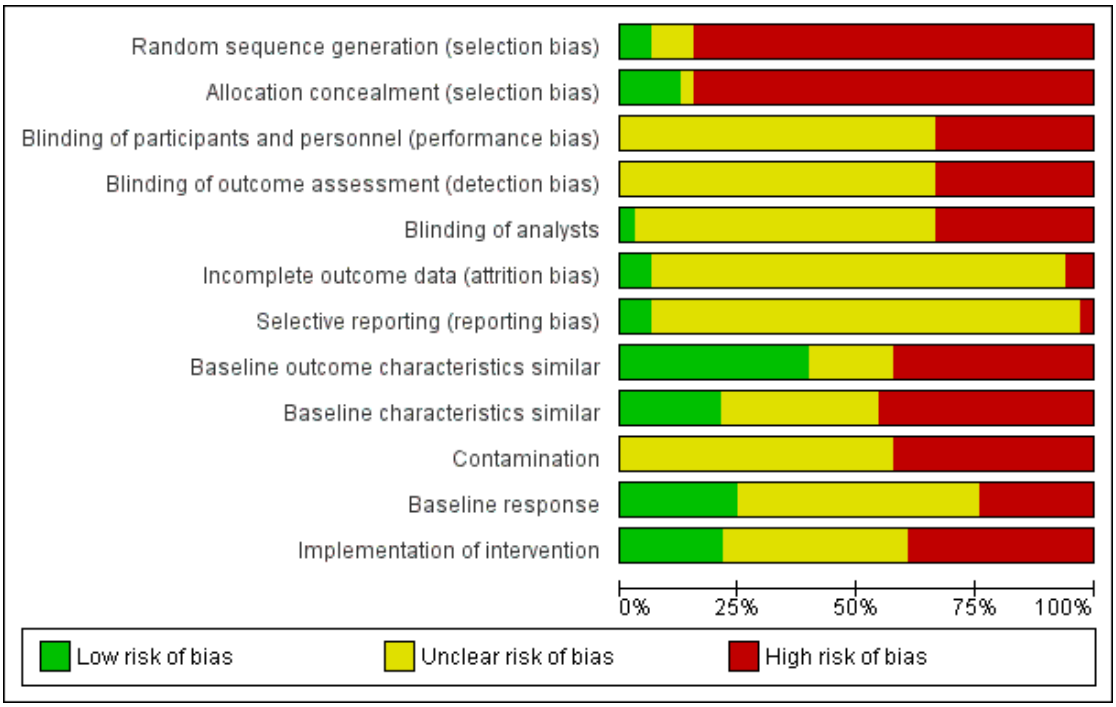


Figure 3. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies.



Additional assessment of study quality (quantitative studies)

As stated in the protocol an additional assessment of study quality was conducted to allow for some of the variations in study quality in non-randomised studies. This was developed from the Hamilton Assessment Tool (HAT) (Thomas 1998), see Appendix 7. Some clarifications were made to this tool following approval of the protocol and these are marked with an asterisk in Appendix 7. The HAT included assessment of study quality across six domains: selection at baseline, study design, control for confounding by study design or analysis, blinding of participants and assessors, data collection methods, and withdrawals at final follow-up. In addition, an item on performance bias was added to this tool but this was not incorporated into the overall assessment of internal validity. The assessment of performance addressed issues of variation in exposure to the intervention, and the potential to benefit from the intervention, by assessing the variation in the extent of the intervention delivered and also variation in baseline housing conditions across the sample (see Methods: *Intervention implementation and performance bias*).

The HAT allows for an overall indicator of study quality using the three options A, B, and C to indicate minimal, moderate, and considerable potential for bias, respectively. Our overall assessment drew on four of the HAT domains: study design, selection, withdrawals, and confounding. Given the number of non-randomised studies included in this review this tool allowed for greater sensitivity to variation in study quality. A comparison of these two assessments is provided in Table 8.

One item, selection, in HAT assessed the representativeness of the study sample by examining the representativeness of the sample compared to the population being targeted by the intervention and the baseline response rate. This element of bias was not incorporated into the Cochrane RoB tool and an additional RoB item was created by the review authors to allow the RoB tool to reflect the key HAT items used to develop the assessment of overall study quality.

Details of the individual bias domains assessed are presented below. Assessments using both the RoB tool and the HAT were reported where appropriate.

Qualitative studies

A separate assessment of study quality and reporting was developed for the qualitative studies and is reported below.

Agreement between review authors

Levels of agreement between review authors was high. All initial disagreements around the quality assessment items were quickly resolved by discussion between the two review authors.

Allocation

Five studies used a randomised controlled design (Barton 2007; CHARISMA 2011; Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010). All the RCTs were of 'warmth and energy efficiency improvements'. The non-randomised studies were assessed as having a high risk of bias due to non-random allocation of the intervention.

Sequence generation (RoB): the generation of a random sequence for the intervention allocation was described clearly in only two studies. One study used contemporaneous dynamic randomisation (CHARISMA 2011). The second study drew names of residents on a waiting list for housing improvements out of a bucket at a public meeting (Barton 2007). In the other three RCTs the method of sequence generation was not clear. Non-randomised studies (n = 28) were considered to have a high RoB in this domain.

Allocation concealment (RoB): four of the RCTs reported methods that were judged to conceal allocation from the participants and investigators (Barton 2007; CHARISMA 2011; Howden-Chapman 2007; Howden-Chapman 2008). One RCT did not report any attempt to conceal allocation (Osman 2010). Non-randomised studies (n = 28) were considered to have a high RoB in this domain.

Study design (HAT): the HAT tool was designed to accommodate assessment of non-randomised studies and did not incorporate separate assessments of randomisation integrity. This item graded the study according to study design, whether the study included a control group, whether the outcomes were assessed retrospectively, and whether the study used a cohort or a repeat cross-sectional design. This was the HAT item with the greatest number of studies assessed to have a minimal potential for bias. Nineteen studies were assessed to have a minimal level of bias due to study design (Grade A). These included five RCTs and 14 controlled before and after (CBA) study designs which followed the same cohort before and after the intervention. Eleven studies were assessed to have a moderate amount of bias due to study design (Grade B). These studies included eight uncontrolled before and after studies (UBA) and three CBA studies which used area based cross-sectional data rather than tracing changes in a cohort of individuals (XCBA). To be assessed as having moderate bias, studies using cross-sectional data to assess changes in health outcomes were required to indicate that there was little change in the population living within the intervention neighbourhood between the assessment of baseline and follow-up outcomes. Three studies were assessed to have considerable potential for bias due to study design. Two studies (Breysse 2011; Health Action Kirklees) assessed changes in health retrospectively and did not use a control group (retrospective uncontrolled design). In the second study the design was unclear with respect to how the changes in health outcomes were reported (Halpern 1995). Study design did not appear to be related to intervention type but was used as a key criterion in the HAT final assessment of overall study quality.

Blinding

Blinding was only reported to be incorporated into one study which blinded analysts to intervention allocation (CHARISMA 2011). Otherwise there were no reported attempts to blind participants or outcome assessors. Uncontrolled studies were assessed to be at high RoB for this domain. The controlled studies were assessed to be unclear as it was not clear either to what extent the participants, assessors, or analysts were aware of intervention allocation or to what extent blinding would affect the outcomes.

Blinding of participants (Cochrane RoB): none of the RCTs or controlled studies (n = 22) reported blinding participants to whether or not they received the intervention, and they were judged to be unclear with respect to the potential risk of bias in this domain. Studies with no control group (n = 11) were considered to have a high RoB in this domain.

Blinding of assessors (Cochrane RoB): none of the RCTs or controlled studies (n = 22) reported blinding those who assessed the health outcomes regarding who had received the intervention; in many cases the outcome was assessed using a self-completion questionnaire completed by the study participant. All the RCTs and controlled studies were judged to be unclear with respect to the potential risk of bias in this domain. Studies with no control group (n = 11) were considered to have a high RoB in this domain.

Blinding of analysts (Cochrane RoB): one RCT reported blinding analysts to the intervention status (CHARISMA 2011). None of the remaining RCTs or controlled studies (n = 21) reported blinding the analysts to intervention status when conducting the analysis; these studies were judged to be unclear with respect to the potential risk of bias in this domain. Studies with no control group (n = 11) were considered to have a high RoB in this domain.

Blinding (HAT): the HAT assessment of blinding combined assessment of blinding of participants with an assessment of blinding of outcome assessors. This item was the HAT item with the greatest number of studies assessed to have a high potential for bias. One study reported blinding analysts to allocation status and was

assessed to have a moderate level of bias due to blinding (Grade B). The remaining studies did not report blinding or participants, outcome assessors, or analysts and were assessed as having considerable potential for bias (Grade C) due to lack of blinding. The assessment of blinding was not used in the HAT final assessment of overall study quality.

Incomplete outcome data

Incomplete outcome data (Cochrane RoB): data on study withdrawals to assess attrition bias were poorly reported, and the majority of studies were assessed as unclear for this RoB domain. Two RCTs (Howden-Chapman 2007; Howden-Chapman 2008) were conducted by the same research team and investigated warmth and energy efficiency improvements. These were both assessed as having a low RoB with respect to incomplete outcome data. Both these studies reported missing data and provided supporting data to confirm that the numbers of and reasons for study withdrawals were similar in both the intervention and the control groups. Studies which only assessed changes in health outcomes retrospectively were judged to have a high RoB for this domain (n = 3) (Breysse 2011; Health Action Kirklees; Spiegel 2003). The remaining studies were assessed to have an unclear potential for bias in this domain, largely due to the authors not reporting the reasons for missing data or unclear reporting. Due to the high levels of poor reporting, there was no clear association between attrition bias and study design or intervention category.

Withdrawals (HAT): the HAT tool assessed potential bias due to attrition at sample level by examining the proportion of the original participants who were included in the final assessment of outcomes. Seven studies (Barton 2007; Chapin 1938; CHARISMA 2011; Howden-Chapman 2008; Osman 2010; Rojas de Arias 1999; Wilner 1960) were assessed to have minimal bias attributable to withdrawals (Grade A: > 79% original sample at follow-up). Ten studies were assessed to have the potential for moderate bias (Grade B: 60% to 79% original sample at follow-up). Fourteen studies were assessed as Grade C (< 60% original sample at follow-up, retrospective study design, unclear). Studies which used area based cross-sectional data (XCBA and XUBA), rather than following a cohort of individuals, were automatically graded as C for withdrawals (Aziz 1990; McGonigle 1936; Spiegel 2003). There was no clear relationship between the HAT assessment of withdrawals and intervention category. None of the RCTs were assessed as Grade C for withdrawals.

Selective reporting

Reporting of outcomes being assessed prior to the study was rare. This did not appear to be related to intervention type or study design.

Selective reporting (Cochrane RoB): two RCTs from the UK (CHARISMA 2011; Osman 2010) were assessed to have a low risk of reporting bias. A protocol for both these studies was available and all outcomes stated in the protocols were reported in the findings papers. Another RCT (Barton 2007) was judged to have a high risk of bias in this domain as a key outcome (lung function) was listed in the trial register but was not reported in the findings paper. The remaining 30 quantitative studies were all assessed as unclear in this domain as no protocol was available. It was noted that there may have been some selective reporting of health measures, in particular those which used a fixed number of items, for example the SF-36 or other validated measures which draw on multiple items. Some studies reported using a particular measure but it was not always clear if the full measure had been reported. For example, five studies used the SF-36 as a key health outcome but did not report it in its entirety (Critchley 2004; Evans 2000; Howden-Chapman 2007; Kearns 2008; Platt 2007).

HAT: there was no assessment of reporting bias in the HAT tool.

Other potential sources of bias

In addition to the mandatory RoB items we included three items developed and recommended by the Cochrane Effective Practice and Organisation of Care Group (EPOC). These items were considered to be relevant to community based interventions such as housing improvement. The items were: similarity of outcomes measures at baseline; similarity of population characteristics at baseline; and contamination of the control group with respect to exposure to the intervention. As outlined in the protocol we planned to assess study quality using a tool developed for non-randomised studies in public health (Thomas 1998). We were keen to incorporate our assessment of quality into the RoB tool, both to provide a single assessment of study quality and also to demonstrate how the newly developed RoB tool might be used for reviews which include predominantly non-randomised studies of community interventions such as housing. One issue, baseline response rate, which is related to internal validity was included in the HAT and was not covered by the standard RoB items or the EPOC items. We incorporated these two items into the RoB tool. We also incorporated a further item on performance, which reflected variation in implementation and exposure to the intervention. The additional assessments of sources of bias are described below.

Confounding

The key characteristics that were specified as important potential confounders for this review were baseline housing quality, eligibility for housing improvement, socio-economic status, and health status. These characteristics were applied to both the RoB and the HAT assessments.

Baseline outcomes similar (RoB): this is a quality assessment item recommended by the Cochrane EPOC group and was the RoB item which was most likely to be assessed as being at low

RoB. For many of the studies it was unclear whether the key outcomes were similar across the intervention and control groups at baseline. Some studies reported baseline data for both groups but no statistical test to confirm similarity; such studies were assessed as unclear for this RoB domain. Each of the RCTs (n = 5), which were all in the warmth and energy efficiency' intervention category, demonstrated similarity of baseline outcomes, and half of the controlled studies also reported similar baseline outcome characteristics (Aziz 1990; Braubach 2008; Iversen 1986; Kearns 2008; Lloyd 2008; Platt 2007; Thomas 2005; Thomson 2007). This domain was either coded as unclear or high RoB for the remaining controlled studies. Studies without a control group (n = 10) were assessed to be at a high RoB for this domain.

Baseline characteristics similar (RoB): this is a quality assessment item recommended by the Cochrane EPOC group. Four of the RCTs, which were all in the 'warmth and energy efficiency' intervention category, each reported similar baseline characteristics. Three out of 17 CBA studies reported similar outcomes at baseline and were assessed to be at low risk of bias in this domain (Aziz 1990; Iversen 1986; Thomson 2007). In three of the controlled studies there were important differences in the baseline characteristics reported. In two of these studies (Barnes 2003; Shortt 2007) the control group was not eligible for the housing improvement, and in Kearns 2008 the control group lived in better quality housing at baseline. For the remaining controlled studies it was likely that there were differences between the intervention and the control groups at baseline, but in most studies there were insufficient data to confirm differences and what specifically these differences were. Even when the control groups were selected from a similar area with similar levels of socio-economic deprivation, it was likely that the control group was not eligible for the housing improvement and may have been living in better housing than those eligible for the housing improvement. Studies without a control group (n = 10) were assessed to be at a high RoB for this domain.

Confounding (HAT): confounding as a source of bias was assessed by HAT by combining assessment of how well confounders were controlled for either through matching of baseline characteristics and outcomes between the intervention and control groups or through control for confounding in the analysis. All five of the RCTs of warmth improvements (Barton 2007; CHARISMA 2011; Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010) were assessed to have minimal potential for bias due to confounding (Grade A). These studies were assessed to have matched the intervention and control groups or in the analysis controlled for each of the named key confounders, that is housing quality, socio-economic status, health status, and eligibility for the intervention. None of the other studies were assessed to have minimal bias due to confounding. Ten studies were judged to have moderate potential for bias (Grade B), controlling or matching for two key confounders named above. Seventeen studies were assessed to have inadequately controlled or matched key confounders. For controlled studies (Grade C) (n = 7) this was largely due to limited reporting comparing the intervention and control groups. Uncontrolled studies were all assessed as Grade C. Assessment of Grade B and C did not appear to be related to intervention type.

Contamination

Contamination (RoB): this is a quality assessment item recommended by the Cochrane EPOC group. None of the studies were clearly free from contamination, but this was largely due to lack of reporting to confirm presence or absence of contamination; this did not appear to be related to intervention type or study design. The housing interventions included in this review were not new interventions being trialed to test their efficacy or effectiveness, and were mostly available to the general public. It was therefore possible that householders in a control group may initiate housing improvements independent of the study. Where this occurred it would clearly influence the reported impacts. In most studies there were no data to confirm the presence or absence of this type of contamination resulting in an assessment of unclear in this domain.

Three controlled studies were judged to be at a high RoB for this domain (Osman 2010; Platt 2007; Thomas 2005). The remaining controlled studies (n = 20) were judged to be unclear. Studies without a control group (n = 10) were assessed to be at a high risk of bias for this domain.

Eight studies (Aziz 1990; Barnes 2003; Chapin 1938; Critchley 2004; Kearns 2008; Osman 2010; Thomas 2005; Wilner 1960) reported subgroup analysis to investigate either the relationship between exposure to a specific change in housing condition or extent of the housing condition. These data were extracted and reported as supplementary data in the synthesis.

Contamination (HAT): there was no assessment of contamination bias in the HAT tool.

Baseline response

Baseline response (RoB): this RoB item was developed from the HAT (Thomas 1998) (see Appendix 7). Levels of sample response at baseline varied, this did not appear to be related to intervention type or study design. Eight studies (Ambrose 2000; Barnes 2003; Barton 2007; Blackman 2001; Health Action Kirklees; Hopton 1996; Lloyd 2008; Somerville 2000) which had a sample which was very likely to have be representative of the target population for the study and a baseline response of > 69% or where the study sample was somewhat likely to be representative and the baseline response was > 79% were rated as being at low risk of bias in this domain.

Selection (HAT): baseline response was assessed using an item labelled as 'selection bias' in HAT. This item assessed the baseline response rate and how representative the study population was. Four studies were assessed to have minimal potential bias

due to selection (Grade A) (Ambrose 2000; Barnes 2003; Barton 2007; Hopton 1996). These studies were each assessed to have a sample which was 'very likely' to represent the population from the wider target area for the intervention and also reported a baseline response of 80% to 100%. Six studies (Blackman 2001; Halpern 1995; Health Action Kirklees; Lloyd 2008; Somerville 2000; Wilner 1960) reported data indicating that the study sample was representative of the population from the target area and had a baseline response of greater than 60%. These studies were assessed to have a moderate potential for bias (Grade B) in this item. The study population in the remaining studies (n = 23) were assessed not to be representative of the target population, either due to their characteristics or due to a low or unclear response rate at baseline. These studies were judged to have considerable potential for bias in this item (Grade C). The levels of potential bias assessed using this item did not appear to be related to study design or intervention type.

Intervention implementation

Intervention implementation (RoB): this RoB item was developed by the authors (HT and ST) to provide a summary measure of the extent to which variation in intervention implementation across the sample might influence the final impacts reported. Reporting of the variation of exposure to improved housing conditions was often unclear. Seven studies were assessed as being at a low RoB in this domain reporting minimal variation in the nature and extent of housing improvement delivered across the study population. Fourteen studies were assessed as being at a high RoB, and 13 studies did not report sufficient information to make a judgement and were assessed as unclear. The extent of variation in intervention implementation did not appear to be related to intervention category or study quality, but this was likely to be due to poor reporting of intervention implementation rather than a reflection of actual variation in implementation. In some studies the intervention was deliberately tailored to meet the needs of individual households, but there were rarely clear data reporting a breakdown of numbers of who received what type of intervention. In most studies the sample was analysed as a whole and not by extent of intervention (see also 'contamination' item for those studies which presented both intention-to-treat (ITT) and treatment on treated (TOT) analysis to reflect variation in intervention implementation).

Performance (HAT): this item combined an assessment of variation in the extent of housing improvement or intervention delivered to individuals within a study sample and variation in the extent of improvement in housing conditions experienced by the study sample (see Methods: *Intervention implementation and performance bias* and Appendix 7). This measure was developed by the review authors. Although there was some indication of variation in both the extent of the intervention delivered and the extent of improved conditions experienced within study samples, data to confirm this were rarely reported. It was, therefore difficult to assess levels of performance bias. None of the studies were assessed to have a minimal potential for bias due to performance. Eleven studies were assessed to have a moderate potential for bias (Grade B), and the majority of studies (n = 22) were assessed to have a considerable potential for bias due to variation in performance. Assessment of performance and reporting of intervention heterogeneity did not appear to be related to study design or intervention type.

Assessment of overall study quality (quantitative studies)

The HAT (Appendix 7) included an overall assessment of study quality to provide a summary indication of a study's internal validity. Two items in the HAT were not found to be sensitive to issues in the included studies of housing improvement; these were the items on blinding and data collection. Blinding was not considered to be an appropriate measure of study quality for housing studies and the questions around data collection did not accurately reflect the methods for outcome assessment in community studies relying mainly on self-administered questionnaires. For these reasons these two items were not included in the overall assessment of study quality. The item assessing performance bias was not incorporated into the assessment of overall study quality.

The overall assessment of study quality demonstrated variation across the identified studies. Five RCTs (Barton 2007; CHARISMA 2011; Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010) were assessed as having an overall grade of A. Six CBAs which followed a cohort were graded as A (Braubach 2008; Critchley 2004; Kearns 2008; Platt 2007; Thomson 2007; Wilner 1960) and seven (Barnes 2003; Evans 2000; Hopton 1996; Lloyd 2008; Rojas de Arias 1999; Shortt 2007; Thomas 2005) an overall grade of B. One UBA study (Somerville 2000) had an overall grade of B, and eight were graded as C (Allen 2005; Allen 2005a; Ambrose 2000; Blackman 2001; Chapin 1938; Iversen 1986; Molnar 2010; Wells 2000). The three XCBA studies (Aziz 1990; McGonigle 1936; Spiegel 2003), one XUBA (Halpern 1995) and both the uncontrolled retrospective studies (Breysse 2011; Health Action Kirklees) were graded as C. Study quality was strongly associated with study design, reflecting the criteria for assessment which prioritised by study design. There was no clear pattern linking overall study and intervention type but detection of a pattern was limited due to the small numbers of studies in two of the intervention categories, namely provision of basic housing needs and rehousing from slums.

The synthesis drew on the overall assessment of study quality facilitated by the HAT, prioritising those studies with an overall grade A and B. The specific quality assessment items and other key study characteristics, design, size, population, and context were reported alongside the data synthesis to facilitate transparency.

Study quality assessment for qualitative studies

The qualitative studies (n = 12) were assessed using a series of prompts which were amended to meet the requirements of this review (Appendix 8). The full quality assessment is reported in Table 2. There was some variation in quality of reporting but this did not appear to be related to intervention type. This tool focused on the clarity of reporting and the appropriateness of study methods and data. Nine studies were judged to have used appropriate methods for a clear research question and report supporting data. In four studies it was not clear if the sampling strategy was appropriate (Caldwell 2001; Decent Homes 2012; Ellaway 2000; Gibson 2011 (supplementary to included quantitative study Kearns 2008)). Three of these were also judged to be unclear with respect to the methods for sampling, these same studies did not describe their analysis and so it was unclear if the analysis was appropriate (Caldwell 2001: Decent Homes 2012; Ellaway 2000). Data from three studies (Allen 2005a; Decent Homes 2012; Kearns 2006 (supplementary to included quantitative study Kearns 2008)) were judged to be insufficiently clear to be included in the review. These studies did not have a clear research question, and the methods of sampling and analysis were unclear (Table 2).

Data extraction and calculation of standardized effect estimates

Attempts were made to contact authors to obtain further clarification and data to facilitate calculation of standardized effect sizes with varying levels of response and this inevitably resulted in a partial description and representation of some studies. Data were sometimes unclear or did not tally across a study, for example where the sample sizes for the same outcome differed between tables and text, or where it was very likely that there had been a typographical error, for example reporting a median of 100 for a scale of 1 to 100. These discrepancies were noted in the full data extraction tables (Appendix 2) but these data were not included in the final synthesis or data summaries (Table 9; Table 10; Figure 4). All data reporting direct health outcomes were extracted. One study (Platt 2007) reported variants of the same outcome, for example reporting of a condition retrospectively over different time periods (one week, one month, and six months). The key outcomes representing each condition were extracted indicating that additional data were available. Data on health service use were extracted but not included in the synthesis.

Figure 4. Summary of direction of health impacts from included studies (NB: Arrow size denotes study size not effect size)* data for children also available; ** children only; * area level data not relating to study population alone, **** adults & children aggregatedStudy design: RCT: Randomised Controlled Trial; CBA: Controlled Before & After study; UBA: Uncontrolled Before & After study; XCBA: Cross-sectional controlled Before & After study; XUBA: Uncontrolled cross-sectional Before & After study; RC: Retrospective controlled study (recall of change in health outcomes after intervention); R: Uncontrolled retrospective study. α: more than one intervention group.Effect direction: upward arrow= positive health impact, downward arrow= negative health impact, sideways arrow= mixed effects/conflicting findingsSample size: Final sample size (individuals) in intervention group Large arrow >300; medium arrow 50-300; small arrow <50Statistical significance: Black arrow p<0.05; grey arrow p>0.05; empty arrow= no statistics/data reportedStatistical tests: Controlled studies- Difference between control and intervention group at follow-up (unless stated); a Difference in change between control and intervention group; b Change within intervention group only; Uncontrolled studies: Change since baselineNumber of outcomes within each category synthesis is 1 unless indicated in subscript beside effect direction Synthesis of multiple outcomes within same outcome categoryWhere multiple outcomes all report effect in same direction and with same level of statistical significance, report effect direction and indicate overall level of statistical significanceWhere direction of effect varies across multiple outcomes:Report direction of effect and statistical significance where 70% of outcomes report similar direction and similar statistical significance.If <70% of outcomes report consistent direction of effect report no clear effect/conflicting findings ◂ ▸ (size to reflect sample size)Where statistical significance varies:If direction of effect similar AND >60% outcomes statistically significant, report as statistically significant (black arrow).If direction of effect similar AND <60% outcomes statistically significant, report as not statistically significant (grey arrow).**

Author Year	Study design	Study quality	Housing condition	Interv'n integrity	Final Sample Int/Cont	Time since intervention	General health	Respiratory	Mental	Illness/ symptoms
Intervention: Warmth & Energy Efficiency improvements (post 1980) (n=15)										
CHARISMA 2011 ** *(sub-group)*	RCT	A	^	C	19/19	11 months		▲	▲	
Osman et al 2010	RCT	A	▲	C	45/51	5 months	▼[a]	▼[a]4		
Howden-Chapman et al 2008 **	RCT	A	▲	C	175/174	4-5 months	▲	▲11		◂▸4
Braubach et al 2008	CBA	A	^	C	~210/165	5-8 months	^	^	▼	
Barton et al 2007 *, ****	RCT	A	◂▸	C	193/254	3-10 months	<>	◂▸3	<>2	▼2
Howden-Chapman et al 2007 *	RCT	A	▲	C	1689/1623	<1 year	▲3	▲5	▲3	
Platt et al 2007	CBA	A	▲	B	1281/1084	1-2 years	▲[a]3	◂▸4	◂▸[a]4	◂▸5
Lloyd et al 2008	CBA	B		C	9/27	1-2.5 years				▲[a]
Shortt et al 2007	CBA	B	▲[b]	C	46/54	1-3.5 years		◂▸3	▲	▲3
Somerville et al 2000 **	UBA	B	▲[b]	B	72	3 months		▲[b]7		
Hopton et al 1996 **	CBA	B	▲[b]	C	55/77	5-11 months		◂▸2	◂▸3	◂▸10
Allen 2005	UBA	C	^[b]	C	16	<1 year			▲[b]	
Allen 2005 a	UBA	C	^[b]	C	24	<3 years	▼		▲[b]3	
Health Action Kirklees 2005	R	C	^[b]	B	102	2-8 months				^[b]
Iversen et al 1986	CBA	C	^[b]	B	106/535	3-6 months		▲		◂▸[b]3
Intervention: Rehousing/retrofitting +/- neighbourhood renewal (post 1995) (n=11)										
Kearns et al 2008 *	CBA	A	▲	C	262/284	24 months	▲3	▼	▲4	◂▸3
Thomson et al 2007	CBA	A	▲	B	50/50	12 months	◂▸2		◂▸[a]	
Critchley et al 2004	CBA	A	^	B	~109/137	1-12 months	<>[b]		<>[b]	
Thomas et al 2005	CBA	B	◂▸	C	585/759	22 months			▼[b]	
Barnes et al 2003	CBA	B	◂▸	C	45/45	18 months	▲4		▲	▲
Evans et al 2002	CBA	B	<>[b]	C	17/17	6-18 months	^[b]			▼[b]
Breysse et al 2011*	R	C	▲	C	22	12-18 months	◂▸	▲		◂▸
Molnar et al 2010 *	UBA	C	<>[b]	C	19	5 years				<>[b]4
Blackman et al 2001 *	UBA	C	<>[b]	C	166	5 years	▼[b]	▼[b]2	▲[b]	
Wells 2000	UBA	C	▲[b]	B	23	2-3 years			▲[b]	
Ambrose 1999	UBA	C	▲[b]	C	227	4 years		◂▸[b]2	▲[b]	◂▸[b]2
Halpern 1995	XUBA	C		C	27	10 months			▲2	
Intervention: Provision of basic housing needs/low or middle income country intervention (n=3)										
Rojas de Arias et al 1999	CBA *	B	▲	B	229/132	3-36 months				▲[b]
Spiegel et al 2003	XCBA	C	<>	C	896/807	1-4 years	◂▸			
Aziz et al 1990 **, ****	XCBA	C	^	B	~>200/200	2-3 years				▲4
Intervention: Rehousing from slums (pre 1965) (n=3)										
Wilner et al 1960	CBA	A	^	B	1891/2893	<1 year			▲[a]4	◂▸[a]2
Chapin 1938	UBA	C	V[b]	B	171	8-19 months			^[b]	
McGonigle et al 1936 *, ***	XCBA	C	^	C	<152/289	5 years				V

Where impacts were reported both as adjusted and unadjusted data, the adjusted data were extracted as the quality assessment of the study included an item on control for confounding which reflected the nature of confounders adjusted for in the analysis. Where only unadjusted data were reported this was extracted. A note of what variables were adjusted for has been reported alongside the data (Data and analyses).

The time of follow-up since the intervention varied (range three months to five years) with the exception of one study which revisited the intervention villages and reported area level data nine years after the provision of pit latrines (Aziz 1990). Eleven studies (Aziz 1990; Barnes 2003; Barton 2007; Breysse 2011; CHARISMA 2011; Hopton 1996; Iversen 1986; Kearns 2008; Platt 2007; Wells 2000; Wilner 1960) reported follow-up data for more than one period (Table 11). These studies all differed from each other in at least one of the following: intervention category, study design, study quality, or reported outcomes. The data were therefore not amenable to synthesis so it was not necessary to prioritise a particular time point. Data for the synthesis drew on the final follow-up time point and the time since intervention was reported in the narrative synthesis, noting the long follow-up of Aziz 1990 as an exception.

Subgroup analysis by exposure to intervention

In one study, included in the 'warmth improvement' category, the intervention was primarily mould removal and improved ventilation. This intervention was not included in the review, however a subgroup of the intervention received central heating and analysis of this specific group was reported so the data were included (CHARISMA 2011).

Some studies reported the main analysis and further subgroup analysis comparing reported impacts across groups with varying levels of exposure to the intervention, only one of the studies reported this as an ITT and TOT analysis (Osman 2010). The analysis of subgroups or a TOT analysis was valuable for those studies which had high levels of contamination, where the distinction between the intervention and control groups with respect to exposure to the intervention was unclear (Aziz 1990; Barnes 2003; Chapin 1938; Critchley 2004; Kearns 2008; Osman 2010; Thomas 2005; Wilner 1960), or in studies where there was considerable variation in the extent of housing improvement received by the intervention group. Where both ITT and TOT were reported these were extracted and are reported in the full data extraction tables (Appendix 2). The summaries and synthesis of reported impacts (Table 9; Table 10; Figure 4; Effects of interventions) prioritised the ITT analysis. Subgroup analysis or TOT analyses were also reported in the narrative synthesis.

Data and subgroup analysis for equity issues

Where available, socio-demographic data on gender, age, socio-economic status, and ethnicity of the study population were extracted and reported in Table 4; Table 5; Table 6; and Table 7.

Few studies examined differential impacts across subgroups relevant to equity issues, but where available these were reported in the synthesis. Two studies of rehousing and neighbourhood renewal reported some impacts by gender (Critchley 2004; Thomas 2005). One study of neighbourhood improvements in Cuba reported changes in smoking, physical activity, and self-reported health by four age groups and by gender creating eight subgroups in both the intervention and control groups (Spiegel 2003). One study of improved housing to reduce transmission of Chagas disease in Paraguay reported the findings by gender (Rojas de Arias 1999). A summary of the findings is included in the synthesis under *Socio-economic impacts*. None of the other studies reported impacts by any of the key subgroups identified to be relevant to equity issues, that is gender, socio-economic status, educational status, or religion.

Calculation of standardized effect estimates

Of the 33 included quantitative studies, 22 included a concurrent control group. Data from the controlled studies were scrutinized using prompts in the Comprehensive Meta-Analysis software and RevMan software to calculate standardized effect estimates. Eleven out of the 19 better quality (Overall Grade A and B) controlled studies reported data which enabled calculation of a standardized effect size (Barnes 2003; Barton 2007; Braubach 2008; Hopton 1996; Howden-Chapman 2007; Howden-Chapman 2008; Kearns 2008; Platt 2007; Shortt 2007; Thomson 2007; Wilner 1960). These are reported in the Data and analyses sections grouped by intervention category, outcome category, and by experimental and non-experimental study design. A summary table of the standardized effect estimates is also provided (Table 12). The range of outcomes varied considerably limiting the possibility for an appropriate meta-analysis. Outcomes were predominantly reported as dichotomous variables, but in some cases both dichotomous and continuous variables were reported within an outcome category. To enable comparison of effect size estimates for an outcome category the effect estimate was reported as an odds ratio regardless of whether the outcome was dichotomous or continuous. The transformation from a standardized mean difference for continuous data to an odds ratio was conducted by the Comprehensive Meta-Analysis software, the model for transformation used is detailed in Appendix 10 (Borenstein 2009). Poor reporting limited the calculation of a standardized effect estimate in the remaining 10 controlled studies. For example, in some studies the total sample size was unclear, or there was no reporting of standard error or standard deviation or confidence intervals to accompany

reported change values. None of the studies reported standardized effect size data for more than one time point; an indication of the time since intervention accompanied the reported standardided effect estimates (see Data and analyses; Table 12).

Reporting bias

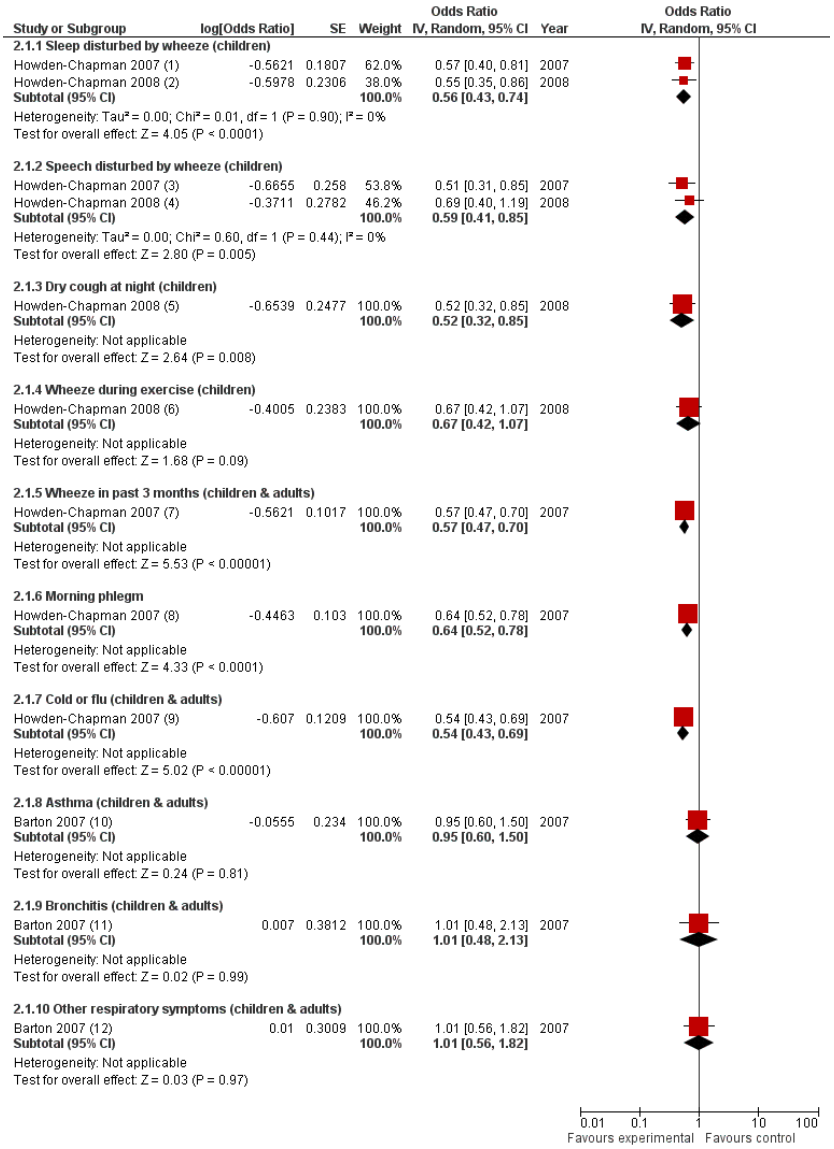
The small number of studies for which it was possible to derive a standardized effect estimate limited the usefulness of a funnel plot to investigate reporting bias. Ten of the better quality (Overall Grade A and B) controlled studies but none of the poorer quality controlled studies (Overall Grade C) reported data amenable to calculation of a standardized effect estimate for various outcome domains. Calculation of standardized effect estimates was not appropriate for uncontrolled studies. This meant that it was not possible to produce a funnel plot which adequately represented the better quality controlled studies. In addition, this review included controlled and uncontrolled studies in the final synthesis according to study quality, suggesting that the review included data from a wide range of studies with regard to study size and quality. There did appear to be some association between study size and reported impacts. Although a formal sensitivity analysis was limited due to the lack of standardized effect data, this was discussed in more detail below (see *Sensitivity analysis*).

Effects of interventions

Synthesis of reported effects

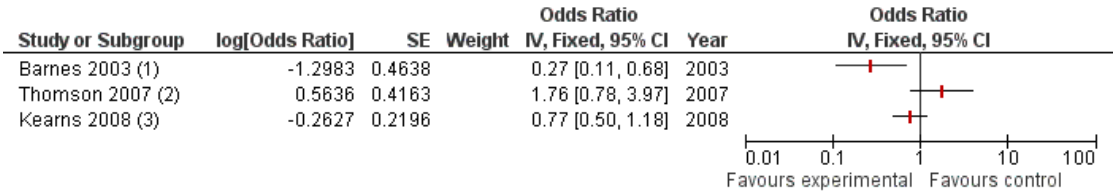
Following critical appraisal and data extraction data were synthesized according to intervention and outcome type. Few studies presented sufficient data to allow calculation of a standardized effect size and the synthesis was predominantly narrative. A summary of available standardized effect estimates is presented in the Data and analyses section (see also Figure 5; Figure 6; Figure 7; Table 12). In addition, a visual summary of the direction of all reported impacts has also been tabulated (Table 10) in what we call an 'effect direction plot'. The effect direction plot allows for effect directions of multiple outcomes and intermediate outcomes, such as change in housing conditions, to be summarized visually. The plot included an indication of study design, study quality, study size, as well as the type of analysis presented in each study or where no statistics were available. These data were further synthesized to provide a one page summary of reported effect directions for each domain regardless of how many outcomes were reported in a single domain (Figure 4). It should be noted that due to the lack of standardized effect sizes the synthesis predominantly reported direction of effect only.

Figure 5. Forest plot of comparison: 2 Standardised effect estimates for respiratory outcomes following warmth & energy efficiency improvements (post 1985), outcome: 2.1 Experimental studies.



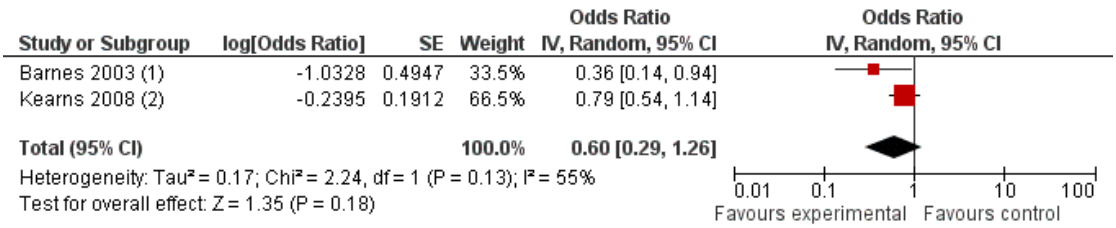
(1) Adjusted for age group, sex, ethnicity, and household. <1 year since intervention.
(2) Adjusted for age, sex, ethnicity, region, parental allergy, indoor air quality, and baseline outcome value where available. 4-5 months sin
(3) Adjusted for age group, sex, ethnicity, and household. <1 year since intervention.
(4) Adjusted for age, sex, ethnicity, region, parental allergy, indoor air quality, and baseline outcome value where available. 4-5 months sin
(5) Adjusted for age, sex, ethnicity, region, parental allergy, indoor air quality, and baseline outcome value where available. 4-5 months sin
(6) Adjusted for age, sex, ethnicity, region, parental allergy, indoor air quality, and baseline outcome value where available. 4-5 months sin
(7) Adjusted for age group, sex, ethnicity, baseline outcome value, and household. <1 year since intervention.
(8) Adjusted for age group, sex, ethnicity, region and household. <1 year since intervention.
(9) Adjusted for age group, sex, ethnicity, and household. <1 year since intervention.
(10) Unadjusted. <2 years since intervention.
(11) Unadjusted. <2 years since intervention.
(12) Unadjusted. <2 years since intervention.

Figure 6. Forest plot of comparison: 5 Standardised effect estimates for general health outcomes following rehousing or retrofitting with or without neighbourhood renewal (post-1995) (non-experimental studies), outcome: 5.1 Poor/fair self-reported health.



(1) Unadjusted, no indication of missing data. 18 months since intervention.
(2) Unadjusted. One year since intervention.
(3) Adjusted for baseline outcome measure. Two years since intervention.

Figure 7. Forest plot of comparison: 5 Standardised effect estimates for general health outcomes following rehousing or retrofitting with or without neighbourhood renewal (post-1995) (non-experimental studies), outcome: 5.3 Health not improved/worse since one year ago.



(1) Unadjusted, no indication of missing data. 18 months since intervention.
(2) Adjusted for baseline outcome measure. Two years since intervention.

The synthesis that follows below reported the outcomes by domain (general health, respiratory health, mental health, and illness or symptoms) as well as the reported impacts on housing conditions, socio-economic outcomes, and any additional analysis by subgroups relevant to equity issues. The 'warmth and energy efficiency' studies included both experimental and non-experimental studies. The results of these studies have been presented separately, but a synthesis of the reported impacts for the better quality experimental and non-experimental studies (Overall Grade A and B) was presented at the start of each outcome domain. For the other intervention categories (rehousing or retrofitting, LMIC interventions, and rehousing from slums) there were no experimental studies and the summary of the better quality non-experimental studies (Overall Grade A and B) was presented without a preceding overview. The findings of those studies assessed to have an Overall Grade of C were reported but not included in the final synthesis. The poorer quality studies (Overall Grade C) were examined to identify additional impact types reported and the existence of adverse impacts not reported in other studies.

For each of the intervention categories a summary table was embedded in the text providing a summary of the studies, their characteristics, and key elements of study quality from the RoB tool and the HAT. Some items from the RoB tool and the HAT have

not been included in these summary tables to allow the most pertinent aspects to be immediately available to the reader. Specifically, items on allocation, blinding, and data collection have not been reported here as there was little variation in blinding, which was not used in the studies, and the usefulness of the data collection item was unclear. These items are fully reported in Table 8.

Sensitivity analysis

The ability to perform a formal sensitivity analysis was limited due to the small number of studies and limited outcome data amenable to calculation of a standardiZed effect size and a meta-analysis. A less formal sensitivity analysis was also limited due to the small number of studies in any single intervention category reporting similar outcomes which were also similar with respect to specific study characteristics, such as study design and other markers of internal validity, as well as study population. The largest group of studies reporting in the same outcome domain comprised 10 studies reporting respiratory outcomes following warmth and energy efficiency improvements but this was comprised of a mix of studies of children and adults and experimental and non-experimental studies.

An investigation of variation in reported impacts for each outcome category and relationship to study characteristics, in particular study design and other aspects of study quality, was carried out by examining the full data for groups of studies in the Access database and the summaries of reported effect directions in Table 10 and Figure 4. Compared with studies in other intervention categories, 'warmth and energy efficiency' studies were most likely to report statistically significant improvements in reported respiratory and illness outcomes. This did not appear to be related to whether the study used an experimental design or not, but may have been related to study size. The group of 'warmth and energy efficiency' studies contained all the RCTs (n = 5) and also two large studies with a sample of over 2000 people. It was possible that the higher number of statistically significant effects was related to study size, larger studies having a greater power to detect small changes that are statistically significant. A greater number of statistically significant impacts may not have been related to a greater effectiveness of the intervention. The mean sample sizes for the quantitative studies according to intervention categories were:

- warmth and energy efficiency improvements (post-1985), n = 15 studies: mean sample size 540 (by Overall Grade A, B, C 997/85/196);
- rehousing or retrofitting ± neighbourhood renewal (post-1995), n = 12 studies: mean sample size 239 (by Overall Grade A, B, C 303/489/84);
- provision of basic housing in low or middle income country (post-1990), n = 3 studies: mean sample size 1051 (by Overall Grade B, C 229/1051);
- rehousing from slums (pre-1970), n = 3: mean sample size 1749 (by Overall Grade A, C 4784/232).

Compared to the studies of rehousing, the warmth studies were larger, in particular the better quality warmth studies with an Overall Grade A. Among the studies of rehousing and retrofitting, the poorer quality studies (Overall Grade C) appeared to be more likely to report statistically significant improvements in mental health outcomes compared with the better quality studies (Overall Grade A and B). For other outcome domains and intervention categories the numbers of studies were insufficient to detect any relationship between reported effect direction and study characteristics.

Warmth and energy efficiency improvements (post-1985), n = 17 (quantitative: 15; qualitative: 7)

Seventeen studies assessed the health impacts of warmth and energy efficiency improvements. Fourteen studies reported quantitative data (Allen 2005; Allen 2005a; Barton 2007; Braubach 2008; Health Action Kirklees; Hopton 1996; Howden-Chapman 2007; Howden-Chapman 2008; Iversen 1986; Lloyd 2008; Osman 2010; Platt 2007; Shortt 2007; Somerville 2000) and six studies reported qualitative data; three of these did not report any quantitative assessment of health impacts (Caldwell 2001; Heyman 2011; Warm Front 2008) and three did (Allen 2005a; Barton 2007; Shortt 2007). One small and poorly conducted RCT of warmth improvements was excluded due to poorly reported data which were difficult to interpret (Eick 2011). Impacts were reported at between three months and 3.5 years after the intervention.

Four of the warmth studies were RCTs (Barton 2007; Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010). Six quantitative studies used a CBA design (Braubach 2008; Hopton 1996; Iversen 1986; Lloyd 2008; Platt 2007; Shortt 2007), three studies used a UBA design (Allen 2005; Allen 2005a; Somerville 2000), and one study was an uncontrolled retrospective study (Health Action Kirklees). A summary table of the included studies, their study design, and assessment of study quality is provided below (Table C). Further details of the study characteristics and reported data are provided in Table 9 (see also Table 10; Figure 4; Appendix 2).

Table C. Summary of characteristics of quantitative warmth and energy efficiency studies

Author, Publication year, Country	Study design	Final sample Int/Cont; Population	Time since intervention	Summary of Study Quality					
				Selection	Confounding	Withdrawals	**Overall grade (HAT)**	Performance	No. of items at low Risk of Bias
Warmth and energy efficiency improvements (after 1980)									
CHARISMA 2011 UK	RCT	19/19*	11 months	C	A	A	**A**	C	5
Osman 2010 UK	RCT	45/133 Elderly population with COPD	5 months	C	A	A	**A**	C	3
Howden-Chapman 2008 New Zealand	RCT	175/174 Children diagnosed with asthma	4-5 months	C	A	A	**A**	C	4
Braubach 2008 Germany	CBA	~210/165 General adult population	5-8 months	C	B	B	**A**	C	1
Barton 2007 UK	RCT	193/254 Adults and children	3-10 months	A	A	A	**A**	C	5
Howden-Chapman 2007 New Zealand	RCT	1689/1623 Adults and children with respiratory disease	<1 year	C	A	B	**A**	C	4
Platt 2007 UK	CBA	1281/1084 Elderly population	1-2 years	C	B	B	**A**	B	2

(*Continued*)

Lloyd 2008 UK	CBA	9/27 General adult population	1-2.5 years	B	C	C	**B**	C	2
Shortt 2007 UK	CBA	46/54 Elderly population	1-3.5 years	C	C	B	**B**	C	0
Somerville 2000 UK	UBA	72 Children with asthma	3 months	B	C	B	**B**	B	1
Hopton 1996 UK	CBA	55/77 Children	5-11 months	A	C	C	**B**	C	1
Allen 2005 UK	UBA	16 Adults with respiratory or cardiac condition	<1 year	C	C	C	**C**	C	0
Allen 2005a UK	UBA	24 Adults diagnosed with heart condition	<3 years	C	C	C	**C**	C	0
Health Action Kirklees UK	RU	102 Adults with respiratory or cardiac condition	2-8 months	B	C	C	**C**	B	1
Iversen 1986 Denmark	CBA	106/535 General adult population	3-6 months	C	C	C	**C**	B	3

* only this subgroup of whole sample (n=89/89) who received warmth improvements, with controls matched for timing of intervention

Warmth and energy efficiency interventions: context, population, intervention

Context and population

All of the 'warmth and energy efficiency' studies were conducted after the year 1980 in high income countries, and 13 were conducted after 2000, indicating that they were relevant to modern day housing conditions. Most of the interventions were delivered to low income households, four studies included only children (CHARISMA 2011; Hopton 1996; Howden-Chapman 2008; Somerville 2000), two studies included children and adults (Barton 2007; Howden-Chapman 2007), and in four studies the majority of the population were elderly (Allen 2005a; Osman 2010; Platt 2007; Shortt 2007). Seven studies targeted households where at least one member had a diagnosed cardiac or respiratory condition (Allen 2005; Allen 2005a; CHARISMA 2011; Health Action Kirklees; Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010) (see Table 4 for details of study population). Two of the studies were conducted in New Zealand (Howden-Chapman 2007; Howden-Chapman 2008), one in Denmark (Iversen 1986), one in Germany (Braubach 2008), and 14 were conducted in the UK (this included the three additional qualitative studies Caldwell 2001; Heyman 2011; Warm Front 2008).

Warmth and energy efficiency interventions

The 'warmth and energy efficiency' interventions varied and included installation, upgrade, repair of central heating, installation of insulation (roof or cavity wall, or both), or double glazing, or any combination of these. The interventions were delivered to individual houses and were often tailored according to need, thus varying across the study sample. Improved warmth and energy efficiency was not always the only aim of these interventions. One study's main purpose was to improve air quality by replacing unflued heaters with a less polluting alternative (Howden-Chapman 2008). Another study in the UK was primarily aiming to remove mould and improve air quality, but a small group within the sample also received central heating and a subgroup analysis was conducted and included in the review (CHARISMA 2011). Some interventions incorporated additional activities such as advice on welfare benefits or additional domestic repairs (Allen 2005; Allen 2005a; Jackson 2011; Platt 2007; Shortt 2007). Details of the interventions are presented in Table 4.

General health impacts (n = 6)

Summary from better quality experimental and non-experimental studies (Overall Grade A and B) (n = 5)

A range of general health measures was reported across five of the better quality studies (Overall Grade A and B, 3 RCTs). Four studies (Braubach 2008; Howden-Chapman 2007; Howden-Chapman 2008; Platt 2007) including two RCTs from New Zealand reported a positive impact on general health following the warmth improvements, one of these studies was in children (Howden-Chapman 2008) and another included children and adults (Howden-Chapman 2007). One RCT among elderly people in the UK who had a pre-existing respiratory condition reported a negative impact on general health, but this was not apparent when a TOT analysis was conducted (Osman 2010).

Experimental studies (Overall Grade A and B) (n = 3)

Three RCTs assessed impacts on general health measures (Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010); each of these RCTs targeted households where at least one member had a diagnosed respiratory condition. Two of the RCTs were conducted in New Zealand by the same research team among children (Howden-Chapman 2008) and adults (Howden-Chapman 2007) and reported statistically significant lower levels of fair or poor health among the intervention group compared with the control group (OR 0.48, 95% CI 0.31 to 0.74, adjusted, Howden-Chapman 2008 (children); OR 0.50, 95% CI 0.38 to 0.68, adjusted, Howden-Chapman 2007 (adults)). The third RCT from the UK involving elderly people reported a small and non-statistically significant difference in the change in general health (Euroqual analogue -0.3, 95% -1.2 to 0.6, adjusted) between the intervention and control group (Osman 2010). There was a small deterioration in this outcome among the intervention group. There was a high RoB from contamination in this study. The TOT analysis reported a small and non-statistically significantly greater improvement in general health among those who had received the intervention compared with those who had not (Euroqual analogue +0.1, -0.8 to 0.9, adjusted).

Non-experimental studies (Overall Grade A and B) (n = 2)

Two CBA studies of adults assessed impacts on general health measures (Braubach 2008; Platt 2007). Both of these studies were assessed to have an Overall Grade A. In one German study there was a greater improvement in self-reported health in the intervention group compared to the control group (proportion reporting self-reported health improved since intervention 29% versus 13%) (Braubach 2008). In one Scottish study a small but statistically significant improvement was observed in two SF-36 domains (physical functioning +2.51, 95% CI 0.67 to 4.37, adjusted; general health +2.57, 95% CI 0.90 to 4.34, adjusted); no difference in change was observed for the other seven SF-36 domains (Platt 2007).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 1)

One UBA study assessed to be of poor quality reported a small and non-statistically significant deterioration in general health (SF-

36 Physical Component Score (PCS)), 36.1 versus 35.8 (Allen 2005a).

Respiratory health impacts (n = 11)

Summary from better quality experimental and non-experimental studies (Overall Grade A and B) (n = 10)

A diverse range of respiratory outcomes was reported across the studies which varied with respect to study design, study population, and the type and implementation of the intervention. The impacts were reported between three and 40 months following the intervention. There were improvements in the wide range of respiratory outcomes reported in six of the better quality studies (Overall Grade A and B, 3 RCTs) for both children and adults (Barton 2007; Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010; Platt 2007; Shortt 2007). This included two well conducted RCTs from New Zealand where the intervention was targeted at households with inadequate warmth and where at least one household member had a pre-existing respiratory condition. In both these studies respiratory health was better among the intervention group compared with the control group following the intervention, the differences were statistically significant (Howden-Chapman 2007; Howden-Chapman 2008). One RCT from the UK reported a negative impact on respiratory health among the intervention group of elderly people with a chronic respiratory condition, but a TOT analysis reported improved respiratory health among those who had actually received the intervention (Osman 2010). Two non-experimental studies from the UK of a predominantly elderly population reported both negative and positive impacts across the different respiratory measures used, suggesting little overall impact (Platt 2007; Shortt 2007). A further subgroup (n = 19) within an RCT (CHARISMA 2011) (Overall Grade A) in the UK reported non-statistically significant improvements in asthma following installation of central heating.

Experimental studies (Overall Grade A and B) (n = 5)

Five RCTs assessed a range of respiratory impacts (Barton 2007; CHARISMA 2011; Howden-Chapman 2007; Howden-Chapman 2008; Osman 2010); these RCTs were all assessed to have an Overall Grade A. Two of these studies included both adults and children (Barton 2007; Howden-Chapman 2007), two studies included only children (CHARISMA 2011; Howden-Chapman 2008), and one study was of elderly people (Osman 2010). Within these studies 27 different measures of respiratory health were used and each study reported their own unique collection of diverse outcomes, for example Barton 2007 reported 10 different respiratory measures, and many of the measures were related to asthma including wheeze and cough.

In the two New Zealand RCTs (Howden-Chapman 2007; Howden-Chapman 2008) two respiratory outcomes that were assessed in children were the same: 'sleep disturbed by wheeze' and 'speech disturbed by wheeze'. These were also reported as standardized effects and so were amenable to meta-analysis. The outcomes were assessed at four to five months (Howden-Chapman 2008) and up to one year after the intervention (Howden-Chapman 2007). There were low levels of heterogeneity reported for both outcomes ('sleep disturbed by wheeze' tau^2= 0.00, I^2 = 0%, Chi ^{2} P = 0.90; 'speech disturbed by wheeze' tau^2= 0.00, I^2 = 0%, Chi^2 P = 0.44), and the data were synthesized using a fixed-effect model (Figure 5). The overall reported odds for 'sleep disturbed by wheeze' was OR 0.56 (95% CI 0.43 to 0.74) and for 'speech disturbed by wheeze' OR 0.59 (95% CI 0.41 to 0.85) suggesting a beneficial effect of the warmth improvements. Both these studies targeted households where at least one member had an existing respiratory condition. These studies also reported statistically significant better outcomes following housing improvement among other respiratory measures when compared to the control group. In one study there were statistically significantly fewer reports for four out of five different measures of cough among children (Howden-Chapman 2008), and in another study statistically significantly fewer reports of morning phlegm and cold or flu symptoms among adults (Howden-Chapman 2007). An RCT conducted in the UK assessed reported impacts in adults and children but did not target those with a pre-existing condition (Barton 2007). Barton 2007 reported a larger improvement in cough, wheeze, and breathlessness among the intervention group compared to the control group for both adults and children, but these differences were not statistically significant. Aggregated data for adults and children reported an improvement among the intervention group for asthma and other respiratory conditions but not for bronchitis but these changes were not statistically significant. Another RCT (CHARISMA 2011) of mould removal and installation of a loft fan to improve ventilation in homes with an asthmatic child was conducted in the UK. Some of the homes also received central heating and subgroup analysis was reported comparing this group with those who received no intervention. There were improvements in the in PedsQL (paediatric asthma quality of life measure) subscores for asthma and 'physical' but neither of these changes were statistically significant (asthma 9.3, 95% CI -1.9 to 20.6; physical 10.3, 95% CI -1.7 to 22.4).

A further RCT conducted in the UK included elderly people with pre-existing chronic obstructive pulmonary disease (Osman 2010). Osman 2010 reported respiratory outcomes, assessed by the St Georges Respiratory Questionnaire (SGRQ); the total SQRG score following the warmth improvements indicated poorer respiratory health among the intervention group compared with the control group (-0.9, 95% CI -6.7 to 4.9, adjusted) but this was not statistically significant. However, there was a high RoB from contamination in this RCT and the TOT analysis reported a better total SQRG score among those in the intervention and control

groups who had received warmth improvements than with those who had not (-5.7, 95% CI -0.7 to -10.7, adjusted) but this difference was statistically significant.

Non-experimental studies (Overall Grade A and B) (n = 5)

Four CBA studies and one UBA (Somerville 2000) study were assessed to have an Overall Grade of A (Braubach 2008; Platt 2007) or B (Hopton 1996; Shortt 2007; Somerville 2000). Four of these were conducted in the UK (Hopton 1996; Platt 2007; Shortt 2007; Somerville 2000) and one in Germany (Braubach 2008). Fifteen different respiratory outcomes were reported across the studies and no two studies reported data amenable to statistical synthesis. The focus of the studies varied and the respiratory outcomes used were diverse.

In two UK studies of adults there were conflicting findings within the studies with respect to respiratory impacts. Both studies report a mix of positive and negative impacts for similar respiratory outcomes. Platt 2007 reported a statistically significant higher level of 'ever diagnosed nasal allergy' (OR 1.52, 95% CI 1.50 to 2.20, adjusted), less asthma (OR 0.92, 95% CI 0.63 to 1.34, adjusted), higher levels of bronchitis (OR 1.29, 95% CI 0.97 to 1.72), and no difference in 'other respiratory symptoms' (no data provided) in the intervention group compared to the control group (Platt 2007). In the other UK study of adults, odds of reporting asthma were lower (OR ~0.57, 95% CI 0.099 to 3.254) but reports of chest infection (OR ~1.88, 95% CI 0.495 to 7.10) and pneumonia (OR ~3.60, 95% CI 0.14 to 90.36) were higher among the intervention group compared to the control group following the intervention; these differences were not statistically significant (Shortt 2007). In the German study of adults there were small reductions in the proportion reporting having experienced 'common cold' (-2%), 'acute' and 'chronic' bronchitis (-0.5% and -0.5% respectively) but no change in 'asthma' following the housing improvement (Braubach 2008).

One CBA study of children (Hopton 1996) reported fewer reports of 'persistent cough' and 'runny nose' following the intervention when compared to the control group, but a higher number of reports of wheezing (persistent cough OR ~0.97, 95% CI 0.44 to 2.149; runny nose OR ~0.686, 95% CI 0.337 to 1.39; wheezing OR ~1.125, 95% CI 0.467 to 2.71) although none of the differences were statistically significant. One UBA study of children (Somerville 2000) reported statistically significant improvements for cough, wheeze, and blocked nose among the intervention group (before versus after (median) cough by day 2 versus 1 P < 0.01; cough by night 3 versus 1 P < 0.01; wheeze by day 2 versus 1 P < 0.01; wheeze by night 2 versus 0 P < 0.01; breathless with exercise 2 versus 1 P < 0.01; breathless 1 versus 0 P < 0.01; runny nose 2 versus 0 P < 0.01; blocked nose 2 versus 0 ns; hay fever 0 versus 0 ns).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 1)

In one poorer quality CBA study, reports of dry throat in adults three to six months after the installation of new windows were lower among the intervention group compared with the control group (OR 0.67) (Iversen 1986); no statistical test data were reported to indicate confidence intervals.

Mental health impacts (n = 9)

Summary from better quality experimental and non-experimental studies (Overall Grade A and B) (n = 7)

Seven of the better quality studies (Overall Grade A and B) reported impacts on mental health outcomes between five and 12 months after the intervention; three of these used an RCT design (Barton 2007; CHARISMA 2011; Howden-Chapman 2007). The RCT from New Zealand reported statistically significant better mental health in adults for all four outcomes assessed compared to the control group (Howden-Chapman 2007). The remaining studies reported a mix of positive and negative impacts but none of the changes or differences between the intervention and the control groups were statistically significant. Two studies were of children (CHARISMA 2011; Hopton 1996).

Experimental studies (Overall Grade A and B) (n = 3)

Two RCTs that assessed mental health impacts in adults (Barton 2007; Howden-Chapman 2007). Howden-Chapman 2007 reported statistically significant improvements in three SF-36 domains: lower levels of 'low happiness' and 'low vitality' when compared to the control group (OR 0.56, 95% CI 0.41 to 0.77; OR 0.51, 95% CI 0.41 to 0.64, adjusted, respectively), and increased improvement in 'role emotional' relative to the control group (+10.9%, P < 0.001); however the full SF-36 Mental Component Score (MCS) was not reported. Barton 2007 reported no improvement in the General Health Questionnaire (GHQ), but no data were reported. A third RCT reported subgroup analysis,a small improvement in the PedsQL (0 to 100) psychosocial scale among children was reported but this difference was not statistically significant (+0.6, 95% CI -10.1 to 11.3).(CHARISMA 2011)

Non-experimental studies (Overall Grade A and B) (n = 4)

Four non-experimental CBA studies reported mental health impacts (Braubach 2008; Hopton 1996; Platt 2007; Shortt 2007) across different measures. Two of the studies were assessed to have an Overall Grade A (Braubach 2008; Platt 2007) and two were assessed as Overall Grade B (Hopton 1996; Shortt 2007). The studies reported conflicting findings within and between the studies and the different outcomes assessed, but none of these studies

reported statistically significant effects. In one study depression was higher in the intervention group (OR 1.40, 95% CI 0.329 to 5.987) (Braubach 2008); in a second study reported mental illness was lower in the intervention group (OR ~0.26, 95% CI 0.05 to 1.30) (Shortt 2007). Platt 2007 reported smaller reductions or improvements in the intervention group compared to the control group for the SF-36 mental health measures (difference in change in regression co-efficient (adjusted) for mental health -0.22, 95% CI -1.88 to 1.30; vitality +0.02, 95% CI -1.81 to 1.87; social functioning +0.28, 95% CI -1.91 to 2.35; role emotional -0.23, 95% CI -2.68 to 2.14). In a study of children reports of 'temper tantrums' were lower among the intervention group after the housing improvement group compared to the control group but reported irritability and 'feeling down' was increased (temper tantrums OR ~0.97, 95% CI 0.44 to 2.149; irritability OR ~1.545, 95% CI 0.569 to 4.196; feeling down OR ~0.66, 95% CI 0.23 to 1.89) but the differences were not statistically significant (Hopton 1996).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 2)

Two small (n = 16; n = 24) UBA studies (Grade C) reported mental health impacts (Allen 2005; Allen 2005a) among adults with existing cardiac or respiratory conditions. These studies were of a very similar intervention and conducted by the same authors but different measures of mental health were used. Improvements in mental health were reported in both studies following the housing improvement. One of the studies reported a statistically significant reduction in the GHQ following the intervention (6.5 versus 2.6, P = 0.001) (Allen 2005). The other study reported a statistically significant improvement in the mean SF-36 MCS and the Hospital Anxiety and Depression Scale (HADS) anxiety score but not the HADS depression score (SF-36 MCS 39.7 versus 45.9, P = 0.013; mean HADS anxiety 11.9 versus 9.8, P = 0.028; HADS depression 10.9 versus 9.5, P = 0.106) (Allen 2005a).

Other illness and symptom impacts (n = 8)

Summary from better quality experimental and non-experimental studies (Overall Grade A and B) (n = 6)

A wide range of 'other' outcomes were reported by six of the better quality studies (Overall Grade A and B) which were not amenable to synthesis. For five of these studies the 'other' outcomes were not among the key outcomes being investigated. There was a mix of positive and negative impacts reported but these were rarely statistically significant. Two studies reported impacts on arthritis and rheumatism, with some suggestion that there may have been a non-statistically significant increase among the intervention group (Barton 2007; Shortt 2007). One study in Scotland investigated changes in blood pressure as its key outcome. The authors reported a statistically significantly greater reduction in blood pressure among the intervention group compared to the control group (diastolic mmHg Int/Cont -11.85mm/+8.22, P < 0.000) (Lloyd 2008). A further study in Scotland also reported a statistically significant smaller change in the number recently diagnosed with hypertension or heart disease following the intervention compared with the control group (Platt 2007).

Experimental studies (Overall Grade A and B) (n = 2)

Two RCTs reported other impacts on illness or symptom outcomes: one of these assessed impacts among UK adults and children (Barton 2007), the second RCT assessed changes among children with a respiratory condition in New Zealand (Howden-Chapman 2008). The reported symptoms were diverse.

Howden-Chapman 2008 reported additional symptoms as dummy variables in their study of children. A lower odds of diarrhoea (OR 0.72, 95% CI 0.45 to 1.16, adjusted) and vomiting (OR 0.88, 95% CI 0.55 to 1.40, adjusted) were reported; and a slightly higher odds of 'ear infection' (OR 1.16, 95% CI 0.68 to 1.99, adjusted) and 'twisted ankle' (OR 1.86, 95% CI 1.03 to 3.35, unadjusted) were reported among the intervention group, none of these differences were statistically significant. The Barton 2007 study reported fewer reports of arthritis (OR ~1.31, 95% CI 0.73 to 2.34) and rheumatism (OR ~0.52, 95% CI 0.16 to 1.67) among adults in the intervention group compared with the control group up to two years after the housing improvement. These differences were not statistically significant.

Non-experimental studies (Overall Grade A and B) (n = 4)

Four CBA studies reported other impacts on illness or symptom outcomes (Hopton 1996; Lloyd 2008; Platt 2007; Shortt 2007); all the studies were conducted in the UK. One of these studies was assessed to have an Overall Grade A (Platt 2007), the other studies were assessed as B. One study investigated impacts among children (Hopton 1996). The reported symptoms across the studies were diverse.

Platt 2007 reported a statistically significantly higher OR for the intervention for two outcomes two years after the intervention (first diagnosis of heart disease OR 0.69, 95% CI 0.52 to 0.916, adjusted; first diagnosis of hypertension OR 0.77, 95% CI 0.610 to 0.972 adjusted). There was a slightly higher level among the intervention group compared with the control group for eczema (OR 1.43, 95% CI 0.89 to 2.28, adjusted), pain (SF-36 bodily pain -1.09, 95% CI -3.33 to 4.41, adjusted), and circulation problems (OR 1.06, 95% CI 0.83 to 1.34, adjusted). These differences were not statistically significant. No statistically significant differences between the intervention group and the control group were reported for health behaviours such as smoking and drinking, and there was no significant change reported for a further 14 outcomes,

such as 'longstanding illness or disability', 'current smoker', 'had alcoholic drink in past week' (no data reported) (Platt 2007).
In the study by Shortt 2007, reports of angina, arthritis or rheumatism, and other illnesses were reduced among the intervention group following the intervention. However, when comparing post-intervention reports the difference was only statistically significant for angina (OR ~0.2, 95% CI 0.04 to 0.966). Lloyd 2008 reported a statistically significantly greater reduction in both systolic and diastolic blood pressure (systolic -19.36, P < 0.000/+2.78, P = 0.396, difference in change 22.14, 95% CI 13.77 to 31.12, P < 0.000; diastolic -11.85, P < 0.000/+8.22, P = 0.011, difference in change 20.07, 95% CI 12.70 to 27.44, P < 0.000). In one study of children (Hopton 1996) changes in nine diverse symptoms were measured. There was a small increase in the mean number of symptoms (including respiratory and mental symptoms) in both groups (before versus after intervention/control 3.69 versus 3.72/3.09 versus 3.89). Of the nine symptoms, a statistically significant difference between the intervention group and the control group following the intervention was only reported for one symptom, the intervention group reporting problems with 'poor appetite' (OR ~0.34, 95% CI 0.146 to 0.80). There was no clear impact reported for the other symptoms (aches and pains OR ~1.537, 95% CI 0.66 to 3.555; diarrhoea OR ~0.735, 95% CI 0.25 to 2.12; earache OR ~0.977, 95% CI 0.347 to 2.749; fever OR ~0.78, 95% CI 0.328 to 1.875; headache OR ~0.68, 95% CI 0.23 to 1.986; sore throat OR ~1.355, 95% CI 0.668 to 2.747; vomiting OR ~0.96, 95% CI 0.38 to 2.44; tiredness OR ~1.52, 95% CI 0.64 to 3.61).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 2)

One CBA study from Denmark (Iversen 1986) and one uncontrolled retrospective study from the UK (Health Action Kirklees) reported other impacts on illness or symptom outcomes. In the Danish study there was some suggestion of improved symptoms. A reduced normalised OR favouring the intervention was reported for joint pain (OR 0.28) and neck or back pain (OR 0.18) three to six months after the intervention (Iversen 1986). In the UK study 78% of the study sample reported an improvement in their medical condition two to eight months after the housing improvement (Health Action Kirklees).

Housing condition impacts (n = 13)

Summary from better quality experimental and non-experimental studies (Overall Grade A and B) (n = 9)

A wide range of measures of housing condition were reported across the studies, including measures of damp, cold, mould, air quality, fuel use and fuel expenditure. Among the better quality studies (Overall Grade A and B) all the studies reported improvement in some measures, but these changes in differences were not always statistically significant. The two RCTs from New Zealand reported improvements among the intervention group across all housing measures; all but one of these outcomes were statistically significantly when compared with the control group (Howden-Chapman 2008; Howden-Chapman 2007). The changes in housing condition outcomes were less clear among the UK studies. Two RCTs from the UK reported small improvements which were not statistically significant (Barton 2007; Osman 2010). The non-experimental studies, four from the UK, reported statistically significant improvements in warmth and damp (Braubach 2008; Hopton 1996; Platt 2007; Shortt 2007; Somerville 2000). One study did not report changes in housing conditions other than confirming that the intervention had been delivered to participants (CHARISMA 2011).

Experimental studies (Overall Grade A and B) (n = 4)

The intervention delivered varied, often being deliberately tailored according to individual need. There was 'considerable' variation in the intervention delivered in Barton 2007 and Howden-Chapman 2007, 'some' variation in Howden-Chapman 2008, and variation was not reported in Osman 2010. Variation in improvements in housing conditions experienced, as opposed to intervention received, across the intervention group was not reported for any of these studies.
The four experimental studies of 'warmth and efficiency' improvements reported a wide range of measures of warmth, damp, mould, air quality, and fuel use in different rooms in the home, for example living room, child's bedroom, etc. Howden-Chapman 2008 reported that the intervention homes were around 1 °C warmer than the control group homes following the intervention (17.07 °C versus 15.97 °C, 95% CI 0.54 to 1.67). All the other measures also reported improvements; the differences were all statistically significant. Howden-Chapman 2007 also reported statistically significant ORs for cold, mould, condensation, and energy use among the intervention group ('house cold most or all time' OR 0.62, 95% CI 0.04 to 0.09; 'reporting any mould' OR 0.24, 95% CI 0.18 to 0.32; 'condensation' OR 0.16, 95% CI 0.11 to 0.22; energy use OR 0.81, 95% CI 0.72 to 0.91, adjusted). A subgroup of the sample had more detailed measures assessed using instruments to assess dwelling temperature and humidity. The data suggested a small increase in mean temperature and humidity (change in temperature (°C) Int/Cont +0.6/+0.2, P = 0.05; % change in relative humidity +3.8/-1.4, P = 0.05) among the intervention group, but a statistically significantly larger reduction in the number of hours where the temperature was below 10 °C in the intervention group (-0.99/+0.45, P = 0.007). Osman 2010 did not report any statistically significant changes in energy efficiency, fuel costs, living room temperature, or humidity despite re-

porting improvement for these measures. One measure of warmth was higher among the intervention group but this was statistically significant (bedroom hours at 18 °C Int versus Cont 111.9 versus 102.2, 95% CI 22.4, 1.6 to 43.4, adjusted). The findings of the TOT analysis also reported improvements for these measures, but only the differences in energy efficiency and fuel costs were statistically significant when comparing change in those who had received the intervention with those who had not (NHER 4.8/5.6 versus 6.0/5.7, 1.1, 95% CI 0.8 to 1.4; annual fuel costs £705/557 versus £612/576, -65.3, 95% CI -31.9 to -98.7, adjusted). Barton 2007 reported little difference in change in temperature, humidity, or air quality between the intervention and control groups. There was less bedroom wall dampness among the intervention group (Int versus Cont -4 versus 0, P = 0.001). This was statistically significant.

Non-experimental studies (Overall Grade A and B) (n = 5)

Five of the better quality (Overall Grade A and B) non-experimental studies reported changes in housing condition (Braubach 2008; Hopton 1996; Platt 2007; Shortt 2007; Somerville 2000). Two of the studies had an Overall Grade A (Braubach 2008; Platt 2007). Platt 2007 reported increased warmth (OR 3.5, P < 0.01), reduced likelihood of heating less than half the house in cold weather (OR 0.22, 95% CI 0.16 to 0.29), fewer rooms not being used due to damp or cold (OR 0.39, P < 0.05). Three other studies reported a statistically significant improvement within the intervention group for both warmth and damp (Hopton 1996; Shortt 2007; Somerville 2000). In the two controlled studies no statistically significant improvements were reported among the control group for the same measures (Hopton 1996; Shortt 2007). A further study reported improvements in warmth and damp compared with the control group but no statistics were reported (Braubach 2008).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 4)

Warmth was reported to have improved in each of these uncontrolled studies and also other measures of housing condition were reported to have improved in these studies. There was no report of deterioration in housing condition. Residents' reports of this were presented but no statistics to test for statistical significance were reported (Allen 2005; Allen 2005a; Health Action Kirklees; Iversen 1986).

Socio-economic and equity impacts (n = 5)

Summary from better quality experimental and non-experimental studies (Overall Grade A and B) (n = 5)

Six of the better quality studies (Overall Grade A and B) reported a range of additional socio-economic impacts. Two RCTs from New Zealand reported statistically significantly lower levels of school absence among children related to receipt of the intervention (Howden-Chapman 2007; Howden-Chapman 2008). A non-experimental study also reported reduced days off school following the intervention (Somerville 2000). One of the New Zealand RCTs also reported statistically significant lower number of days off work among adults following the intervention (Howden-Chapman 2007). One study included an intervention to increase uptake of welfare benefits, and reported a statistically significant increase following the intervention Shortt 2007. None of the studies reported additional analyses relevant to equity issues. Changes in housing costs and affordability were reported alongside the housing impacts.

Experimental studies (Overall Grade A and B) (n = 2)

The two New Zealand RCTs reported socio-economic impacts following warmth improvements (Howden-Chapman 2007; Howden-Chapman 2008). Both studies reported small but statistically significantly lower levels of school absence among the intervention group (effect ratio 0.79, 95% CI 0.66 to 0.96, Howden-Chapman 2008; incident rate ratio 0.81, 95% CI 1.005 to 1.51, adjusted, Howden-Chapman 2007). A subgroup analysis reported a greater effect ratio for those whose pre-intervention heat source was an unflued gas heater (compared to an electric heat source) (effect ratio 0.72, 95% CI 0.55 to 0.93, Howden-Chapman 2008). Howden-Chapman 2007 also reported a statistically significantly lower rate for days off work among the intervention group (incident rate ratio 0.62, 95% CI 0.466 to 0.82, adjusted). This study also included an economic analysis which examined health service use, days off work or school, and fuel costs. The authors concluded that the costs of the benefits of housing improvement outweighed the cost of the intervention.

An RCT in the UK (CHARISMA 2011) also assessed days off school, indicating fewer 'all cause' and 'asthma' related absences in the intervention group. These differences were not statistically significant and also included the total sample of the RCT in which only 22% received an intervention relevant to this review.

Non-experimental studies (Overall Grade A and B) (n = 3)

Three non-experimental studies reported impacts on a range of additional socio-economic outcomes (Platt 2007; Shortt 2007; Somerville 2000). These outcomes varied and were not amenable to synthesis but are summarised below.

Platt 2007 reported an increased likelihood that the intervention group felt able to entertain friends and relatives in their home compared to the control group (friends and relatives dissuaded staying overnight due to poor housing conditions OR 0.42, 95%

CI 0.26 to 0.70; friends and relatives dissuaded from visiting due to poor housing conditions OR 0.4, 95% CI 0.23 to 0.70), and also reduced levels of financial difficulty compared to the control group (OR 0.77, 95% CI 0.6 to 0.99). In a study of children Somerville 2000 measured changes in days off school before and after the housing improvement. The study reported a statistically significant reduction in days off school due to asthma (7.27, 95% CI 3.32 to 11.21) but not for other causes (-1.8, 95% CI -3.86 to 0.26). This study also included an economic analysis which examined health service use and days lost from school as well as fuel costs. The authors concluded that the cost of benefits to the NHS outweighed the cost of actual housing improvement.

The study by Shortt 2007 included an intervention to increase benefit uptake; the mean number of welfare benefits received was statistically significantly higher among the intervention group compared to the control group following the housing and welfare intervention (Int versus Cont 0.02 versus 0.71, P < 0.001).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 0)

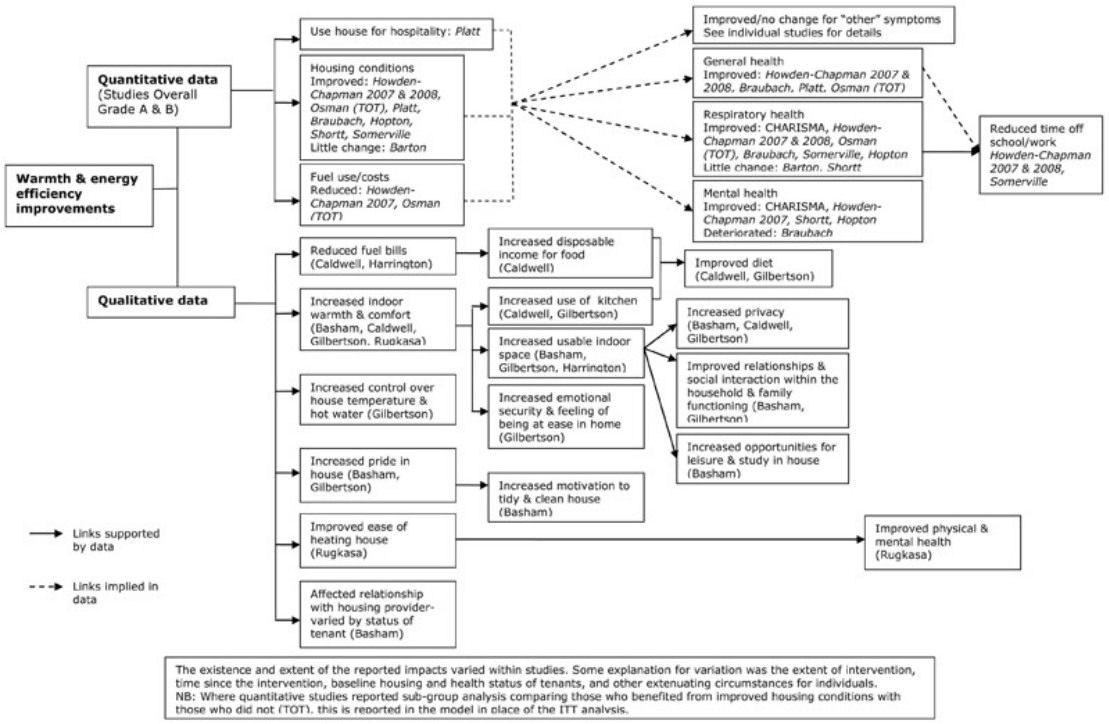
None of the Grade C studies reported socio-economic impacts.

Qualitative data (n = 7)

Seven studies of 'warmth and energy efficiency' improvements reporting qualitative data were identified (Allen 2005a; Basham 2004; Caldwell 2001; Decent Homes 2012; Gilbertson 2006; Harrington 2005; Rugkåsa 2004). All but one (Decent Homes 2012) of these studies were conducted in tandem with a quantitative study of health or health service impacts, the associated study is indicated in Table 3. Three of the quantitative studies were not eligible for inclusion in the review due to the absence of data reported for change in health impacts (Caldwell 2001; Heyman 2011; Warm Front 2008). Following assessment of study quality, largely reporting and appropriateness of methods (see Table 2), two studies (Allen 2005a; Decent Homes 2012) were excluded from the review of qualitative data.

A logic model mapping out the reported impacts and links to impacts was developed independently by two review authors (HT and ST) and a final agreed version was then prepared (Figure 8); a summary of the quantitative findings from the better quality studies (Overall Grade A and B) was included in the model. A range of impacts were reported to be directly as a result of the 'warmth and energy' improvement, such as reduced fuel bills in some studies, as well as other less obviously related impacts such as increased pride in house, changed relationship with housing provider, and increased control over house temperature. Data from three studies (Basham 2004; Caldwell 2001; Harrington 2005) indicated that improved warmth led to increased usable indoor space and this was linked to subsequent improvements in privacy in the home, relationships within the household; and residents in one study reported having greater opportunity to study (Basham 2004). Residents in two studies reported improved diet (Caldwell 2001; Gilbertson 2006). In one study this was linked to increased income due to reduced spending on fuel (Caldwell 2001), and increased use of the kitchen which was linked to increased thermal comfort (Caldwell 2001).

Figure 8. Logic model mapping impact types and direction, and links to health impacts reported in qualitative and quantitative studies of warmth and energy efficiency improvements.



Rehousing or retrofitting ± neighbourhood renewal (post-1995), n = 14 (quantitative: 12 + qualitative: 4)

Fourteen studies of rehousing or retrofitting were included in the review (Ambrose 2000; Barnes 2003; Blackman 2001; Breysse 2011; Bullen et al 2008; Critchley 2004; Ellaway 2000; Evans 2000; Halpern 1995; Kearns 2008; Molnar 2010; Thomas 2005; Thomson 2007; Wells 2000). Four of the studies reported qualitative data (Bullen et al 2008; Ellaway 2000; Kearns 2008;Thomas 2005). Three of these studies were supplementary to a quantitative evaluation (Jackson 2011; Kearns 2008; Thomas 2005), one of the quantitative evaluations was excluded as it only assessed changes in health service use (Jackson 2011, see Bullen et al 2008), which was not considered to be a key outcome for this review. None of the rehousing studies used an experimental design. Six of the quantitative studies used a CBA design (Barnes 2003; Critchley 2004; Evans 2000; Kearns 2008; Thomas 2005; Thomson 2007), four used a UBA design (Ambrose 2000; Blackman 2001; Molnar 2010; Wells 2000). One study reported resident reports of changes in health following the intervention and was a retrospecitve uncontrolled study (Breysse 2011). Halpern 1995 reported collecting data for a control group and following a cohort of participants before and after the intervention, but only area level cross-sectional data for the intervention area were reported so this study was labelled as an XUBA. Six of the studies were assessed to have an Overall Grade A and B (Barnes 2003; Critchley 2004; Kearns 2008; Thomas 2005; Thomson 2007 Evans 2000). Impacts were reported between six months and five years after the intervention. Among the better quality studies (Overall Grade A and B) the follow-up period was narrower, between six and 24 months.

A summary table of the included studies, their study design, and assessment of study quality is provided below (Table D). Further details of the study characteristics and reported data are provided in Table 9 (see also Table 10; Figure 4; Appendix 2).

Table D. Summary of characteristics of quantitative rehousing studies

Author, Publication year, Country	Study design	Final sample Int/ Cont; Population	Time since intervention	Summary of Study Quality					
				Selection	Confounding	Withdrawals	**Overall grade (HAT)**	Performance	No. of items at low Risk of Bias
Rehousing or retrofitting with or without neighbourhood renewal (after 1995)									
Kearns 2008 UK	CBA	262/284 Adults and Children	24 months	C	C	B	**A**	C	1
Thomson 2007 UK	CBA	50/50 General adult population	12 months	C	B	B	**A**	B	3
Critchley 2004 UK	CBA	246 Elderly population	1-12 months	C	B	B	**A**	B	1
Thomas 2005 UK	CBA	585/759 General adult population	22 months	C	B	C	**B**	C	1
Barnes 2003 UK	CBA	45/45 General adult population	18 months	A	C	C	**B**	C	1
Evans 2000 UK	CBA	17/17 General adult population	6-18 months	C	B	C	**B**	C	0
Breysse 2011 USA	RU	24 Adults and 17 Children	12-18 months	C	C	C	**C**	C	0
Molnar 2010 Hungary	UBA	19 Adults and 42 Children in Roma community	5 years	C	C	B	**C**	C	0

(*Continued*)

Blackman 2001 UK	UBA	166 Adults and Children	5 years	B	C	C	**C**	C	1
Wells 2000 USA	UBA	31 General adult population	2-3 years	C	C	B	**C**	B	1
Ambrose 2000 UK	UBA	227 General adult population	4 years	A	C	C	**C**	C	1
Halpern 1995 UK	XUBA	27 General adult population	10 months	C	C	C	**C**	C	0

Rehousing or retrofitting ± neighbourhood renewal: context, population, intervention

Context and population

All but four of the studies of rehousing were conducted in the UK. The studies from the UK were all investigating health impacts of area based interventions, that is delivered across an area rather than to selected individuals within an area. The study samples were all living in socio-economically deprived neighbourhoods and six of the studies included only those in social housing (Ambrose 2000; Barnes 2003; Blackman 2001; Critchley 2004; Ellaway 2000; Evans 2000; Halpern 1995; Jackson 2011; Thomas 2005; Thomson 2007; Wells 2000); one study had a mix of social and private housing (Blackman 2001); another study included a mix of private and social housing tenants but the intervention involved moving to new social housing (Kearns 2008); and the remaining study included only private householders living in a deprived neighbourhood (Evans 2000). Breysse 2011 and Wells 2000 were conducted in the USA and assessed retrofitting to meet 'green' standards (Breysse 2011) and a participatory intervention targeted at families on low incomes on the fringes of home ownership (Wells 2000). The Molnar 2010 study of a Roma community was conducted in Hungary. One of the qualitative studies was conducted in New Zealand (Bullen et al 2008) and was delivered and tailored to eligible individuals within a locality. All the studies primarily reported health outcomes for adults, but three studies also reported some health impacts among children (Blackman 2001; Kearns 2008; Molnar 2010). None of the studies reported specifically targeting those with poor health, but by targeting residents in low income areas of socio-economic deprivation it was likely that the prevalence of poor health would be higher than in other areas.

Rehousing or retrofitting ± neighbourhood renewal interventions

Ten studies were conducted in the UK. The interventions under study were similar being government investment to improve housing conditions in deprived areas, predominantly among social housing. It was likely that most of these interventions would have included warmth and energy efficiency measures such as repair, upgrade, or installation of central heating but this was not clearly reported and it was likely that this would vary considerably across a study population. Most of the interventions included wider neighbourhood changes both to the physical environment and most likely wider socio-economic regeneration activities. In four studies it was not clear if associated wider investment was also being undertaken as part of the housing improvement (Barnes 2003; Critchley 2004; Evans 2000; Kearns 2008). In Kearns 2008 the rehousing involved a change in tenure for 27% of the intervention group, moving from private rented housing to social rented housing. In addition many (around 30%) of the intervention group moved from a flat to a house with a private garden.

One study from the USA involved multiple housing improvement components to meet environmental standards. This included insulation, ventilation, water use, radon mitigation, as well as neighbourhood improvements (Breysse 2011). The other study from the USA involved renovation and extension of existing homes for a selected group of families, the housing investment was dependent on residents contributing labour hours to house building and renovation (around 400 hours per family) (Wells 2000). The study of a Roma community involved providing upgraded or new housing to those considered to be living in 'life threatening' housing conditions (Molnar 2010). One qualitative study from New Zealand (Bullen et al 2008) was a wide ranging programme of housing improvement which was tailored according to household need. The major improvements included insulation, ventilation, and extensions for large families. This programme also provided housing and health advice and aimed to improve links between householders with health and other support agencies.

General health impacts (n = 7)

Non-experimental studies (Overall Grade A and B) (n = 5)

Five non-experimental studies with an Overall Grade of A and B assessed changes in general health outcomes (Barnes 2003; Critchley 2004; Evans 2000; Kearns 2008; Thomson 2007). There would appear to have been an overall positive impact on general health outcomes, but not all studies reported improvements.

Three studies reported similar outcomes, 'self-reported health' and 'health worse since one year ago', which were amenable to calculation of a standardized effect estimate (Barnes 2003; Kearns 2008; Thomson 2007) and statistical synthesis. Statistical heterogeneity was high (I^2 = 78%, Chi^2 = 8.93, P = 0.01) for 'self-reported health' and a meta-analysis was not performed; the effect sizes reported lower levels of poor health in the intervention group compared with the control group in two studies (Barnes 2003; Kearns 2008) but not in the third study (Thomson 2007); only one of these findings, from a small study where the reported data were estimated, was statistically significant (Barnes 2003) (OR ~0.27, 95% CI 0.11 to 0.68 (Barnes 2003); OR 0.769, 95% CI 0.50 to 1.176, adjusted (Kearns 2008); OR 1.75795%, CI 0.777 to 3.97 (Thomson 2007)). Data from two studies (Barnes 2003; Kearns 2008) on 'health worse since one year ago' were statistically synthesized. The overall effect estimate was OR 0.60 (95% CI 0.29 to 1.26, tau^2 = 0.17, I^2 = 55%, Chi^2 P = 0.13) and was not statistically significant. Kearns 2008 also reported improved levels of long standing illness (OR 0.68, 95% CI 0.44 to 1.05, adjusted) and increased change in the SF-36 physical functioning score (+0.39/-0.55, P = 0.36). Neither of these differences or changes were statistically significant. In a subgroup analysis comparing those who reported improvement in dwelling fabric with those who did not, there was a higher proportion of increased SF-36 physical functioning scores among those who had experienced improvements in dwelling fabric compared with those who had not, this difference was statistically significant (45.4%/31.0%, P = 0.024) (Kearns 2008). Thomson 2007 reported a small difference in the SF-36 Physical Component Score (PCS) following the rehousing. The SF-36 PCS was lower in the intervention group than the control group but this difference was not statistically significant (OR 0.96, 95% CI 0.437 to 2.11). Critchley 2004 and Evans 2000 also reported changes in the SF-36. Critchley 2004 reported changes in the SF-36 general health measure (estimated from graph Men Int -3/-0.5 Cont 0/-8; Women Int +0.5/+4 Cont -1.5/-1), the authors reported no statistically significant changes at the 95% level. In a subgroup analysis comparing those with documented improved energy efficient housing with those where energy efficiency was not improved, the greatest improvement in SF-36 domains was seen for occupants of houses where energy efficiency had changed from 'low' to 'high' following the investment (Critchley 2004). Evans 2000 reported an increase in the median among the intervention group but not in the control group (SF-36 general health 50 versus 57/56 versus 50), and a reduction in the median of the SF-36 'physical function' domain for both the intervention and the control groups (65 versus 35/60 versus 59); no statistics were reported. Barnes 2003 reported an additional general health measure, reporting a lower level of 'health problems affecting daily activities' in the intervention group compared to the control group, but the difference was not statistically significant (OR ~0.52, 95% CI 0.62 to 3.73). Barnes 2003 reported some additional analyses comparing those who had received central heating with those who had not but the data were unclear and not amenable to extraction.

Studies assessed to have a low overall study quality (Overall Grade C) (n = 2)

One UBA study and one retrospective uncontrolled (RU) study of housing-led neighbourhood renewal which reported general health impacts were assessed as having a low Overall Grade C (Blackman 2001). Blackman 2001 reported a statistically significant increase in self-reported poor health among adults (9.7% versus 22.0%, P < 0.01) but an improvement among children (2.3% versus 0.0%, ns). In the Breysse 2011 study, there were mixed reports about changes in general health 12 to 18 months since the intervention, with most reports indicating no change (health better, same, worse: adults 5, 9, 4, P = 0.786; children 5, 8, 2, P = 0.358).

Respiratory health impacts (n = 4)

Non-experimental studies (Overall Grade A and B) (n = 1)

One CBA (Overall Grade A) assessed changes in respiratory impacts. Kearns 2008 reported a small and non-statistically significantly higher level of wheezing (OR 1.04, 95% CI 0.69 to 1.56, adjusted) among adults and a mix of positive and negative differences between the intervention and the control groups for six measures among children, these differences were not statistically significant (asthma OR 1.039, 95% CI 0.65 to 1.66; breathlessness OR 1.185, 95% CI 0.459 to 3.06; persistent cough OR 1.09, 95% CI 0.66 to 1.80; bronchitis OR 0.31, 95% CI 0.03 to 3.01; sinus and catarrh OR 0.89, 95%CI 0.48 to 1.65; hay fever OR 0.99, 95% CI 0.51 to 1.91).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 3)

Two UBA studies reported respiratory outcomes (Ambrose 2000; Blackman 2001). Ambrose 2000 reported mixed findings, increased cough or colds and reduced asthma or bronchial symptoms after the intervention; both these findings were statistically significant (cough and cold 41.9% versus 66.7%, P < 0.001; asthmatic and bronchial 17.0% versus 5.7%, P < 0.001). Blackman 2001 reported increases in both acute (13.3% versus 17.5%, ns) and chronic respiratory conditions (chronic 31.9% versus 44.0%, P < 0.05) following the intervention among adults. Among children Blackman 2001 reported a reduction in reported acute respiratory illness (25.6% versus 20.9%, ns) and a small increase in chronic respiratory conditions (23.3% versus 25.6%, ns). These changes were not statistically significant. A RU study from the US reported recalled changes in respiratory measures 12 to 18 months since the intervention. There were no statistically significant reported changes for asthma (adults/children -4%, P = 0.317/0%). Non-asthma respiratory symptoms were reported by participants to be less following the intervention, this change was statistically significant among adults but not children (adults/children -23%, P = 0.025/-15%, P = 0.317).

Mental health impacts (n = 9)

Non-experimental studies (Overall Grade A and B) (n = 5)

The findings from these five studies were diverse, with a mix of studies reporting negative, positive, or no change in mental health. Two studies from Scotland (UK) reported SF-36 mental health measures. In Thomson 2007 the intervention group were less likely to report a deterioration in mental health, measured by the SF-36 Mental Component Score, following the housing improvement suggesting benefits for the intervention group (OR 0.733, 95% CI 0.333 to 1.613); this difference was not statistically significant. In Kearns 2008, although there were improvements in the four SF-36 mental health domains assessed, none of these changes were statistically significant for the difference in change between the intervention and control groups (Int v Cont- mental health +1.1 versus +2.1, P = 0.36; vitality +0.1 versus +0.3, P = 0.87; social functioning +0.9 versus +1.5, P missing; role-emotional +1.3 versus +1.2, P = 0.94). In a subgroup analysis comparing those with 'some improvement' with those with 'no improvement' the differences between the two groups were statistically significant for each measure, suggesting a benefit to mental health following the housing improvement (mental health 62.5%/44.9%, P < 0.000; vitality 65.0%/32.6%, P < 0.000; social functioning 42.5%/31.8%, P < 0.000; role-emotional 50.0%/31.6%, P < 0.000) (Kearns 2008). Critchley 2004 also reported changes in the SF-36 measure of mental health and there were some improvements for women but not men in the intervention group, the changes were not statistically significant (Men Int -2/0 Cont 0/-1 Women Int +0.5/+4.5 Cont -1/-1.5). In a subgroup analysis there was no clear link between changes in vitality (SF-36) and documented improvements in energy efficient housing. Thomas 2005 reported an increase in GHQ caseness (caseness indicates poor mental health) in both the intervention and the control groups and for both men and women (Male/Female 18.8% versus 35%/22.3% versus 33%). However, there was a high level of contamination with regard to housing improvements with 66% of the intervention group and 55% of the control group receiving the intervention. Subgroup analysis compared those who had received housing improvements with those from the intervention and control areas who had not; the changes in mental health in the two groups were not statistically significant (change in median GHQ +0.053, P = 0.904 and for 'no housing improvement' +0.092, P = 0.535). Barnes 2003 reported a reduced level of self-reported anxiety of depression among the intervention group compared with the control group following the housing improvement (OR 0.361, 95% CI 0.152 to 0.856), this difference was statistically significant; 'optimism for the future' was also increased in the intervention group.

Studies assessed to have a low overall study quality (Overall Grade C) (n = 4)

Three of the poorer quality studies (Overall Grade C) reported mental health impacts (Ambrose 2000; Blackman 2001; Wells 2000). In the two UK studies Ambrose 2000 and Blackman 2001 reported increased 'stress or depression' or 'mental health issues' among the intervention group following the housing improvement (6.1% versus 1.2%, P < 0.01 (Ambrose 2000); 52.4% versus 41.0%, P < 0.05 (Blackman 2001)). Blackman 2001 also reported an increase in mental health problems among children in the intervention group (20.9% versus 2.3%, P < 0.05). One XUBA study reported reductions in the HADS scores 10 months after the housing improvement. There was a statistically significant reduction in both anxiety and depression following the intervention (proportion of anxiety cases (score 8+) 57.1% versus 22.6%, P = 0.008; proportion depression cases (score 8+) 25.0% versus 3.7%, P = 0.025) (Halpern 1995).

In the USA study by Wells 2000 there was a statistically significant

improvement in mental health reported following the intervention (31.00 versus 22.26, P < 0.001).

Other illness and symptom impacts (n = 5)

Non-experimental studies (Overall Grade A and B) (n = 2)

Two better quality (Overall Grade A and B) non-experimental studies reported additional illness or symptom outcomes (Barnes 2003; Kearns 2008) but the overall benefit was unclear.

Kearns 2008 reported a reduction in the mean number of symptoms among adults for both the intervention and the control group with a slightly smaller reduction in the intervention group (-0.3/-0.4, P = 0.61), the difference in change was not statistically significant between the two groups. There was also a reduced likelihood of accidents among the intervention group but this was not statistically significant (OR 0.92, 95% CI 0.57 to 1.49). Following the intervention the intervention and control groups were compared with respect to three health behaviours; the intervention group were more likely to be smokers (OR 1.47, 95% CI 0.85 to 2.55), and eat five portions of fruit and vegetables per day (OR 1.26, 95% CI 0.82 to 1.92), but were less likely to be heavy drinkers (OR 0.61, 95% CI 0.30 to 1.24); none of these differences were statistically significant. A greater proportion of the intervention group had walked in their local neighbourhood recently (53.8% versus 41.2%). Barnes 2003 reported that the intervention group were more likely than the control group to report 'physical and emotional problems not interfered with normal daily activities in past month' (OR 1.516, 95% CI 0.617 to 3.73), 'mobility problems' (OR ~0.53, 95% CI 0.22 to 1.32), and 'pain and discomfort' (OR~0.40, 95% 0.17 to 0.94), only the pain item reported statistically significant differences. There were also reports among the intervention group of reduced problems with 'self-care' (8% versus 17%, ns) and 'usual activities' (22% versus 42%, P < 0.05) when compared with the control group.

Kearns 2008 reported some outcomes among children. There were reports of slightly higher eczema (OR 1.148, 95% CI 0.68 to 1.93), chronic illness (OR 1.039, 95% CI 0.549 to 1.966), and 'not sleeping' (OR 1.128, 95% CI 0.618 to 2.059) among the intervention group. None of these differences were statistically significant. There was no difference in reports of headaches (OR 0.99, 95% CI 0.60 to 1.626) or indigestion (OR 0.94, 95% CI 0.058 to 15.145) between the two groups.

Studies assessed to have a low overall study quality (Overall Grade C) (n = 3)

Three of the poorer quality studies (Overall Grade C) reported impacts on illness and other symptoms. Breysse 2011 reported residents' recollections of changes in injuries 12 to 18 months after retrofitting. No changes were reported among adults. An increase in injuries was reported for children (+18%), which was not statistically significant. Molnar 2010 reported changes among nine households making it difficult to assess statistical significance. Five years after the intervention there was an increase in the number of adults reporting hypertension (2 versus 4) but no change in the numbers of adults reporting 'functional limitation', thrombosis, varicositas (1 versus 1, 1 versus 1 respectively) or children with epilepsy, brain tumour, or spinal hernia (2 versus 2, 1 versus 1, 2 versus 2 respectively). There were families with children with scabies, louse or impetigo (3 versus 2) following rehousing. Ambrose 2000 reported fewer illnesses but more 'illness days' per person in the intervention group compared to the control group suggesting fewer illnesses but longer episodes among the intervention group (number of illness episodes/day 0.0036 versus 0.0056; illness days per person 0.37 versus 0.05). Reports of 'aches and pains' were higher in the intervention group (22.6% versus 11.5%, P < 0.001); this difference was statistically significant. Reports of 'dietary and digestive' problems were lower in the intervention group following the housing improvement (12.4% versus 14.9%).

Housing conditions and neighbourhood impacts (n = 12)

Non-experimental studies (Overall Grade A and B) (n = 6)

Three of the better quality studies reported an overall improvement in the housing indicators assessed (Critchley 2004; Kearns 2008; Thomson 2007), and in three studies the extent of improvements among the intervention group was less clear (Barnes 2003; Evans 2000; Thomas 2005). In one study 66% of the intervention group and 55% of the control group received housing improvements (Thomas 2005). In the other studies contamination was not clearly confirmed or eliminated. There were reports in three studies of improved neighbourhood measures (Barnes 2003; Kearns 2008; Thomson 2007); in one study the effect on neighbourhood measures was unclear (Critchley 2004).

Residents in the Thomson 2007 study reported improvements in dampness (Int/Cont +24%/+2%, 95% CI 8.82 to 35.18), draughts (Int/Cont+28%/+10%, 95% CI 2.62 to 33.38), and heating system (Int/Cont +22%/+4%, 95% CI 4.82 to 31.18) which were statistically significantly greater than the change in the control group. Changes in ability to 'keep warm in winter' (Int/Cont +20%/+6%, 95% CI 0.82 to 27.18) and 'other housing problems' (Int/Cont 10%/+12%, 95% CI -10.27 to 14.27) suggested some improvement but the difference in change was not statistically significant when compared with the control group. A greater increase in the rent was reported for the intervention group compared to the control group (mean change in weekly rent (n = 33) Int/Cont +£6.65/+£1.31), however the dataset was small and over half of the residents did not pay rent, being dependent on welfare provision. In the Kearns 2008 study, a large number of the intervention group changed tenure from private renting to

social renting, and many moved from flats to houses with a private garden. The study reported changes for nine measures of housing condition, including damp, space, and privacy. These problems were all reduced following the intervention but seven were reduced in the control group as well. A comparison of the mean number of housing problems suggested a statistically significant overall improvement for the intervention compared with the control group (7.10 versus 2.90, P < 0.001/4.30 versus 3.88, ns). In addition, Kearns 2008 reported a large reduction among the intervention group with respect to 'difficulties paying rent or mortgage' (-14.3%) and 'difficulties paying utility bills' (-18.7%).

Changes in housing conditions were less clear in three of the better quality studies (Barnes 2003; Critchley 2004; Evans 2000). Mean energy efficiency ratings (SAP or Standard Assessment Procedure for energy efficiency) were reported to have increased indicating an improvement in some of the intervention group; this increase was only statistically significant in one of the intervention areas. Critchley 2004 also reported housing satisfaction among the intervention group (+22% and +39%). Barnes 2003 also reported higher levels of housing satisfaction among the intervention group compared with the intervention group but this difference was not statistically significant (82% versus 70%, ns). The data suggested that the intervention group did experience improvements in warmth in the home. The mean temperature increased more in the intervention group than in the control group (living room Int versus Cont +4.7 °C versus +0.1 °C; bedroom +6.0 °C versus +0.0 °C). Reports of 'affordable heating' increased in both the intervention and the control group following the intervention but fuel costs were similar for both groups before and after the intervention. Evans 2000 reported a small drop in temperature among the intervention group but a similar rise in temperature among the control group (Int versus Cont -0.1 °C versus +0.14 °C).

Barnes 2003, Critchley 2004, Kearns 2008 and Thomson 2007 also reported changes in wider neighbourhood measures. Thomson 2007 reported a reduction in the mean 'number of neighbourhood problems' in the intervention group but an increase in the control group, neither of these differences were statistically significant (Int -1.02, 95% CI -0.231 to 2.271; Cont +0.14, 95% CI -1.148 to 0.868). Kearns 2008 reported increased satisfaction with the neighbourhood (+13.5%) and landlord (+19.6%) among the intervention group. Barnes 2003 reported higher levels of 'neighbourhood satisfaction' (82% versus 77%, ns), more 'fear of crime' (61% versus 57%, ns), more 'feeling safe outside the home' (79% versus 67%, ns), and similar levels of 'feeling safe in the home' (80% versus 81%, ns). These differences were not statistically significant. Critchley 2004 reported improvement in one measure of neighbourhood but not in another among the intervention group, and the opposite reports among the control group ('feeling very safe in neighbourhood' Int/Cont -2%/+7%; 'neighbours likely to help each other' Int/Cont +26%/-5%).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 6)

Twelve to 18 months after the intervention, Breysse 2011 reported reduced radon levels (before versus after 3.1 versus 0.7 pCi/litre) and reduced energy consumption (before versus after 9.76 versus 5.05 British Thermal Units per heating degree days/square foot/year). Residents reported improvements in a range of housing conditions but these were not typically statistically significant (water dampness -26%, P = 0.102; musty smell -25%, P = 0.046; dehumidifier use -25%, P = 0.046; humidifier use +7%, P = 0.157; cockroaches -12%, P = 0.414; mice and rats -25%, P = 0.046; insecticides -19%, P = 0.083; smoke inside home -13%, P = 0.157; clean > 1 time per week +31%, P = 0.025). Changes in housing conditions reported by Molnar 2010 were mixed. The whole study sample were rehoused or benefited from some housing improvement. The number of families living in houses with electricity and running water increased (before versus after 90% versus 100%, and 0% versus 66%, respectively), and levels of overcrowding reduced following rehousing (families with > 3 people per room 50% versus 17%). Housing costs were reported to have increased (mean 10 to 30 euros versus 50 to 70 euros) and three families reported 'better to stay in previous dwelling'. Ambrose 2000 reported statistically significant improvements in all the nine measures of housing condition reported following the improvements (self-reported damp -34.2%, P < 0.001; heating keeps everyone warm +37.2%, P < 0.001; heating not used due to cost -23%, P < 0.001; infestation 11.6%, P < 0.01; repairs needed -32.9%, P < 0.001; very or fairly satisfied with house +41.4%, P<0.001; repairs needed -32.9%, P < 0.001; heating not used due to cost -1.0%, P < 0.01; feel quite safe in home +25.3%, P < 0.001). In addition, there were statistically significant improvements in the measures of neighbourhood reported (very or fairly satisfied with estate +32.1%, P < 0.001; know people nearby 'quite well or very well' +15.4%, P < 0.001; belong to community 'very much' +13.1%, P < 0.001). A small amount of data on housing costs were sourced directly from utility suppliers. The data indicated an increase in housing costs following the intervention (change in weekly housing costs: rent (n = 19) +31.4% (£18.97), water (n = 19) +£1.56, gas (n = 9) -£2.13, electricity (n = 6) -£1.43; mean change in overall housing costs for subgroup (n = 20) +26.8%).

Blackman 2001 reported improvements in each of the housing condition measures reported but these were not all statistically significant (dwelling has no draughts 50.0% versus 73.5%, P < 0.05; dwelling has draughts that affect health 11.2% versus 6.1%, ns; dwelling has no damp 76.0% versus 85.7%, ns; dwelling has damp that affects health 3.1% versus 4.1% ns; unable to always keep warm last winter 15.4% versus 14.3%, ns; happy with present home 85.7% versus 84.7%, ns). There were additional questions about the local area which indicated some improvements but the differences were not statistically significant. Halpern 1995 reported changes in neighbourliness and feelings about the neighbourhood but the sample size was unclear and this was a subgroup of the whole sample which contained 27 people. The reported

data indicated improvements in neighbourliness and views of the neighbourhood.

The study from the USA (Wells 2000) reported statistically significant improvements in a number of housing indicators following the intervention (crowding 1.39 versus 2.24, P < 0.001; indoor climate 1.79 versus 2.30, P < 0.001; cleanliness 1.41 versus 1.79, P < 0.001; structural quality 2.79 versus 3.00, P < 0.001; hazards 1.29 versus 1.46, P < 0.05; overall housing quality 1.73 versus 2.14, P < 0.001).

Socio-economic and equity impacts (n = 5)

Changes in housing costs and affordability were reported alongside the housing impacts.

Non-experimental studies (Overall Grade A and B) (n = 3)

Two CBA studies assessed to have an Overall Grade A reported socio-economic impacts.

Kearns 2008 reported a range of 20 additional measures which included privacy, control, empowerment, identity, networks, belonging, and neighbourliness. Most of the measures indicated a small improvement following the intervention and some of the differences were statistically significant. There was little indication of negative impacts. Time off school was also assessed. Among the intervention group the level of school absence was higher than the control group but this difference was not statistically significant (> 4 days off school in past month Int versus Cont 18 (26.9%) versus 13 (20.3%), P = 0.378). Critchley 2004 reported a similar reduction in the intervention and the control groups for those 'unable to afford basic essentials' (-18.8% versus -18.5%).

Critchley 2004 reported changes in the SF-36 general health measure and mental health by gender (General health: Men Int-3/-0.5 Cont 0/-8; Women Int +0.5/+4 Cont-1.5/-1; Mental health: Men Int -2/0 Cont 0/-1 Women Int +0.5/+4.5 Cont -1/-1.5). There appeared to be improvement among women in the intervention group but not among men; the authors reported that none of the changes were statistically significant. Thomas 2005 reported a greater increase in poor mental health among men compared to women (GHQ caseness Men/Women +16.2%/+10.7%).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 2)

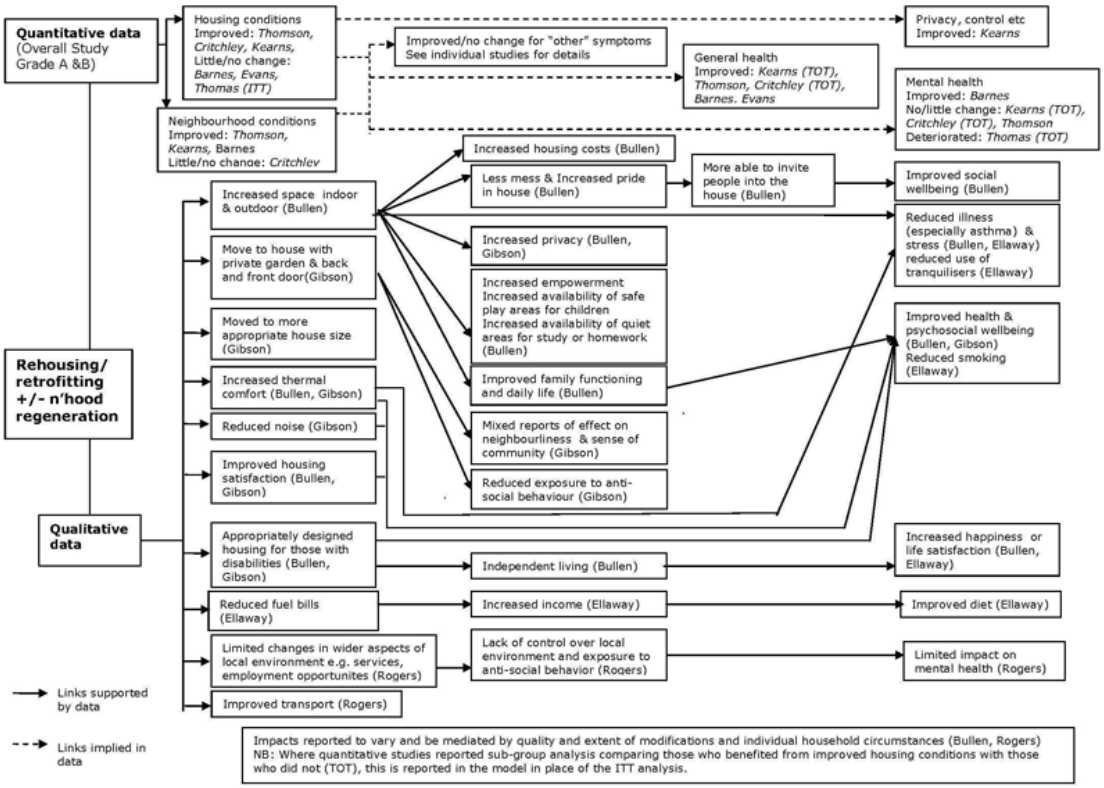
Molnar 2010 reported some improvements in education (adults with less than 8 years schooling before versus after 67% versus 60%) and employment (have permanent job before versus after 4 versus 3), as well as other data on changes in social networks and income. However, the numbers were small and the data presented were difficult to interpret, in particular for changes in social networks and income. Ambrose 2000 reported some change in socio-economic outcomes in a subsample but the data were not clear with respect to sample selection or sample size. There were reports of a small reduction in those in full-time employment (Bef/Aft 10.5%/9.7%) and an increase in those receiving income support (Bef/Aft 65.4%/76.0%). Five other measures of life in the local area appeared to improve following the intervention (Bef/Aft: 'feel quite safe in home' 46.7%/72.0%, 'local criminal activity very serious/fairly serious' 72.0%/46.0%, 'very/quite satisfied with children's school' 49.5%/68.0%, 'know people nearby quite well/very well' 76.6%/92.0%, 'belong to community very much' 44.9%/58.0%).

Qualitative data (n = 5)

Five studies of rehousing and retrofitting improvements reporting qualitative data were identified (Bullen et al 2008; Ellaway 2000; Gibson 2010; Kearns 2006; Rogers et al 2008). Four of these studies were conducted in tandem with a quantitative study of health or health service impacts. The associated study is indicated in Table 3. One of the quantitative studies was not eligible for inclusion in the review due to the absence of data on direct health impacts (Jackson 2011). Following assessment of study quality, largely reporting and appropriateness of methods (Table 2), one study (Kearns 2006) was not included in the review of qualitative data.

A logic model mapping out the reported impacts and links to impacts was developed independently by two review authors (HT and ST), and a final agreed version was then prepared (Figure 9); a summary of the quantitative findings from the better quality studies (Overall Grade A and B) was included in the model. A range of health or health related impacts were reported in three studies (Bullen et al 2008; Ellaway 2000; Rogers et al 2008): improved wellbeing happiness and life satisfaction, reduced respiratory illness and stress, reduced smoking and tranquilliser use, and improved diet. Improved levels of thermal comfort, reduced noise, and general improved housing satisfaction were linked by residents to improved health and well-being. There were a number of other impacts reported in single studies. For example, in one New Zealand study increased space was reported to be linked to seven intermediate outcomes: increased privacy, empowerment, reduced clutter, improved family functioning, and more safe space for children to play. There was also some reporting of increased space leading to increased bills. Improved levels of thermal comfort, reduced noise, and general improved housing satisfaction were linked by residents to improved health and well-being. Overall there was very little evidence of negative impacts reported.

Figure 9. Logic model mapping impact types and direction, and links to health impacts reported in qualitative and quantitative studies of rehousing/retrofitting ± neighbourhood renewal.



Provision of basic housing in low or middle income country (post-1990), n = 3 (quantitative: 3)

Three quantitative studies of basic housing provision or low or middle income country (LMIC) housing improvement interventions were included in the review (Aziz 1990; Rojas de Arias 1999; Spiegel 2003). Each of these studies was from an LMIC. None of the studies used an experimental design, one used a CBA design and was assessed to be Grade B (overall study quality) (Rojas de Arias 1999). The other two studies were assessed as poor quality (Overall Grade C). No qualitative studies were reported. Two studies used an XCBA design to assess the effects of an area based programme to improve living conditions. Although it cannot be certain that the population changed between baseline and follow-up, there was no indication of this and socio-demographic data for the neighbourhoods was largely unchanged at follow-up. Impacts were assessed three to 36 months (Rojas de Arias 1999), one to four (Spiegel 2003), and two to three years (Aziz 1990) after the intervention. Aziz 1990 also reported a further wave of data collection nine years after the intervention but this was not the main wave for the initial evaluation. Table E provides a summary of the study quality and other important study characteristics. For more details of the studies and the reported impacts see Table 9, Table 10, Figure 4, and Appendix 2.

Table E. Summary of characteristics of quantitative studies of provision of basic housing improvements in low and middle income countries

Author, Publication year, Country	Study design	Final sample Int/Cont; Population	Time since intervention	Summary of Study Quality					
				Selection	Confounding	Withdrawals	**Overall grade (HAT)**	Performance	No. of items at low Risk of Bias
Provision of basic housing needs/low or middle income country intervention									
Rojas de Arias 1999 Paraguay	CBA (3 intervention groups)	229/260/132 General adult population	3-36 months	C	C	A	**B**	B	0
Spiegel 2003 Cuba	XCBA	896/807 General adult population	1-4 years	C	C	C	**C**	C	0
Aziz 1990 Bangladesh	XCBA	>200/200 Children only	2-3 years	C	B	C	**C**	B	2

Provision of basic housing LMIC: context, population, intervention

Context and population

This group of studies varied considerably with respect to context, population, and intervention. One of the LMIC studies was conducted in a rural part of Paraguay (Rojas de Arias 1999), one in a deprived and dilapidated urban neighbourhood in Cuba (Spiegel 2003), and the other in a rural Bangladesh village (Aziz 1990). In the Bangladesh study the population were predominantly Muslim and there was a high level of illiteracy, this study investigated the impacts on children.

Provision of basic housing LMIC interventions

One of these studies assessed the effectiveness of measures to reduce Chagas disease by reducing exposure to the disease vectors (triatomines) (Rojas de Arias 1999). The intervention included two components: application of insecticide, and housing improvement to ensure smooth crack free surfaces in existing housing. In the Aziz 1990 study the intervention was to provide sealed double pit-water latrines to household and communal water hand pumps to a village where provision was low. In the Cuban study (Spiegel 2003) the intervention included repairs to housing as well as wider neighbourhood improvements to water and sanitation infrastructure, street lighting, and repair of public buildings. Other leisure activities and a club for the local community were also initiated.

General health impacts (n = 1)

Studies assessed to have a low overall study quality (Overall Grade C) (n = 1)

One LMIC study assessed general health impacts (Spiegel 2003). This was a XCBA conducted in Cuba. Statistically significant improvements in self-reported health were reported among men in the intervention group but not among women, one to four years after the intervention.

Respiratory health impacts (n = 0)

None of the LMIC studies assessed respiratory health impacts.

Mental health impacts (n = 0)

None of the LMIC studies assessed mental health impacts.

Other illness and symptom impacts (n = 2)

Non-experimental studies (Overall Grade B) (n = 1)

The proportion of participants who were sero-positive for triatotmine was reduced in each of the three intervention groups (Rojas de Arias 1999). The reduction was statistically significant in one group (insecticide only) (before versus after % triatomine serology Int A/B/A+B 28.5 versus 17.4, P = 0.02/14.0 versus 12.7, P = 0.67/19.4 versus 16.9, P = 0.39).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 1)

One study had an illness measure as its key outcome of interest, examining impacts on childhood diarrhoea following provision of latrines in a rural community in Bangladesh (Aziz 1990). Three years after the intervention the incidence of diarrhoea and dysentery had decreased in the intervention group but increased in the control group (change incidence of all diarrhoea episodes per child per year Int -1.02, 95% CI -0.96 to -1.09 versus Cont +0.75, 95% CI 0.70 to 0.80; incidence of dysentery Int -1.16, 95% CI -1.0 to -1.34 versus Cont +0.73, 95% CI 0.61 to 0.88). Data were also reported for children under five years of age, divided into five age groups. There were statistically significantly greater reductions in diarrhoea among children between 6 to 11, 12 to 23, 24 to 35, and 36 to 59 months but not for 0 to 5 months. Among the intervention group a statistically significantly lower number of diarrhoea episodes were reported among children who used the latrine compared with those who did not use the latrine; this difference was reported two and three years after the provision of latrines and water hand pumps. Among children one to three years old there were improvements three years after the intervention in measures of 'weight-for-age' and 'weight-for-height' in both the intervention group and the control group, and improvements only in the intervention group for 'height-for-age'; it was not clear if this difference was statistically significant. Subgroup analysis suggested that it was not clear if the improvements of these measures were related to use of the latrine. Long term follow-up for the Aziz 1990 study was also conducted. It was reported that nine years after the intervention was initiated latrine use was higher in the intervention village, and that when compared with children in the control village there were fewer diarrhoea episodes in the intervention village. This difference was statistically significant for children over five years but not for children under five years (< 5 year olds 23 (6%) versus 26 (10%) ns, > 5 years old 46 (1.3%) versus 77 (3.0%), P < 0.001).

Housing condition impacts (n = 3)

Non-experimental studies (Overall Grade B) (n = 1)

Rojas de Arias 1999 reported a reduction in the number of houses with triatomine infestation in each of the intervention groups. The reduction was statistically significant in each group, with the biggest reduction reported among the houses which received insecticide only compared with housing improvement only or insecticide and housing improvement (% households with triatomine infestation Int A/B/A+B 45.1 versus 2.4, P < 0.000/32.8 versus 3.4, P < 0.000/48.6 versus 16.4, P < 0.000).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 2)

Spiegel 2003 reported substantial improvements in housing conditions and neighbourhood conditions, however over half the intervention group still reported unmet need for water supply, street and sidewalks, sewage overflow, indoor toilets, garbage collection, local shops, schools, and cultural activities (after intervention (Int/Cont) unmet need for internal housing repair 77.8%/76.9%; unmet need external housing repair 79.7%/87.1%). In addition, similar improvements were reported in the control group. Aziz 1990 reported that latrines and water hand pumps had been installed and were in use throughout the village.

Socio-economic and equity impacts (n = 2)

None of the LMIC studies reported socio-economic outcomes. Two studies reported some health outcomes by gender and age, which may be of relevance with respect to equity of impacts. Studies were assessed to have a low overall study quality (Overall Grade C) (n = 1).

Non-experimental studies (Overall Grade B) (n = 1)

Rojas de Arias 1999 reported subgroup analysis by gender and age. The proportion of sero-positivity for triatomine was reduced among women in each of the intervention groups, the biggest reduction was among the 'insecticide only' group (A) (before versus after A/B/A+B: 6.2 versus 21.7, P = 0.070/15.0 versus 11.1, P = 0.374/19.3 versus 14.6, P = 0.278). None of these changes was statistically significant. Among men triatomine sero-positivity reduced among the 'insecticide only' group (A), but increased in both the 'housing improvement only' group (B) and the 'housing improvement and insecticide' group (A+B) (before versus after A/B/A+B 23.3 versus 7.6, P = 0.121/13.0 versus 14.3, P = 0.776/19.5 versus 22.8, P = 0.492); none of these changes were statistically significant. An analysis by 17 age groups was presented graphically, there was no pattern on the graph to suggest that change in sero-positivity was more likely in a particular age group.

Studies assessed to have a low overall study quality (Overall Grade C) (n = 1)

One XCBA study assessed to be poor quality (Overall Grade C) (Spiegel 2003) reported some health data by gender and age. Prevalence for smoking, physical activity, and self-reported health were reported by age group (four groups) and gender, creating eight subgroups. For smoking there were few significant changes and there did not appear to be any differential impacts. For physical activity, this appeared to be more likely to fall over the time of the intervention among women but not men, although there was a similar pattern in the control group suggesting that this was not related to the intervention. Statistically significant improvements in self-reported health were reported across all age groups among men but only in the youngest group of women (15 to 20 years). There was no statistically significant improvement reported among women in the control neighbourhood and some statistically significant improvement for younger men (15 to 20, 21 to 40 years) in the control neighbourhood.

Qualitative data (n = 0)

None of the LMIC studies reported qualitative data.

Logic model mapping reported impacts

A logic model of the reported impacts of LMIC studies was not prepared as there was only one study with an Overall Grade of A and B and this was not considered sufficient to develop a useful logic model mapping impacts and related pathways to impacts along with socio-economic impacts associated with the housing improvement.

Rehousing from slums (pre-1970), n = 3 (quantitative: 3)

Three studies of rehousing from slums were included in the review reporting impacts between eight months to five years after the rehousing. None of the studies of rehousing from slums used an experimental design and no qualitative studies were reported. Two studies included a control group (McGonigle 1936; Wilner 1960) and assessed outcomes before and after the rehousing; one of these used cross-sectional data before and after the intervention rather than tracing a cohort of residents (McGonigle 1936). The third study was an uncontrolled before and after study (Chapin 1938). For any one outcome domain there was no more than one study which had an Overall Grade A and B.

A summary table of the included studies, their study design, and assessment of study quality is provided below (Table F). Further details of the study characteristics and reported data are provided in Table 9 (see also Table 10; Figure 4; Appendix 2).

Table F. Summary of characteristics of quantitative studies of rehousing from slums

Author, Publication year, Country	Study design	Final sample Int/Cont; Population	Time since intervention	Summary of Study Quality					
				Selection	Confounding	Withdrawals	**Overall grade (HAT)**	Performance	No. of items at low Risk of Bias
Rehousing from slums (before 1970)									
Wilner 1960 USA	CBA	1891/2893 General adult population	<1 year	B	B	A	**A**	B	1
Chapin 1938 USA	UBA	23 General adult population	8-19 months	C	C	A	**C**	B	1

(*Continued*)

McGonigle 1936 UK	XCBA	152/289 Adults and children	5 years	C	B	C	**C**	C	0

Rehousing from slums: population, context, intervention

Population and context

Two of the studies of rehousing from slums were from the USA (Chapin 1938; Wilner 1960) and one was set in the UK (McGonigle 1936). Each of the studies were among people living in poverty. One of the Amercan studies had a mixed ethnic group and the other only included black families (Wilner 1960). McGonigle 1936 reported data for adults and children.

Intervention

The interventions within this category were broadly similar although limited detail was reported. Each of the interventions involved relocating poor families from slum conditions to improved or new housing in a new neighbourhood. One of the studies reported providing improved water and sanitation facilities and it was likely that this was part of the intervention for each of these studies.

General health impacts (n = 0)

None of the studies of rehousing from slums reported general health impacts.

Respiratory health impacts (n = 0)

None of the studies of rehousing from slums reported respiratory health impacts.

Mental health impacts (n = 2)

Non-experimental studies (Overall Grade A and B) (n = 1)

Wilner 1960 reported differences between the intervention group and the control group around one year after the rehousing with respect to six measures of mental health. The intervention group were less likely to report a negative outcome for five of these outcomes (negative mood OR ~0.91, 95% CI 0.70 to 1.82; dissatisfaction with status quo OR ~0.86, 95% CI 0.66 to 1.12; potency OR ~0.81, 95% CI 0.63 to 1.05; pessimism OR ~0.82, 95% CI 0.63 to 1.06; emotionality OR ~0.80, 95% CI 0.61 to 1.03), and more likely to report a negative outcome for one item (nervousness OR ~1.16, 95% CI 0.89 to 1.50). None of these differences were statistically significant. A subgroup analysis reported that the extent of improvement in morale measures was directly related to the extent of housing quality improvement, indicating a dose response relationship (large/med/no change in housing quality optimism scale +25.0%/+16.0%/+5.9%, ~OR 5.33; 'satisfaction with status quo' +34.6%/+25.4%/+14.7%, ~OR 3.07; 'feel 'better off' compared to 5 years ago' +23.1%/+13.3%/-1.5% (this analysis included 33% of control group and appeared to include only half of the 'control group movers', this may be due to movers who were untraceable)).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 1)

One of the poorer quality studies (Overall Grade C) reported an improvement in morale (mean morale score 65.5 versus 63.52) following the intervention (Chapin 1938). A subgroup analysis compared those who had experienced changes in overcrowding. Those moving from an overcrowded house to a non-overcrowded house reported a smaller improvement in morale (-3.8%) compared with those who moved from a non-overcrowded house to an overcrowded house (-8.5%) but those who were overcrowded before and after the move reported an even smaller change in morale (-2.5%).

Other illness and symptom impacts (n = 2)

Non-experimental studies (Overall Grade A and B) (n = 1)

Wilner 1960 reported reductions in the number of illnesses in both the intervention group and the control group. It was unclear if the amount of reduction was greater in the control group or the intervention group as this varied with follow-up (illness episodes in past two months (rate per 1000) Int versus Cont (all ages) -129.9 versus -206.0, Time I-Time -431.1 versus -362.3). The reports of disability were greater following rehousing (OR ~1.145, 95% CI 0.98 to 1.34) but this difference between the intervention and control groups was not statistically significant.

Studies assessed to have a low overall study quality (Grade C) (n = 1)

McGonigle 1936 reported changes in standardized death rates per 1000 people for the local area. Rates before the intervention were higher in the control area, following the rehousing the rates in the intervention area increased but rates in the control area fell (Bef Int/Cont 22.91/33.55 versus Aft Int/Cont 26.10/22.78). The authors reported that the increased death rates affected those from 10 to 65 years of age rather than those at the extremes of life. Infant mortality rates (unclear if these were standardized) per 1000 live births fell in the intervention area and the control area (Bef Int/Cont 172.6/173.2 versus Aft Int/Cont 117.8/134.0). There was no report of an infective epidemic such as tuberculosis, diphtheria, meningitis, or whooping cough to explain increased adult mortality rate.

Housing condition impacts (n = 3)

Non-experimental studies (Overall Grade A and B) (n = 1)

In Wilner 1960 improvements in housing satisfaction and space satisfaction were reported for both the intervention group and the control group, but the increase was greater among the intervention group ('like apartment a lot' Int/Cont +55.3%, P < 0.001/+16.5%, P < 0.001; 'family members not bothered by not enough space' +33.1%/+12.4%). The authors reported that deficiencies such as lack of hot water, sharing of facilities, crowding, lack of central heating, and infestation were wiped out. In general despite considerable moving about in the first 18 months of the 'after' period, control families did not improve their housing to the same extent.

Studies assessed to have a low overall study quality (Overall Grade C) (n = 2)

Chapin 1938 reported a small reduction in the number of rooms per household following the housing improvement (Bef 5.22 versus Aft 4.78), but little change in overcrowding (person to room ratio 0.82 versus 0.83). McGonigle 1936 reported that all the intervention households moved to new housing but no data documenting the changes in housing conditions were reported.

Socio-economic and equity impacts (n = 3)

Non-experimental studies (Overall Grade A and B) (n = 1)

Wilner 1960 reported some improvements among the intervention group in measures of neighbourhood and neighbourliness as well as feelings about their place in the world, while these improvements were all greater than the improvements reported among the control group the differences were sometimes small (Int/Cont 'places where children play are not safe' -39.8%, P < 0.001/+0.5%, ns; 'family often sit and talk' +11.1%, P < 0.01/+1.9%, ns; 'neighbourly contacts live in the building' +59.1%/-3.1%; 'I belong to people going up in world' +7.6%/+6.4%; feel 'better off' compared to five years ago +19.0%, P < 0.001/+4.0%, ns).

Studies assessed to have a low overall study quality (Overall Grade C) (n = 2)

McGonigle 1936 reported changes in mean rent among the intervention and control neighbourhoods for a small group of households. Mean rent was the same at baseline for the two groups but had nearly doubled for the intervention group following the rehousing, while the control group rent had not increased by much (mean rent Bef Int/Cont 4sh8d/4sh8d versus Aft Int/Cont 9sh0d/4sh11d); there was a similar small fall in household income in both groups over the same time period (Bef Int/Cont 47sh1d/44sh7d versus Aft Int/Cont 30sh5d/30sh9d). A greater number of the households in the intervention area were labelled as 'unemployed families' (Int versus Cont 31.3% versus 20.8%) suggesting no income. A further investigation identified a shortage of main dietary constituents except carbohydrates, the shortages were greater in families among the intervention households.

Chapin 1938 also reported an increase in rent for those rehoused (mean dwelling unit rental $15.68 versus $17.98).

Qualitative data (n = 0)

None of the studies of rehousing from slums reported qualitative data.

Logic model mapping reported impacts

A logic model of the reported impacts of rehousing from slums was not prepared as there was only one study with an Overall Grade of A.

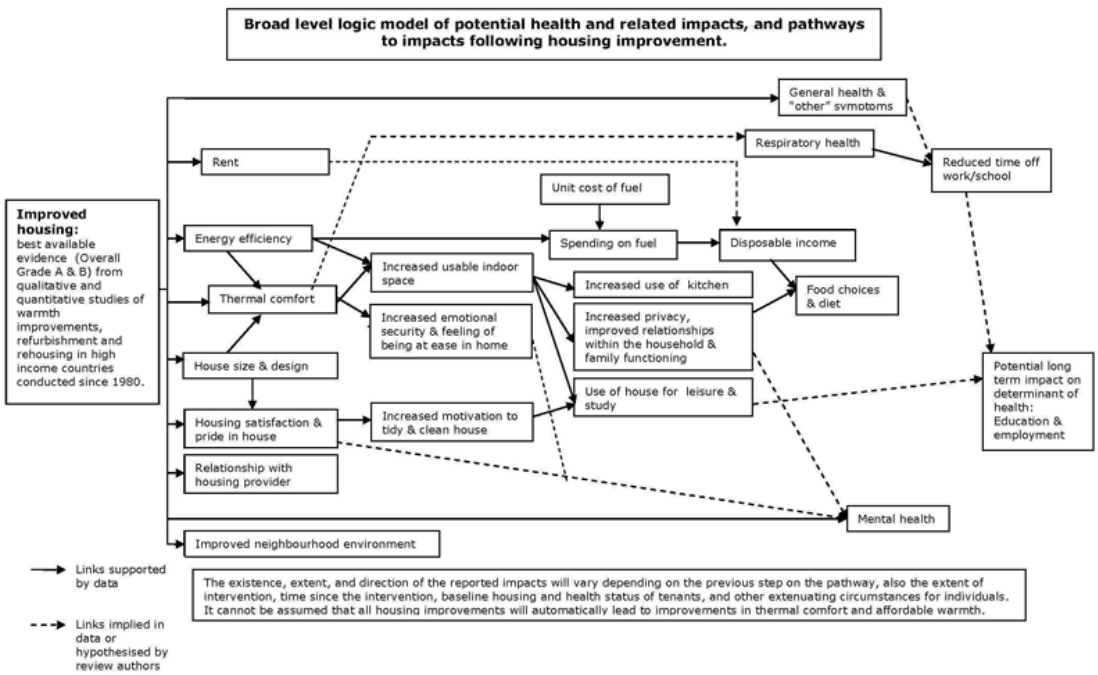
DISCUSSION

Thirty-nine studies assessing the health impacts of housing improvement were included in this review. The majority of the available evidence is quantitative (n = 33). Some of the quantitative studies reported qualitative data (n = 5); an additional six studies reporting qualitative data were identified. Following quality assessment nine of the qualitative studies were included in the final synthesis. Five RCTs were identified, all of warmth improvements. The remaining quantitative studies comprised controlled (n = 17) and uncontrolled studies (n = 11). Over one third (n = 14, 42.4%) of the quantitative studies were assessed to be poor quality (Overall Grade C) and three qualitative studies were assessed

to be of limited value due to poor reporting; these studies were not included in the final synthesis but were examined to identify additional impact types and existence of adverse impacts. All the included studies reported health impacts but only a few reported additional socio-economic impacts and even fewer reported differential impacts across groups relevant to equity issues.

Studies from diverse geographical, cultural, and historical contexts were included. The studies were grouped to reflect the wide range in intervention type as well as broad socio-economic and historical contexts covered by the included studies and to enable a synthesis of broadly similar interventions and contexts. The groups comprised (quantitative studies with an Overall Grade A and B) 19 (11) studies of 'warmth and energy efficiency' improvements (post-1985); 14 (6) studies of 'rehousing or retrofitting ± neighbourhood renewal' (post-1995); 3 (1) studies of 'provision of basic housing' in low or middle income country (post-1990); and 3 (0) studies of rehousing from slums (pre-1970). The four intervention categories are broad and there is a wide range of intervention types within each group (see full details of each intervention evaluated in Table 4; Table 5; Table 6; Table 7). Within the intervention categories, further heterogeneity with respect to study design, study quality, reported outcomes, and study population limited the suitability of a meta-analysis. In addition, poor reporting limited the possibilities for calculating standardized effect sizes. The data were synthesized narratively. A visual summary of reported quantitative data for each intervention category is in Table 10 and Figure 4. In addition, a logic model mapping reported impacts drawing on the better quality quantitative and qualitative data for the warmth and energy efficiency studies and the rehousing or retrofitting studies is provided in Figure 8 and Figure 9, respectively. An overall logic model of the nature and direction of reported health and socio-economic impacts following housing improvements is provided in Figure 10.

Figure 10. Logic model mapping impact types and direction, and links to health impacts reported in qualitative and quantitative studies of modern day housing improvements in developed world (warmth and energy efficiency improvements, and rehousing/retrofitting).



Summary of main results

Warmth and energy efficiency improvements (post-1980)

The most commonly assessed outcome among this group of studies was respiratory health. Studies often reported multiple measures of

respiratory health. An overall assessment of the multiple measures reported within the better quality studies suggests that improvements are possible among adults and children following warmth improvements. However, in some studies there were conflicting results across the variety of measures assessed suggesting an unclear overall impact. Improvements in measures of general health were also reported following warmth improvements. Changes in mental health outcomes were less clear across the better quality studies. There is very little indication of adverse health impacts following warmth improvements in any of the identified studies.

One RCT reported deterioration in general and respiratory health in the ITT analysis; there was no indication of negative impacts in the subgroup analysis comparing those who had received the warmth improvement interventions with those who had not (Osman 2010). Two RCTs from New Zealand reported improvements in all the general health and respiratory health measures assessed, many of which were statistically significant. Both these studies included children and targeted households known to have inadequate warmth and at least one household member with a diagnosed respiratory condition (Howden-Chapman 2007; Howden-Chapman 2008).

Improvements in measures of housing condition were reported most consistently in the New Zealand studies (Howden-Chapman 2007; Howden-Chapman 2008). Housing condition improvements were reported in some of the UK studies but were not clear for all measures, either demonstrating little change or not being statistically significant. This difference in reported changes in housing conditions may reflect the different approach to delivery of housing improvements. The New Zealand studies targeted individual households with inadequate heating, while the UK studies were more likely to deliver the warmth improvements across a whole area. In addition, there is some suggestion that housing conditions are different in the two countries and exposure to cold may be higher in New Zealand than in the UK, suggesting greater potential to benefit in New Zealand. The New Zealand climate is different to the UK, winters are cold and levels of excess winter mortality are similar to those in the UK (Davie 2007) yet insulation and central heating are rare and many houses are constructed from poorly insulated weatherboard (Howden-Chapman 2007).

Three studies which assessed illness related absences from school or work reported statistically significant reductions following the warmth improvements (Howden-Chapman 2007; Howden-Chapman 2008; Somerville 2000). There was a suggestion from one quantitative study that warmth improvements were linked to increased use of the home for hospitality purposes (Platt 2007).

Examination of the poorer quality quantitative studies (Overall Grade C) did not reveal any additional impact types, nor was there any indication of contradictory evidence with regard to effect directions.

A range of impacts were reported and linked to warmth improvements in the included qualitative studies (n = 6). Improved thermal comfort was reported to increase the usable indoor space (Basham 2004; Gilbertson 2006; Harrington 2005). This was subsequently linked to improvements in diet, privacy, household and family relationships as well as opportunities for leisure and studying.

Rehousing and retrofitting ± neighbourhood renewal (post-1995)

The better quality studies within this group all evaluated programmes of housing-led neighbourhood renewal in the UK. The evidence of health impacts from these studies is unclear. Although there were reports of improvement in general health and mental health there were also studies reporting no overall change. Only one small study reported an improvement in general health which was statistically significant (Barnes 2003). One study reported deterioration in mental health following housing improvement. Despite contamination in this study, poorer mental health was also reported in a subgroup analysis comparing those who received housing improvement and those who did not (Thomas 2005). This contrasts with another study which reported no change in mental health among the intervention group but reported a statistically significant improvement in a subgroup analysis comparing those with 'some' and 'no' housing improvement (Kearns 2008). One study reported an increase in wheeze but this was not statistically significant (Kearns 2008). Overall impacts on 'other' illness or health related behaviours were mixed reflecting the diverse outcomes assessed, none of the impacts reported were statistically significant.

These interventions are area based and it is likely that exposure to the intervention and potential to benefit varied considerably across the area and within the study samples. This was reflected in the studies that reported impacts on housing conditions. In three studies the reported improvement in housing condition reported by residents was unclear, in one of these studies a third of the intervention did not receive housing improvements and over half of the control group did (Thomas 2005). Where studies reported change in neighbourhood measures these appeared to improve following the intervention.

Socio-economic impacts were only reported in two quantitative studies. There was little indication of negative socio-economic impacts.

Examination of the poorer quality quantitative studies (Overall Grade C) did not reveal any additional impact types, nor was there any indication of contradictory evidence with regard to effect directions.

In the qualitative data (n= 5 studies) there were reports linking improved housing to improved thermal comfort, increased space, reduced noise, and increased housing satisfaction. Respondents made subsequent links to improvements in physical and mental health. Few studies reported the same reasons for health impacts, but it would appear that those who viewed the housing improvements positively linked the improved living conditions to health improvement.

Provision of basic housing in low and middle income countries (post-1990)

Evidence of the health and socio-economic impacts of housing improvements in LMICs is limited with respect to quantity and quality. One better quality (Overall Grade B) study of improving housing structure to reduce transmission of Chagas disease reported reduction in sero-positivity for triatomine but these improvements were only statistically significant for those living in homes which had been treated with insecticide alone and not for those living in homes benefiting from the structural improvements (Rojas de Arias 1999). The two poorer quality studies identified evaluated different interventions in different contexts. One study assessed housing and neighbourhood improvements in an urban area of Cuba and assessed impacts on self-reported health among adults (Spiegel 2003). The second study assessed impacts of latrine provision in rural Bangladesh, and assessed impacts on childhood diarrhoea (Aziz 1990). Both studies were assessed to be of poor quality using area level data. There was no report of an overall deterioration in the health impacts assessed. In the Cuban study the overall impact was unclear but in the study from Bangladesh there were improvements in the measures assessed. Neither of these studies reported socio-economic impacts. The small number of studies identified in LMICs may be a reflection of the review's inclusion criteria and a different approach to improving living conditions in LMICs. It would appear that in the LMIC context housing related improvements may be delivered at a communal level, for example provision of an improved communal water supply and latrines. This review only included studies where the intervention was delivered at a household level and the small number of included studies should not be interpreted as a near absence of data on improving water and sanitation. Rather it may be that a review of improved living conditions in an LMIC context may require inclusion criteria which are more appropriate to that context.

Rehousing from slums (pre-1970)

Evidence of the health and socio-economic impacts from the historical studies of rehousing from slums is limited with respect to quantity and quality. Only one study was assessed to have an Overall Grade of A or B. This USA study from the 1950s reports improvements in a range of mental health outcomes, these appear to be related to the extent of improvement in housing condition experienced, but are not statistically significant. Impacts on measures of illness and disability are less clear. There were also reports of improved measures of neighbourliness (Wilner 1960).

One of the poorer quality studies (Overall Grade C) reported an increase in adult mortality following rehousing, but no statistics were presented. The authors suggest that this adverse effect was related to an increase in rent among the intervention group impacting of disposable income for an adequate diet (McGonigle 1936).

Overall summary of the health and socio-economic impacts of housing improvement

Using the data on reported health and socio-economic impacts from the quantitative and qualitative studies, and the reported links between improved housing conditions and impacts reported in the qualitative data, a logic model of the impacts following housing improvement was drawn up (Figure 10). The model draws only on the warmth and energy efficiency (post-1980) and rehousing or retrofitting (post-1995) studies as these intervention categories included a group of better quality studies (Overall Grade A and B) and included qualitative data. In addition, the interventions relate to a similar context relevant to modern day housing improvements in the high income countries. Drawing on the better quality studies from these two intervention categories it would appear that improved warmth and energy efficiency measures, which are often part of wider rehousing and retrofitting programmes, can lead to improvements in health. Although the pathways to tangible health impacts are not always clear, the qualitative reports indicate that increased usable indoor space as a result of improvements in thermal comfort and affordable warmth can have many benefits for householders, which may lead to improved physical and mental health.

Overall completeness and applicability of evidence

This review included studies from around the world. The searches were deliberately sensitive to allow for all relevant studies to be identified. The identified studies in this review were grouped according to intervention type as well as time period and context. Studies from LMIC and also those studies which evaluated housing programmes of historical interest were synthesized separately. At a broad level, the main body of evidence and the best available evidence (Overall Grade A and B) relate to modern day housing improvements in high income countries. The majority of identified studies come from the UK (n = 21, 66%), suggesting a gap in the evidence for other countries and contexts. There is a near absence of evidence on the health impacts of housing improvements relevant to LMICs.

The summary of reported quantitative data in Table 10 and Figure 4 provides an indication of the gaps in available evidence with respect to assessment of specific health impact types as well as study designs used to evaluate housing interventions. The field of warmth improvements has the greatest quantity and quality of evidence of health impacts, much of which assesses respiratory outcomes. The body of evidence on warmth improvements includes studies of children, adults, and older adults. However, even for warmth improvements the evidence is limited with respect to a specific intervention, context, population, and timescale for an expected outcome. Very few studies reported data on additional socio-economic outcomes.

Reporting bias

Unclear levels of reporting bias were highly prevalent among the identified studies, casting uncertainty on the completeness of reported data within studies and raising the possibility of reporting bias. A protocol was identified for two studies (CHARISMA 2011; Osman 2010), making it difficult to confirm what outcomes had been selected for reporting and if there were unreported outcomes with conflicting findings. However, many studies did report multiple similar outcomes with conflicting direction of effect, which may lessen the likely influence of reporting bias.

Comparability of data, reported effect type and size

Very few studies reported data amenable to calculation of standardized effect sizes. Although these were calculated and reported where possible (Table 12) these data do not adequately represent the body of evidence identified in this review (Table 9). In addition, the heterogeneity of the reported outcomes alone limited the comparability of studies and the synthesis, either statistically or narratively, even those within the same intervention category. The effect sizes across the diverse outcomes reported are difficult to compare and the synthesis is limited to reporting similar effect types and directions for broadly defined housing interventions and contexts. Despite heterogeneity of population, intervention and context limit the potential for synthesis but provide a good opportunity to investigate explanations for differences in reported impacts.

Applicability of heterogeneous studies: interventions, exposure to intervention, and potential to benefit among study samples

Despite the broad similarities among the groups of studies there are important variations in the nature, components, and implementation of the interventions both between and within studies. This might question how well the studies, even within the same intervention categories, relate to each other and also how usefully the data can be synthesized and the findings applied elsewhere. The variation in the intervention and implementation means that there is likely to be considerable variation in the potential for the intervention to effect improvements in housing conditions both between and within the studies. In addition, there is variation in the context and study population with respect to baseline housing conditions and baseline health status which will influence the potential for both improvements in housing conditions and improvements in health outcomes.

Between study and within study variation in intervention and exposure to improved housing conditions

Available details of interventions and their various components were extracted but studies rarely reported this in much detail. As indicated above, even within the intervention categories across the studies there was considerable variation in the nature of the intervention and housing conditions at baseline, and therefore substantial variation in exposure to improved housing conditions across the studies. Detailed data and standardized data on changes in housing outcomes were not available to allow a robust comparison of the heterogeneity of exposure to housing improvement across the studies.

The assessment of 'intervention Integrity' was developed as part of the study quality assessment to assess within study variation in the extent of the intervention delivered and also variation in the extent of improvement in housing conditions actually reported by householders. A risk of bias item on implementation was also developed to reflect this assessment. Variation in the intervention within a study was often implied but details of the variations were rarely reported. Eleven studies were assessed to have only some or minimal variation in the intervention delivered; for the remaining 21 studies variation in the intervention was considerable or unclear due to poor reporting. There was minimal or some variation in the reported improvement in housing conditions reported by residents in six studies; in the remaining 26 studies variation in reported improvements was considerable or unclear. The warmth and energy efficiency interventions were typically tailored to meet the individual household's requirements, meaning that there could be a wide variation in the extent of the intervention received. Similarly, the area based programmes of rehousing and retrofitting, LMIC programmes, and rehousing from slums comprised various components and it is likely that there would have been considerable variation in what individual households were exposed to as well as the baseline status with respect to housing condition and health status.

A further issue which might determine the overall effectiveness of the housing improvements being evaluated is where improvement in housing conditions might be affected by householders themselves and the use of appliances, in particular use of new heating systems. A programme to install or upgrade heating systems cannot be assumed to improve housing conditions if the householder does not use the system. Reasons for not using a new appliance may be due to lack of knowledge, difficulties in operating, or fear of cost (Winder 2003). Such issues were not reported specifically in any of the included studies.

Between study and within study variation in potential to benefit

The potential to benefit, both with respect to baseline housing conditions and baseline health status, is likely to also affect the potential effectiveness of the intervention.

The potential to benefit varied across the studies. Four of the better quality (Overall Grade A and B) warmth and energy efficiency studies specifically targeted those with poor health. One of these studies, an RCT (Osman 2010) from the UK, targeted

elderly people with a diagnosis of COPD. High levels of contamination were used to explain the absence of reported health improvement in the initial analysis, but health improvement was reported in the TOT analysis. Two other studies which targeted those with poor health were well conducted RCTs from New Zealand (Howden-Chapman 2007; Howden-Chapman 2008). The fourth study, from the UK, was of children with asthma; this was a subgroup analysis within an RCT (CHARISMA 2011). As mentioned earlier in the discussion, it would also appear that baseline housing conditions and exposure to cold might be similar or worse than in the UK, suggesting greater potential to improve housing conditions. In both New Zealand studies all the respiratory health measures were improved among the intervention group compared to the control group following the warmth improvements, and a large proportion of these were statistically significant. This compares with five of the better quality European studies where those with poor health were not targeted and where there were conflicting or unclear impacts on respiratory health (Braubach 2008; Hopton 1996; Lloyd 2008; Platt 2007; Shortt 2007).

There was also variation within studies in the potential to benefit at baseline. Within study samples there was variation in the extent of housing and health problems at baseline and this points to variation in potential for participants to benefit. Details of baseline housing conditions were rarely reported in sufficient detail to allow accurate assessment of the potential to benefit within studies and few studies reported subgroup analysis by the extent of housing improvement experienced.

Contamination

Contamination, where a proportion of the control group receive the intervention, may also skew assessments of effectiveness. None of the included controlled studies were judged to be free from potential contamination but this was largely due to unclear reporting; uncontrolled studies were judged to be at a high risk of bias for this domain. Three controlled studies were also judged to be at a high risk of bias for this domain (Osman 2010; Platt 2007; Thomas 2005). Eight studies (Aziz 1990; Barnes 2003; Chapin 1938; Critchley 2004; Kearns 2008; Osman 2010; Thomas 2005; Wilner 1960) reported subgroup analysis to investigate either the relationship between exposure to a specific change in housing condition or extent of the housing condition. Some of these subgroup analyses indicate more apparent health benefits among groups with confirmed housing improvements compared with the reported impacts for the whole sample.

The above issues are pertinent to the type of interventions included in this review, and other social interventions where there is variation across the study sample with respect to the intervention components, baseline need, and implementation by both providers and users; and also where contamination can arise due to wide availability of the intervention and may introduce Type III error (Dobson 1980; Schwartz 1999). In this review the extent to which these issues influence the reported impacts is unclear but it is likely that these issues may lead to an underestimation of the potential effectiveness of housing improvement to effect health and socio-economic improvements among those in most need. While the many variations in the included studies made synthesis difficult, there was still value in comparing studies with different intervention approaches and contexts. Comparing the findings of the UK studies with the New Zealand studies where baseline health and housing condition was poor indicates that targeting those with the greatest potential to benefit is more likely to lead to health improvements than broader programmes which do not target individual households in most need.

Completeness of evidence for a theory of housing improvement and health and socio-economic impacts

The extreme heterogeneity of the studies included in this review, in particular with respect to the variation in intervention and potential to benefit, might bring into question the applicability and generalisability of the findings of this review. However, it could also be argued that comparing these broadly similar yet individually diverse studies in relation to the intervention received, and also the potential to benefit, can provide a rich data set with which to identify explanations for some of the variation in reported impacts within and between studies.

The main body of evidence relating to modern day housing improvements in high income countries (warmth and energy efficiency (post-1980), and rehousing or retrofitting (post-1995)) was brought together in two separate logic models (Figure 8; Figure 9) and then used to develop a single overall model of housing improvement and health impacts drawing on the best available qualitative and quantitative evidence (Overall Grade A and B) (Figure 10). While this model is empirically based, and may be useful to inform future research and appropriate impacts to be assessed, the model should be regarded as indicative rather than conclusive. The model is limited to reporting effect direction and may also overemphasise the validity and quantity of the qualitative data to the detriment of the quantitative data.

Owing to the few outcomes amenable to calculation of a standardized effect size this model is limited to reporting the nature or type of reported impacts, and it is not possible to comment on the possible effect size. The model demonstrates the value of the qualitative data in reporting links between impacts and pathways to subsequent health impacts. The quantitative data are limited in this regard, only reporting the existence of a health impact rather than reporting intermediate impacts which are likely to act as precursors to subsequent health impacts. The qualitative data report a wider range of impacts compared to the quantitative data, reflecting the open ended questions which are a characteristic strength of qualitative data. Moreover, the quality assessment of the qualitative studies and subsequent data extraction and synthesis were not as comprehensive as the assessment and synthesis of the quan-

titative studies.

Quality of the evidence

This review included experimental studies and controlled and uncontrolled non-experimental studies. To accommodate greater sensitivity to the variations in study quality across the different study designs, the Hamilton tool was developed to incorporate additional items reflecting the standard Cochrane risk of bias items and the EPOC items developed for more complex interventions (comparing baseline characteristics and outcomes, and contamination). The Hamilton tool was used to distinguish between the better quality (Overall Grade A and B) and poorer quality studies (Overall Grade C).

Only the better quality quantitative studies (Overall Grade A and B) (n = 19) were included in the final synthesis. This comprised five RCTs, 13 non-experimental controlled studies, and one uncontrolled study. The poorer quality studies included both controlled and uncontrolled studies. Risk of bias items were rarely assessed to be 'low'; the number of 'low' risk of bias items among the better quality studies (Overall Grade A and B) ranged from zero to six out of a possible 12 items. All studies had at least two items which were 'unclear', either due to poor reporting or because it was not clear to what extent a risk of bias item would influence the reported impacts. This suggests that as a body of evidence there is a considerable risk of bias and that the overall quality of the evidence is poor, in many cases the level of potential bias is largely unknown.

Five RCTs were identified, and these were all studies of warmth and energy efficiency measures. Warmth improvements, in contrast to area based programmes of housing renewal or rehousing, may be easier to control and are therefore more amenable to randomisation. In addition, it would not be possible to randomise area based programmes targeting a single area. Some of the area based interventions used a cross-sectional before and after design (Aziz 1990; Halpern 1995; McGonigle 1936; Spiegel 2003). Although there was no indication of population changes over the study period there is still uncertainty about reported changes in the population where the same cohort of individuals was included at both time points. In addition to being a more robust study design, the RCTs had clearer reporting for some items and were generally well conducted, although only one of the RCTs was assessed as having a low risk of bias for the two selection bias items (Barton 2007). With the exception of study design the quality of evidence did not appear to be related to intervention type.

Because of the inclusion of non-experimental study designs, and the rarity of RCTs, the nature of housing improvements assessment of study quality items relating to randomisation and blinding were not sensitive to variations in study quality in this review. The Hamilton tool included an item on 'data collection' but it was unclear how this might introduce bias in the identified studies and this item and the blinding item were not considered in the assessment of the overall grade for the studies. The items developed by EPOC were more sensitive to variations in study quality, comparing baseline characteristics and outcomes, and contamination. Over half of the better quality studies (Overall Grade A and B) had intervention and control groups with similar health outcomes at baseline, although similarity of demographics and housing quality were less frequently reported. Three additional items considered to be relevant to study quality, baseline response, withdrawals at follow-up, and implementation were added to the risk of bias assessment. Over half of the included studies were judged to have an unrepresentative sample, and also less than half the studies achieved over 60% follow-up of the original sample. It is likely that this introduced bias into the studies. Previous work suggests that those least likely to participate in research are those at most risk of poor health (Parry 2001) and therefore possibly with the greatest potential to benefit. The low levels of recruitment and follow-up may further suggest that the reported impacts are underestimated as those with the greatest potential to benefit may not have participated or completed the study.

The potential influence of variations in the intervention within studies, including implementation, use of the improvements, potential to benefit, and contamination, have been discussed above (see Overall completeness and applicability of evidence) and may introduce additional bias. The extent of the potential bias introduced by these issues is largely unknown due to poor reporting, also it can not be assumed in what direction the bias will have an effect.

Assessment of internal validity of non-randomised studies (NRS): comparison of Cochrane risk of bias (RoB) and Hamilton assessment tool (HAT)

The inclusion of a wide range of non-randomised study designs in this review required consideration of an appropriate tool to critically appraise study quality with respect to internal validity and potential bias. Before embarking on the review, we selected the HAT tool which was developed for reviews of public health interventions, specifically to help assess bias in non-randomised studies. During the course of this review the Cochrane risk of bias (RoB) tool was developed. We were keen to incorporate the RoB tool in this review, partly for completeness and in compliance with Cochrane requirements but also to test the usefulness of the RoB tool for reviews where non-randomised studies are unlikely to provide the main body of evidence.

The RoB assessment of 'high', 'low', and 'unclear' risk of bias is less sensitive to variations in study quality than the HAT assessment which uses 'A', 'B', and 'C'. In addition the development of a grade to indicate overall study quality was useful in maintaining transparency in the final narrative synthesis, both in terms of why studies were included and in providing an immediate indication of variation in study quality across both randomised and non-randomised studies. The use of three categories of potential bias was

useful when reporting both the HAT overall grade and the HAT individual items. To ensure the reader was aware of the variations in the individual HAT items we provided tables detailing each item alongside the text of the narrative synthesis.

Unlike the RoB tool, the HAT tool allows for creation of an overall assessment of study quality. We used this summary measure to make decisions about which studies to include in the final synthesis. The use of a summary measure of bias across a study is contentious. In line with guidance outlined in the *Cochrane Handbook for Systematic Reviews of Interventions*, we did not include the HAT items on 'blinding' or 'data collection' in the overall summary assessment as these items were not considered to be useful in assessing bias for this group of studies. There is some indication that studies which are assessed to have a high RoB in any of the RoB items should be excluded. Applying this to our review would have resulted in an empty review and also assumes that non-randomised studies would not be included in the review, far less the synthesis.

We adapted both the Cochrane RoB tool and the HAT to allow for greater sensitivity to the issues relevant to the studies included in this review. This partly involved articulating specific aspects of both tools, for example specifying what key confounders to be considered when assessing potential bias due to confounding. We also created a new RoB item (baseline response) to allow both tools to be compared with respect to the key items used by HAT to assess overall study quality (study design, selection, confounding, and withdrawals).

Despite developing and applying the RoB tool to the studies in this review we ultimately relied on the HAT to make decisions about which studies to include in the synthesis. We found the sensitivity of the HAT tool to variations in study quality across the diverse study designs and the overall summary grade useful to make use of the best available evidence addressing the review question. The Cochrane RoB tool, and its use in reviews which include non-randomised studies, continues to be the subject of much discussion within The Cochrane Collaboration. The development of a tool to assess potential bias which can be applied across study designs would be useful for reviews which include non-randomised studies and would allow comparison within and across reviews. Within the field of public health this is particularly pertinent if Cochrane reviews are to address questions relating to the health impacts of interventions which have not been evaluated using randomised studies. Further work to test and refine the RoB tool is required.

Potential biases in the review process

This protocol for this review was first approved by The Campbell Collaboration in late 2004 (Thomson 2004), and the review was started in 2005. The completed review was submitted in Autumn of 2007. Following internal editorial review it was agreed that it would be valuable to prepare this review as a joint review with the Campbell and Cochrane Collaborations. The discussions about the procedures for a joint review took some time. We were invited to register the review in July 2009 and submitted the protocol in September 2009; this was approved by both Collaborations in June 2010. Due to the delays it was necessary to update the searches, which were rerun in 2007 and 2010, and then again in July 2012 following internal review. It was impossible to repeat exactly the same search strategy due to changes in the bibliographic databases. The searches in 2007, 2010, and 2012 were developed based on the original search strategy but were made more sensitive where exact terms were not possible. The time delays in this review and changes in review methods, for example introduction of the risk of bias tool, required the review authors to revisit all the studies to ensure that the screening, data extraction, and reporting had been conducted consistently across all the studies regardless of when they were identified.

The searches were sensitive but some literature may be underrepresented. Only two studies from LMICs were identified. The search covered terms which would identify interventions relevant to LMICs but there may be additional terms which were not included in the search strategy. However, intervention studies of housing improvements may be less common in LMICs or may relate to provision of communal facilities rather than interventions related to improvement of individual houses. In addition to searching databases for journal publications we also searched for grey literature. The Campbell Collaboration provided a librarian to search in Scandinavian databases of grey literature; no studies were identified from these searches. Facilities to search for unpublished literature beyond the UK and Scandinavia and non-English studies were limited, and it is possible that additional evidence may be identifiable in sources which we are not aware of.

Many of the identified studies reported more than one outcome for any single outcome domain, general health, mental health, and respiratory health. The outcomes for each domain were combined into a single summary measure to try to avoid bias being introduced by double counting or over-representing reported impacts from any single study.

Two of the authors of this review were also authors of one of the included studies (Thomson 2007).

The limited studies reporting standardized effect size data, and also the near absence of studies similar enough to be synthesized, limited the exploration of publication bias and also sensitivity analysis.

Agreements and disagreements with other studies or reviews

This review has not been published as a Cochrane or Campbell review before. However, earlier versions of this review were published in 2001 and 2009 (Thomson 2001; Thomson 2009). Since the 2001 review there has been a considerable increase in the quantity and also in the quality of studies investigating the health impacts of housing improvement, in particular within the field of

warmth and energy efficiency improvements. In the 2001 review no RCTs were identified. The 2009 review was more similar to this review, but subsequent searching identified two further studies (Aziz 1990; Osman 2010). Studies which did not assess change in health outcomes or did not report data on health outcomes, only reporting impacts narratively, were not included in this review but were included in the earlier 2009 review (Sedky 2001; Aiga 2002; Caldwell 2001; Cattaneo 2007; Choudhary 2002; Eick 2011; Green 1999; Heyman 2011; Warm Front 2008; Winder 2003; Wolff 2001). Five of these studies are from LMICs (Table 1).

The findings and reported conclusions of this review and the 2009 review are very similar. The body of best available evidence is largely the same, with the exception of one more recent RCT (Osman 2010). The data in this review have been subject to more systematic treatment with respect to critical appraisal, extraction, and synthesis. In addition, the 2009 review was limited in what it could report, being published in a journal with a strict word limit. It was not, therefore, possible to present a detailed synthesis or elaborate on the issues encountered.

Since the publication of the protocol for this review there has been a collection of reviews of housing interventions and health published. However the methods, in particular the selection and appraisal of included studies, in these reviews is unclear and the reviews do not have a wide coverage outside the USA (DiGuiseppi 2010; Jacobs 2010; Krieger 2010; Lindberg 2010; Sandel 2010).

AUTHORS' CONCLUSIONS

Implications for practice

The evidence from this review suggests that housing improvements that do deliver tangible improvements in housing conditions can lead to improved health, even a few months after the intervention. This review drew on the best available quantitative and qualitative evidence on changes in health outcomes as well as changes in determinants of health. A logic model mapping the reported health impacts and pathways to health impacts and socio-economic impacts following housing improvement generally has been prepared (Figure 10). It would appear that provision of adequate and affordable space and warmth are key determinants of subsequent health and health impacts, in particular respiratory health. The extent of health improvement reported will depend on the extent of improvement in actual housing conditions experienced by householders. Health improvement is most likely if the housing improvements are targeted at those in most need, that is those living in poor housing and with existing poor health. The nature of available evidence prevents estimates of effect size to be calculated.

Other impacts associated with improved thermal comfort and affordable warmth are linked to an effective increase in house size by increasing usable space. Increased usable space can promote improvements in diet, privacy, household and family relationships, as well as opportunities for leisure and studying. Improvements in health following warmth improvements may also lead to reduced absences from school or work.

The health impacts of housing improvements delivered across a whole area or neighbourhood, rather than targeted according to individual household need, are less clear. Area based interventions may involve a wider range of housing improvements, ranging from a new kitchen to rehousing, and often these programmes will include warmth improvements. However, area based programmes do not discriminate between those in most need. This together with the wide range in the extent of housing improvement delivered means that evaluations reporting impacts for the programme may not detect the possible benefits experienced by subgroups of households with the greatest potential to benefit.

Changes in housing costs may be associated with housing improvement. Improvements in energy efficiency may reduce fuel use but changes in spending on fuel are also influenced by the unit cost of fuel, which has risen significantly in recent years. Rent is usually directly linked to housing quality, and rent will often increase to reflect housing upgrades. For those on low incomes these increases may be covered by welfare provision. There is mixed evidence from studies with regard to impacts on housing costs and disposable income. Area based programmes of housing-led renewal often incorporate wider neighbourhood improvements and may lead to reductions in reported neighbourhood problems. Increased housing costs may prevent the full potential for housing improvement to generate health impacts to be realised.

Evidence from LMICs and historical studies of rehousing from slums is limited both in quantity and quality. Despite the different interventions and contexts there may still be lessons to be learnt from these studies to support the development of a broad theory of housing conditions and health. There is little evidence of adverse impacts following housing improvement, with the exception of where the improvement was followed by a considerable increase in rent.

It may be disappointing that the evidence for the health benefits following housing improvement is not more conclusive. The evidence in this review relates to the effectiveness of housing improvement programmes rather than the efficacy of improved housing conditions on health. Evidence of effectiveness relates to the health impacts which can be expected following implementation of a housing improvement programme, whether or not the programme did lead to actual improvements in housing conditions for occupants. The evidence of effectiveness of housing improvement programmes is inconclusive, and it may be that the potential for improved housing conditions to lead to health impacts may be greater than indicated in the evaluation of housing improvement programmes. There are three possible explanations for this.

It is possible that real improvements in housing conditions were not experienced by those receiving the intervention. Delivery of a housing improvement cannot be assumed to lead to improved housing conditions for occupants. For example, following delivery of a housing improvement the potential for a housing outcome, such as warmth, to be improved may be countered by concerns about costs, confusion about operating a new heating system, etc., or it may be that the baseline housing conditions were already adequate. Secondly, the greatest potential for health improvement is among those with existing poor health. Where a population mostly has good health, it will limit the potential for a programme of housing improvement to lead to significant improvements in health status. Thirdly, many of the housing interventions delivered are widely available for householders to implement themselves, independent of a housing programme. Much of the evidence reviewed here compares those who were part of a housing improvement programme, the intervention group, with those who were not, the control group. Where householders who were not part of the housing improvement programme initiated their own housing improvement during the study this is 'contamination'. Contamination of the control group limits the value of comparing the two groups and makes it more difficult to detect the actual impact of the housing programme. The inconclusive evidence of health impacts following housing improvement may be a result of housing improvement programmes that do not deliver tangible improvements in housing conditions to those with existing health conditions. These issues further underline the need to target households in greatest need and ensuring the intervention delivers tangible improvements in housing conditions if the potential for health improvement following housing improvement is to be maximised.

The evidence reviewed here does not shed any light on the potential for housing improvement programmes on health inequalities. Most of the studies were of low income groups with poor health, and improving living standards and health for these groups is desirable. However, reducing the gap in health outcomes or life expectancy between affluent and deprived groups implies that health outcomes in the worse off groups will improve at a faster rate than among affluent groups. This requires data on the changes in health outcomes across groups. We did not identify studies with suitable data.

Implications for research

The increased quantity and quality of available research evidence over the past decade, particularly within the field of warmth improvements, is welcome. However, it is clear that even with the body of warmth studies there is plenty of room to improve knowledge. The existing group of studies remains diverse and there is no single group of studies that are sufficiently homogeneous to allow a robust synthesis, whether by narrative synthesis or meta-analysis. Much remains to be learned about the timescale of impacts, impacts for specific population groups and contexts, and impacts of specific interventions. The value of qualitative data is evident in this review in its ability to identify impacts not prespecified in questionnaires, and also in identifying possible pathways to impacts on health and more immediate socio-economic determinants of health. Future quantitative evaluations can be enhanced by the inclusion of a qualitative element.

The identification of five RCTs in this review is an exciting addition to research evidence in this field and beyond. These studies demonstrate that RCTs are possible for a community based social intervention and provide tangible examples of how future evaluations of housing improvements might be conducted. However, it may not be either appropriate or feasible to recommend all future studies of housing improvement use an experimental design. Conducting an RCT of an intervention like housing improvement is not easy and the authors of these studies are to be commended for their commitment to the method. RCTs require high levels of control over allocation of the intervention, which requires intense negotiation and well developed relationships between researchers and those paying for and implementing the intervention. This is not always possible. In addition, randomisation of a neighbourhood renewal programme delivered to one or two areas is clearly not workable.

From the available evidence in this review it is clear that there are considerable gaps in knowledge in relation to housing improvement and health, and also relatively simple ways in which the utility of future evidence could be improved. Poor reporting across all the studies meant that key aspects of study quality could not be assessed, leading perhaps to an overestimate of the potential for bias. Improved reporting could reduce the uncertainties around the weight of available evidence. In addition, improved reporting of sample sizes, missing data by outcome, and actual numbers for reported outcomes, rather than just a statistic or a narrative, could greatly increase the data amenable to calculation of a standardized effect size.

Implementation of a housing improvement programme across an area or delivery of housing improvement interventions to households cannot be assumed to lead to improved housing conditions. While a few of the studies in this review did report data to confirm improvements in housing conditions, and some studies reported additional subgroup analysis comparing those who had benefited from improved conditions with those who had not, most of the studies focused on assessing effectiveness of the programme of investment. Knowledge about the efficacy of improved housing conditions for health improvement remains an important and largely poorly investigated topic. It could be argued that establishing efficacy should precede housing investment, which is at least part premised on hypothesised health improvement. Data confirming similarity of the potential to benefit among the intervention and control groups, that is baseline health and housing conditions, as well as changes in housing conditions could greatly improve understanding of the efficacy compared with effectiveness of housing

improvements.

Study size varies considerably in the current body of evidence and this limits the possibilities for subgroup analysis. Where large studies are possible, subgroup analyses by extent of improvement in housing condition, health status at baseline, and other population characteristics, including those relevant to equity issues, would be valuable. These analyses could shed light on what works and for whom, potentially improving the cost-effectiveness of future investment.

Implementation of a housing improvement programme or delivery of housing improvement interventions cannot be assumed to lead to improved housing conditions. Assessment of efficacy and effectiveness is possible within future studies if data are reported on changes in housing conditions within the sample. Knowledge of efficacy can be further enhanced in large studies which allow for subgroup analysis by extent of improvement in housing conditions.

Finally, this review has covered a broad topic. Although there is the over-arching theme of housing improvement the extreme levels of heterogeneity in intervention characteristics, as well as context and populations, has presented challenges to the management of the review and the synthesis. There is a growing number of studies of warmth improvements, there is also potential overlap between some studies of warmth improvements and studies of air quality, dampness and thermal comfort. This, together with the emerging theme linking improved thermal comfort to health suggests that it might be appropriate to split future versions of this review. We recommend that future studies of warmth and energy efficiency measures be managed in a separate review. It may also be appropriate to separate studies of modern day housing improvements conducted in high income countries from those conducted in LMICs.

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Allen 2005

Methods	Uncontrolled before and after
Participants	Residents vulnerable to poor housing referred for health reasons to project (referral criteria- coronary heart disease, cerebro-vascular accident, peripheral vascular disease, type II diabetes with functional difficulties, chronic obstructive pulmonary disease, asthma children with complex and life limiting diseases). All income derived from welfare 46%, 83% of Pakistani origin
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	GHQ-12.
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after
Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group

Allen 2005 (*Continued*)

Baseline response	High risk	Somewhat representative population and 50% baseline response
Implementation of intervention	High risk	Intervention varied considerably across sample

Allen 2005a

Methods	Uncontrolled before and after
Participants	Owner occupiers (94%) with diagnosed serious heart condition. 60% <65 years, 80% lived in home >10 years, 62% Asian, 60% dependant on benefits
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	SF-36 (PCS, MCS), Hospital Anxiety and Depression Scale (HADS)
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after
Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	Insufficient data reported to permit judgement
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group

Allen 2005a (*Continued*)

Baseline response	Unclear risk	Representative population but baseline response unclear
Implementation of intervention	High risk	Intervention varied considerably across sample

Ambrose 2000

Methods	Uncontrolled before and after
Participants	Social housing tenants. High levels of socio-economic deprivation (in receipt of income support 65.4%; unemployed 9.2%). Bangladeshi 69.2%, White 18.7%
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	Illness episodes, symptoms: cough/cold, asthmatic/bronchial, stress/depression, dietary/digestive, aches/pains. Health service use (primary care, hospital admission), medication. Employment
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after
Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data unclear
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group

Ambrose 2000 (*Continued*)

Contamination	High risk	No control group
Baseline response	Low risk	Representative population and 89% baseline response
Implementation of intervention	High risk	Intervention varied considerably across sample

Aziz 1990

Methods	Cross-sectional controlled before and after
Participants	Children living in agricultural villages in rural Bangladesh. Household data: % Illiterate adults male/female 49/78, 77% Muslim
Interventions	Provision of basic housing needs/developing country intervention
Outcomes	Parent reported or clinic reported child episodes of diarrhoea
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Cross-sectional controlled before and after
Allocation concealment (selection bias)	High risk	Cross-sectional controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Analysis at village level - no indication of missing individual level data
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	Baseline health status similar
Baseline characteristics similar	Low risk	Baseline socio-demographics and living conditions similar

Aziz 1990 (*Continued*)

Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Representative population and baseline response not reported
Implementation of intervention	Unclear risk	Some variation in intervention across sample

Barnes 2003

Methods	Controlled before and after
Participants	Social housing tenants. Mixed age groups, 32% have some form of disability. Ethnicity: 65% White; 23% Black/Asian
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	Self-reported health, health problems/emotional problems interfering with daily activities, self-reported pain, discomfort, anxiety, depression. Health service use (primary care)
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	No indication of missing data for individual outcomes
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	Control group had poorer health, not controlled for in analysis

Barnes 2003 (*Continued*)

Baseline characteristics similar	High risk	Control group older and not eligible for housing improvement
Contamination	Unclear risk	Cannot tell
Baseline response	Low risk	Somewhat representative population and 95% baseline response
Implementation of intervention	High risk	Intervention varied considerably across sample

Barton 2007

Methods	Randomised controlled (stepped wedge) design
Participants	Social housing tenants in deprived area (Jarman index of socio-economic deprivation 22.7, regional level of 12.8 (Devon)). 58% <20 years, 10% <50 years
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Child and adult reported asthma symptoms (summed), itchy eyes, water eyes, runny nose, blocked nose, rheumatism, arthritis
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Out of a bucket by councillor at public meeting
Allocation concealment (selection bias)	Low risk	Out of a bucket by councillor at public meeting
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	No indication of missing data for individual outcomes

Barton 2007 (*Continued*)

Selective reporting (reporting bias)	High risk	Lung function data reported in trial register to be collected but not reported. No protocol available
Baseline outcome characteristics similar	Low risk	Baseline reported asthma similar
Baseline characteristics similar	Low risk	Baseline socio-demographic data and housing quality similar
Contamination	Unclear risk	Cannot tell
Baseline response	Low risk	Representative population and 94% baseline response
Implementation of intervention	High risk	Intervention varied considerably across sample

Blackman 2001

Methods	Uncontrolled before and after
Participants	Residents of neighbourhood renewal area, mixed tenure (56.1% owner occupier; 29. 6% social rented), 41.8% in receipt of housing benefit/household with no wage earner; 73.5% 5 years or more lived at this address. 96.4% White; Male/Female 32%/68%; age 0-15 yrs 20.6%; age 16 to 64 yrs 67.5%; age 65+ yrs 12%; Household type (%) n=98 households; Adults plus children 36.1%; non-pensioner adult(s) only 35.1%; 1+ pensioner household 28.9%
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	Self-reported health, self-reported acute respiratory health, (children and adults). Self-reported mental health problem (adults/children). Self-reported health service use, prescribed medication
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after
Blinding of participants and personnel (performance bias) Health	High risk	No control group

Blackman 2001 (*Continued*)

Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group
Baseline response	Low risk	Representative population and 70% baseline response
Implementation of intervention	Unclear risk	Intervention varied across sample but unclear to what extent

Braubach 2008

Methods	Controlled before and after
Participants	Residents of social housing in three neighbourhoods of Frankfurt. Mean age 46 years (range 1-97; 1-17 years 13%, 18-64 years 60%, >64 years 27%); Male/Female 42%/58%. Mix of low and middle income households
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Asthma attacks, sick days, common cold, acute bronchitis/emphysema, depression, self-reported health
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after

Braubach 2008 (*Continued*)

Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Insufficient data reported to permit judgement
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	Health outcomes similar
Baseline characteristics similar	Unclear risk	Insufficient data to permit judgement
Contamination	Unclear risk	4% control group received intervention
Baseline response	High risk	Representative population and 42% baseline response
Implementation of intervention	Unclear risk	Some variation of intervention across sample

Breysse 2011

Methods	Retrospective uncontrolled
Participants	Low income (annual household income $28,000), minority ethnic groups (Adults: White-Hispanic 9%; White-nonHispanic 36%; African 32%, African-American 9%), 67% Female. 57% adults born outside USA
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	Self-reported change in: general health, respiratory health, and injuries (adults and children)
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Retrospective uncontrolled

Breysse 2011 (*Continued*)

Allocation concealment (selection bias)	High risk	Retrospective uncontrolled
Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group
Baseline response	Unclear risk	Very representative and 57% baseline response
Implementation of intervention	Unclear risk	Intervention delivered to meet pre-specified standard and intervention varied to some extent as baseline conditions were not identical

Chapin 1938

Methods	Uncontrolled before and after	
Participants	Residents of housing with inadequate facilities in neighbourhood with high crime rate. Many households foreign born with large families. Ethnicity: Black 62%, Jewish 23%, White 15%	
Interventions	Rehousing from slums (before 1970)	
Outcomes	Morale ('scale to measure degree to which the individual feels competent to cope with the future and achieve his desired goals'), adjustment - 'measure of generalised adjustment'	
Notes		
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**

Chapin 1938 (*Continued*)

Random sequence generation (selection bias)	High risk	Uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after
Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	Insufficient data reported to permit judgement
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group
Baseline response	High risk	Somewhat representative population and 50% baseline response
Implementation of intervention	Low risk	Minimal variation in intervention across sample

CHARISMA 2011

Methods	Randomised controlled trial
Participants	Children aged 5-14 years prescribed >2 steroid inhalers in past year
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	PedsQL. Parent completed asthma specific and general quality of life measure. Days off school
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**

CHARISMA 2011 (*Continued*)

Random sequence generation (selection bias)	Low risk	Contemporaneous dynamic randomisation
Allocation concealment (selection bias)	Low risk	Randomisation performed centrally
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Low risk	Analyst blinded to allocation
Incomplete outcome data (attrition bias) Health	Unclear risk	No indication of missing data for individual outcomes
Selective reporting (reporting bias)	Low risk	Protocol (provided by authors) states primary outcome as reported
Baseline outcome characteristics similar	Low risk	Baseline asthma measures similar
Baseline characteristics similar	Unclear risk	Data on baseline characteristics reported for age eligibility but not socio-economic status
Contamination	Unclear risk	Cannot tell
Baseline response	High risk	Very representative of population but only 43.8% baseline response
Implementation of intervention	High risk	Intervention varied considerably across sample

Critchley 2004

Methods	Controlled before and after
Participants	Social housing tenants. Predominantly retired and dependent on welfare: 66% > 60 years
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	SF-36 (8 domains presented but not analysed by 2 main SF-36 components), self-reported health service use (primary care), affordability
Notes	

Risk of bias

Critchley 2004 (*Continued*)

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported by intervention/ control
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Unclear risk	Insufficient data to permit judgement
Baseline characteristics similar	Unclear risk	Similar eligibility for housing improvement but socio-demographic differences and unclear if this controlled for in analysis
Contamination	Unclear risk	Cannot tell
Baseline response	High risk	Somewhat representative population and 55% baseline response
Implementation of intervention	Low risk	Minimal variation in intervention across sample

Evans 2000

Methods	Controlled before and after
Participants	Private householders in socio-economically deprived urban area
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	SF-36 (selected questions).
Notes	

Risk of bias

Evans 2000 (*Continued*)

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Unclear risk	Small differences but unclear if statistically significant, not controlled for in analysis
Baseline characteristics similar	Unclear risk	Insufficient data to permit judgement, differences not controlled for in analysis
Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Selection process unclear, baseline response not reported
Implementation of intervention	High risk	Intervention varied considerably across sample

Halpern 1995

Methods	Cross-sectional uncontrolled before and after (some control group data)
Participants	Social housing tenants. High number female single parent families; Mean age females interviewed at stage 1, 2 ,3 = 42.4, 39.8, 40.2 years respectively. Mean years at present house 8.2; mean number of children <14 years 1.4; 37% employed; mean reported household income £97-134/wk
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	Hospital Anxiety and Depression Scale (HADS), self esteem.

Notes		
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Cross-sectional uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after
Blinding of participants and personnel (performance bias) Health	High risk	No control group data
Blinding of outcome assessment (detection bias) Health	High risk	No control group data
Blinding of analysts	High risk	No control group data
Incomplete outcome data (attrition bias) Health	Unclear risk	Insufficient data reported to permit judgement
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group data
Baseline characteristics similar	High risk	Similar socio-demographics, differences in eligibility for improvement and housing quality unclear, final follow-up control group data not reported
Contamination	High risk	Cannot tell and limited control group data
Baseline response	Unclear risk	Somewhat representative population and 60-70% baseline response
Implementation of intervention	High risk	Intervention varied considerably across sample

Health Action Kirklees

Methods	Retrospective uncontrolled
Participants	Private householders, under 60 years/with young children/not in receipt of welfare, who suffer from or are at risk from cold related illness (confirmed by health professional)
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Self-reported health, health service use, medication use.
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Retrospective uncontrolled study
Allocation concealment (selection bias)	High risk	Retrospective uncontrolled study
Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	High risk	Retrospective study
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group
Baseline response	Low risk	Representative population and 73% baseline response
Implementation of intervention	Unclear risk	Intervention varied across sample but unclear to what extent

Hopton 1996

Methods	Controlled before and after
Participants	Social housing tenants in isolated deprived neighbourhood: 42% household with someone unemployed
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Parent reported children's symptoms (list of 16).
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported by intervention/control, and numbers not reported by outcome
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Unclear risk	Insufficient data to permit judgement
Baseline characteristics similar	Unclear risk	Both groups from same housing area but other similarities unclear
Contamination	Unclear risk	Cannot tell
Baseline response	Low risk	Representative population and 83% baseline response
Implementation of intervention	Unclear risk	Intervention varied across sample but unclear to what extent

Howden-Chapman 2007

Methods	Randomised controlled trial
Participants	Various tenures (24% rented, 76% owner occupier - nationally 32%/68%). At least one person in household suffered from respiratory disease, lived in uninsulated house. 66% in bottom 3 deciles of deprived areas. Ethnicity: 49% Maori migrant pacific. 66% in bottom 3 deciles of deprived areas
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Self-reported health, self-reported wheezing, morning phlegm, sleep disturbed by wheezing, speech disturbed by wheezing, SF-36 (selected questions reported). Health service use (primary care and hospital admission for respiratory condition). Attendance at or days off school or work
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Randomisation sequence generated by independent researcher
Allocation concealment (selection bias)	Low risk	Randomisation sequence generated by independent researcher
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Low risk	Similar numbers and reasons for missing data across groups
Selective reporting (reporting bias)	Unclear risk	Incomplete SF-36 data reported, no protocol available.
Baseline outcome characteristics similar	Low risk	Baseline health outcomes similar
Baseline characteristics similar	Low risk	Baseline socio-demographics and housing quality similar
Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Somewhat representative of population but baseline response not reported

Howden-Chapman 2007 (*Continued*)

Implementation of intervention	High risk	Intervention varied considerably across sample

Howden-Chapman 2008

Methods	Randomised controlled trial
Participants	Four New Zealand cities. Households with child (6-12 years) with doctor diagnosed asthma in house with main form of heating plug in heater or unflued LPG heater. Mean age 9.6 years, ~58.5% male, ~36.5% Maori (compared to 15% general population), 47% NZ European Int/Cont
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Peak flow, FEV, LRS, URS, cough (various measures), use of inhalers, wheeze, diarrhoea, vomiting, infections, twisted ankle, health service use related to asthma, days of school
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Randomisation sequence generated by independent researcher
Allocation concealment (selection bias)	Low risk	Randomisation sequence generated by independent researcher
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or study personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Low risk	Similar numbers and reasons for missing data across groups
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	Baseline health outcomes similar
Baseline characteristics similar	Low risk	Baseline socio-demographics & housing quality similar
Contamination	Unclear risk	Cannot tell

Baseline response	Unclear risk	Somewhat representative of population but baseline response not reported
Implementation of intervention	Unclear risk	Some variation in intervention across sample

Iversen 1986

Methods	Controlled before and after
Participants	Private low-rise flatted housing in middle income area
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Symptoms: eye irritation, joint pains, dry throat.
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	Baseline outcome value controlled for in analysis
Baseline characteristics similar	Low risk	Analysis controlled for differences in area, age, sex and smoking
Contamination	Unclear risk	Cannot tell

Iversen 1986 (*Continued*)

Baseline response	Unclear risk	Representativeness and selection unclear, 54% baseline response
Implementation of intervention	Low risk	Minimal variation in intervention across sample

Kearns 2008

Methods	Controlled before and after
Participants	Social housing tenants. Age <30 yrs 15.8%; >60 yrs 14.4%; 77.9% urban resident, 21.4% rural resident
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	SF-36, common symptoms, psycho-social benefits plus qualitative data. Income and affordability
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	Baseline health differences controlled for in analysis
Baseline characteristics similar	High risk	Control group lived in better quality housing and older than intervention group

Kearns 2008 (*Continued*)

Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Baseline response not reported
Implementation of intervention	Unclear risk	Intervention varied across sample but unclear to what extent

Lloyd 2008

Methods	Controlled before and after
Participants	Social housing tenants in deprived neighbourhood
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Blood pressure.
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported by intervention/control
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	Blood pressure at baseline similar
Baseline characteristics similar	Unclear risk	Housing type similar but insufficient socio-demographic data to permit judgement

Lloyd 2008 (*Continued*)

Contamination	Unclear risk	Cannot tell
Baseline response	Low risk	Representative population and 72% baseline response
Implementation of intervention	Unclear risk	Intervention varied across sample but unclear to what extent

McGonigle 1936

Methods	Cross-sectional controlled before and after
Participants	Residents of slum areas with higher mortality rates than rest of England and local borough; 18.75 and 22.15 deaths per 1000 compared with 12.00 and 13.96
Interventions	Rehousing from slums (before 1970)
Outcomes	Standardized death rates (adult and infant); adequacy of diet, income and affordability, employment
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Cross-sectional controlled before and after
Allocation concealment (selection bias)	High risk	Cross-sectional controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Unclear risk	Both areas had similar outcomes but insufficient data to permit judgement

McGonigle 1936 (*Continued*)

Baseline characteristics similar	Unclear risk	Both groups from similar areas but insufficient data to permit judgement
Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Somewhat representative population and area level data used
Implementation of intervention	Unclear risk	Intervention varied across sample but unclear to what extent

Molnar 2010

Methods	Uncontrolled before and after
Participants	Roma adults living in disadvantaged rural village. Previously living in life-threatening conditions
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	Functional limitations, chronic disease, infections, injuries
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after
Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	No indication of missing data for individual outcomes
Selective reporting (reporting bias)	Unclear risk	No protocol available

Molnar 2010 (*Continued*)

Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group
Baseline response	Unclear risk	Representativeness and baseline response unclear
Implementation of intervention	High risk	Mix of refurbishment and rehousing

Osman 2010

Methods	Randomised controlled trial
Participants	Elderly people with recent hospital admission for COPD living in own homes (47% social housing)
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	St Georges Respiratory Questionnaire, Euroqol Visual Analogue Scale, fuel costs
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Method of sequence generation not reported
Allocation concealment (selection bias)	Unclear risk	Method of allocation concealment not described
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Unclear reasons for withdrawals reported, ITT analysis, but no indication of missing data
Selective reporting (reporting bias)	Low risk	Project summary and protocol available and all stated outcomes reported

Osman 2010 (*Continued*)

Baseline outcome characteristics similar	Low risk	Baseline health status similar
Baseline characteristics similar	Low risk	Baseline socio-demographic data, eligibility for improvement and housing quality similar
Contamination	High risk	18% control group received intervention
Baseline response	Unclear risk	Somewhat representative and baseline response not reported
Implementation of intervention	Unclear risk	Intervention varied across sample but unclear to what extent

Platt 2007

Methods	Controlled before and after
Participants	Social housing tenants (53.5%) and owner-occupiers (41.5%). Mean age 62 years, Male/Female 36%/64%, socio-economically deprived 61%, predominantly pensioners with no children in house
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	SF-36 (2 domains presented), self-reported symptoms (17, includes first diagnosis of: heart disease, nasal allergy, hypertension, smoking). 4 self-report health service use, 2 self-reported medication use. Income and affordability, social contact and relationships within the household and beyond
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts

Platt 2007 (*Continued*)

Incomplete outcome data (attrition bias) Health	Unclear risk	Some differences between intervention/control group but insufficient data to judge
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	Analysis controlled for baseline outcome value
Baseline characteristics similar	Unclear risk	Similar socio-demographic data and house type. Control group not eligible for housing improvement and some already had intervention
Contamination	High risk	13% control group had intervention at baseline
Baseline response	Unclear risk	Baseline response not reported
Implementation of intervention	Low risk	Minimal variation in intervention across sample

Rojas de Arias 1999

Methods	Controlled before and after (three intervention groups)
Participants	Rural households 50-100km from capital of Paraguay. Housing mainly made of mud walls and thatched rooves
Interventions	Provision of basic housing needs/developing country intervention
Outcomes	Sero-positivity of Triatomine cruzi (ELISA and IIF).
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors

Rojas de Arias 1999 (*Continued*)

Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No data provided
Baseline characteristics similar	High risk	No data provided
Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Baseline response not reported
Implementation of intervention	Unclear risk	Analysis of 3 groups by intervention received but intervention delivered to 67-90% sample

Shortt 2007

Methods	Controlled before and after
Participants	High percentage >60 years and <5 years. High proportion owner occupiers/private rented housing in rural areas, in receipt of welfare benefits. 78% Int group houses built pre-1950. Low uptake of domestic energy efficiency improvements; Areas in middle range of deprivation index
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Self-reported health, GP data on small number, self-reported respiratory conditions, angina and mental/stress conditions. Income and affordability
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel

Shortt 2007 (*Continued*)

Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	No indication of missing data for individual outcomes
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	Control group health outcomes better
Baseline characteristics similar	High risk	Control group not eligible for improvement, lived in newer houses and were younger, differences not controlled for in analysis
Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Somewhat representative population, baseline response not reported
Implementation of intervention	High risk	Intervention varied considerably across sample

Somerville 2000

Methods	Uncontrolled before and after
Participants	Asthmatic children under 16 years living in social housing reported to have damp
Interventions	Warmth and energy efficiency improvements (after 1980)
Outcomes	Self-rated asthma symptoms (summed score of cough by day/night, wheeze by day/night, breathless with exercise, breathless), hay fever, diarrhoea. Attendance at or days off school or work
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after

Somerville 2000 (*Continued*)

Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	Insufficient data reported to permit judgement
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group
Baseline response	Low risk	Somewhat representative population and 75% baseline response, selection unclear
Implementation of intervention	Unclear risk	Some variation in intervention across sample

Spiegel 2003

Methods	Cross-sectional controlled before and after
Participants	Urban neighbourhood with predominantly dilapidated buildings and inadequate basic amenities such as potable water. Male/Female 41%/59%, mean age 45.1 years, education 11.2 years (mean), Ethnicity: White 58%, Mulatto/Black 36%
Interventions	Provision of basic housing needs/developing country intervention
Outcomes	Self-reported health, smoking, respiratory illness, suicide attempts
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Cross-sectional controlled before and after

Spiegel 2003 (*Continued*)

Allocation concealment (selection bias)	High risk	Cross-sectional controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	High risk	Retrospective design
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Unclear risk	Some differences in suicide and respiratory outcomes
Baseline characteristics similar	Unclear risk	Area location and type similar
Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Somewhat representative population but baseline response not reported
Implementation of intervention	High risk	Intervention varied considerably across sample

Thomas 2005

Methods	Controlled before and after	
Participants	Social housing tenants in deprived area. Mean age Int/Cont 51/53, Male/Female 52%/48%	
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)	
Outcomes	GHQ-12.	
Notes		
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**

Thomas 2005 (*Continued*)

Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Insufficient data reported to permit judgement
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	GHQ score similar at baseline
Baseline characteristics similar	Unclear risk	Socio-demographics similar, but unclear if housing quality, type & eligibility for improvement similar. Analysis controlled for differences in area and age
Contamination	High risk	55% control area and 65% intervention area received housing improvement
Baseline response	High risk	Somewhat representative population and 17% baseline response
Implementation of intervention	High risk	Intervention varied considerably across sample

Thomson 2007

Methods	Controlled before and after
Participants	Social housing tenants. More than half of participants were dependent on housing benefit
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	Self-reported health, SF-36 (PCS & MCS).
Notes	
Risk of bias	

Thomson 2007 (*Continued*)

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for missing data not reported
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Low risk	Health outcomes similar at baseline
Baseline characteristics similar	Low risk	Both groups similar socio-demographics and housing quality. Control group not eligible for intervention
Contamination	Unclear risk	Cannot tell
Baseline response	High risk	Representative population and 49% baseline response
Implementation of intervention	Low risk	Minimal variation in intervention across sample

Wells 2000

Methods	Uncontrolled before and after
Participants	Families on fringe of home-ownership, in need of improved housing and willing to enter commitment of housing partnership including mortgage contributions. 74% female head of household; family size 2 to 8 persons. Mean income/month $1,396, mean income to needs ratio=1.10 (1.0=poverty line). Ethnicity: 61% African-American, 37% White; mean age 33 years
Interventions	Rehousing or retrofitting with or without neighbourhood renewal (after 1995)
Outcomes	Psychological well-being (instrument - PERI - Psychiatric Epidemiology Research Instrument for non-clinical populations - 21 item)

Wells 2000 (*Continued*)

Notes		
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Uncontrolled before and after
Allocation concealment (selection bias)	High risk	Uncontrolled before and after
Blinding of participants and personnel (performance bias) Health	High risk	No control group
Blinding of outcome assessment (detection bias) Health	High risk	No control group
Blinding of analysts	High risk	No control group
Incomplete outcome data (attrition bias) Health	Unclear risk	Reasons for withdrawals reported unclear if related to final outcome
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	High risk	No control group
Baseline characteristics similar	High risk	No control group
Contamination	High risk	No control group
Baseline response	High risk	Target population, small selected sample, and baseline response unclear
Implementation of intervention	Low risk	Minimal variation in intervention across sample

Wilner 1960

Methods	Controlled before and after
Participants	Black families living in slum areas
Interventions	Rehousing from slums (before 1970)
Outcomes	Self-reported illness episodes, positive mood, nervousness, morale, optimism/pessimism. Income and affordability, social contact and relationships within the household and

	beyond	
Notes		
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Controlled before and after
Allocation concealment (selection bias)	High risk	Controlled before and after
Blinding of participants and personnel (performance bias) Health	Unclear risk	No report of blinding of study participants or personnel
Blinding of outcome assessment (detection bias) Health	Unclear risk	No report of blinding of outcome assessors
Blinding of analysts	Unclear risk	No report of blinding of data analysts
Incomplete outcome data (attrition bias) Health	Unclear risk	No indication of missing data for individual outcomes
Selective reporting (reporting bias)	Unclear risk	No protocol available
Baseline outcome characteristics similar	Unclear risk	Similar health outcomes at baseline but data unclear
Baseline characteristics similar	Unclear risk	Data indicates similar socio-demographic data and housing quality. Control group not eligible for housing improvement
Contamination	Unclear risk	Cannot tell
Baseline response	Unclear risk	Somewhat representative population and 79% baseline response
Implementation of intervention	Low risk	Minimal variation in intervention across sample

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Aiga 2002	Direct health outcome assessed but no assessment of change following intervention
Allen 2011	Air quality interventions not eligible for inclusion, and included in another Cochrane review. See Excluded studies 'Results' section
Bailie 2012	Unclear what housing improvement comprised and who in the sample received it. Estimated <17% in receipt of intervention
Burr 2007	Air quality interventions not eligible for inclusion, and included in another Cochrane review. See Excluded studies 'Results' section
Butala 2010	Case control study
Caldwell 2001	No data reported for direct health outcomes
Cattaneo 2007	Direct health outcome assessed but no assessment of change following intervention
Choudhary 2002	Direct health outcome assessed but no assessment of change following intervention
Coggon 1991	Case control study
Eick 2011	Data unclear for intervention included in the review
El Ansari 2008	Area level data, unclear proportion exposed to housing improvement
Ferguson 1954	Direct health outcome assessed but no assessment of change following intervention
Green 1999	Direct health outcome assessed but no assessment of change following intervention
Heyman 2011	No data reported for direct health outcomes
Jackson 2011	Health service use outcomes only and no baseline data
Jones 1999	Case control study
Kahlmeier 2001	Participants not part of discrete housing improvement intervention
Keatinge 1989	Case control study
Kovesi 2009	Air quality interventions not eligible for inclusion, and included in another Cochrane review. See Excluded studies 'Results' section
Marsh 1999	Retrospective analysis - participants not part of discrete housing improvement programme
Meddings 2004	Case control study

(*Continued*)

Pholeros 1993	No direct health outcomes reported - health service use data only
Roder 2008	No data reported for direct health outcomes
Sedky 2001	Direct health outcome assessed but no assessment of change following intervention
Smith 1997	Direct health outcome assessed but no assessment of change following intervention
Telfar-Barnard 2011	Direct health outcome assessed but no assessment of change following intervention
Vyas 1998	Insufficient information available - author contacted but no response
Walker 1999	No direct health outcomes reported - health service use data only
Wambem 1973	No direct health outcomes reported - health service use data only
Warm Front 2008	Direct health outcome assessed but no assessment of change following intervention
Warner 2000	Air quality interventions not eligible for inclusion, and included in another Cochrane review. See Excluded studies 'Results' section
Westaway 2007	Unclear if intervention eligible - author contacted but no response
Winder 2003	No data reported for direct health outcomes
Wolff 2001	Direct health outcome assessed but no assessment of change following intervention
Woodin 1996	No direct health outcomes reported- health service use data only
Wright 2009	Air quality interventions not eligible for inclusion, and included in another Cochrane review. See Excluded studies 'Results' section

Characteristics of studies awaiting assessment *[ordered by study ID]*

Decent Homes 2012

Methods	Qualitative interviews
Participants	Social housing tenants
Interventions	Warmth improvements
Outcomes	Open ended
Notes	

Ellaway 2000

Methods	Qualitative interviews
Participants	Social housing tenants
Interventions	Rehousing or refurbishment
Outcomes	Open ended
Notes	

Characteristics of ongoing studies *[ordered by study ID]*

GoWell

Trial name or title	GoWell
Methods	Cross sectional Controlled Before & After with some longitudinal follow-up over 10 years
Participants	Residents of deprived neighbourhoods in Glasgow, predominantly social housing tenants
Interventions	Major neighbourhood and housing investment
Outcomes	SF-12 and multiple measures of wellbeing
Starting date	2006
Contact information	Ade Kearns (Principal Investigator) a.kearns@lbss.gla.ac.uk
Notes	

Lyons 2011

Trial name or title	Health impact, and economic value, of meeting housing quality standards
Methods	Controlled before and after using routine data
Participants	Social housing tenants (>20,000 households)
Interventions	Housing-led neighbourhood regeneration
Outcomes	Well-being and health service use
Starting date	2011
Contact information	r.a.lyons@swansea.ac.uk

Notes	

WHEZ

Trial name or title	Warm Homes for Elder New Zealanders
Methods	Randomised controlled trial
Participants	Adults over 55 years with diagnosed chronic obstructive pulmonary disease (COPD) who reported an exacerbation in the last 3 years, or who have 'moderate' (or worse) COPD
Interventions	The participants are randomised to receive a fuel voucher/subsidy (NZ $500). Participants will also have their house insulated if necessary and feasible
Outcomes	Moderate exacerbations of COPD that are treated with systemic corticosteroids and/or antibiotics. Severe exacerbations of COPD for which hospitalisation is required
Starting date	2008
Contact information	WHEZ
Notes	

DATA AND ANALYSES

Comparison 1. Standardized effect estimates for self-reported health following warmth and energy efficiency improvements (post-1985)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Poor/fair self-reported health	1		Odds Ratio (Random, 95% CI)	0.59 [0.47, 0.74]
2 Poor/fair self-reported health (children)	1		Odds Ratio (Random, 95% CI)	0.48 [0.31, 0.74]

Comparison 2. Standardized effect estimates for respiratory outcomes following warmth and energy efficiency improvements (post-1985)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Experimental studies	3		Odds Ratio (Random, 95% CI)	Subtotals only
1.1 Sleep disturbed by wheeze (children)	2		Odds Ratio (Random, 95% CI)	0.56 [0.43, 0.74]
1.2 Speech disturbed by wheeze (children)	2		Odds Ratio (Random, 95% CI)	0.59 [0.41, 0.85]
1.3 Dry cough at night (children)	1		Odds Ratio (Random, 95% CI)	0.52 [0.32, 0.85]
1.4 Wheeze during exercise (children)	1		Odds Ratio (Random, 95% CI)	0.67 [0.42, 1.07]
1.5 Wheeze in past 3 months (children & adults)	1		Odds Ratio (Random, 95% CI)	0.57 [0.47, 0.70]
1.6 Morning phlegm	1		Odds Ratio (Random, 95% CI)	0.64 [0.52, 0.78]
1.7 Cold or flu (children & adults)	1		Odds Ratio (Random, 95% CI)	0.54 [0.43, 0.69]
1.8 Asthma (children & adults)	1		Odds Ratio (Random, 95% CI)	0.95 [0.60, 1.50]
1.9 Bronchitis (children & adults)	1		Odds Ratio (Random, 95% CI)	1.01 [0.48, 2.13]
1.10 Other respiratory symptoms (children & adults)	1		Odds Ratio (Random, 95% CI)	1.01 [0.56, 1.82]
2 Non-experimental studies	3		Odds Ratio (Random, 95% CI)	Subtotals only
2.1 Ever diagnosed nasal allergy	1		Odds Ratio (Random, 95% CI)	1.52 [1.05, 2.20]
2.2 Ever diagnosed bronchitis	1		Odds Ratio (Random, 95% CI)	1.29 [0.97, 1.72]
2.3 Ever diagnosed asthma	1		Odds Ratio (Random, 95% CI)	0.92 [0.63, 1.34]
2.4 Asthma	1		Odds Ratio (Random, 95% CI)	0.57 [0.10, 3.26]
2.5 Chest infection/bronchitis	1		Odds Ratio (Random, 95% CI)	1.87 [0.50, 7.10]
2.6 Pneumonia/hypothermia	1		Odds Ratio (Random, 95% CI)	3.59 [0.14, 90.28]

2.7 Persistent cough (children)	1	Odds Ratio (Random, 95% CI)	0.97 [0.44, 2.15]
2.8 Wheezing (children)	1	Odds Ratio (Random, 95% CI)	1.13 [0.47, 2.71]
2.9 Runny nose (children)	1	Odds Ratio (Random, 95% CI)	0.69 [0.34, 1.40]

Comparison 3. Standardized effect estimates for mental health outcomes following warmth and energy efficiency improvements (post-1985)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Experimental studies	1		Odds Ratio (Random, 95% CI)	Subtotals only
1.1 Low happiness (SF-36)	1		Odds Ratio (Random, 95% CI)	0.56 [0.41, 0.77]
1.2 Low vitality (SF-36)	1		Odds Ratio (Random, 95% CI)	0.51 [0.41, 0.64]
2 Non-experimental studies	3		Odds Ratio (Random, 95% CI)	Subtotals only
2.1 Depression	1		Odds Ratio (Random, 95% CI)	1.40 [0.33, 5.99]
2.2 Stress/Mental illness	1		Odds Ratio (Random, 95% CI)	0.26 [0.05, 1.29]
2.3 Feeling down (children)	1		Odds Ratio (Random, 95% CI)	0.66 [0.23, 1.89]
2.4 Temper tantrums (children)	1		Odds Ratio (Random, 95% CI)	0.97 [0.44, 2.15]
2.5 Irritability (children)	1		Odds Ratio (Random, 95% CI)	1.54 [0.57, 4.20]

Comparison 4. Standardized effect estimates for illness and symptom outcomes following warmth and energy efficiency improvements (post-1985)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Experimental studies	2		Odds Ratio (Random, 95% CI)	Subtotals only
1.1 Diarrhoea (children)	1		Odds Ratio (Random, 95% CI)	0.72 [0.45, 1.15]
1.2 Ear infection (children)	1		Odds Ratio (Random, 95% CI)	1.16 [0.68, 1.98]
1.3 Vomitting (children)	1		Odds Ratio (Random, 95% CI)	0.88 [0.55, 1.41]
1.4 Twisted ankle (children)	1		Odds Ratio (Random, 95% CI)	1.86 [1.03, 3.36]
1.5 Arthritis (children & adults)	1		Odds Ratio (Random, 95% CI)	1.06 [0.53, 2.10]
1.6 Rheumatism (children & adults)	1		Odds Ratio (Random, 95% CI)	1.91 [0.83, 4.39]
2 Non-experimental studies	3		Odds Ratio (Random, 95% CI)	Subtotals only
2.1 Ever diagnosed hypertension	1		Odds Ratio (Random, 95% CI)	0.77 [0.61, 0.97]
2.2 Ever diagnosed heart disease	1		Odds Ratio (Random, 95% CI)	0.69 [0.52, 0.92]
2.3 Ever diagnosed circulation problem	1		Odds Ratio (Random, 95% CI)	1.06 [0.83, 1.35]
2.4 Ever diagnosed eczema	1		Odds Ratio (Random, 95% CI)	1.43 [0.89, 2.30]
2.5 “Other” Illnesses	1		Odds Ratio (Random, 95% CI)	0.57 [0.10, 3.26]
2.6 Arthritis	1		Odds Ratio (Random, 95% CI)	1.62 [0.34, 7.64]
2.7 Angina	1		Odds Ratio (Random, 95% CI)	0.20 [0.04, 0.98]

2.8 Aches & pains (children)	1	Odds Ratio (Random, 95% CI)	1.54 [0.66, 3.56]
2.9 Diarrhoea (children)	1	Odds Ratio (Random, 95% CI)	0.73 [0.25, 2.13]
2.10 Earache (children)	1	Odds Ratio (Random, 95% CI)	0.98 [0.35, 2.75]
2.11 Fever (children)	1	Odds Ratio (Random, 95% CI)	0.78 [0.33, 1.87]
2.12 Headaches (children)	1	Odds Ratio (Random, 95% CI)	0.68 [0.23, 1.99]
2.13 Poor appetite (children)	1	Odds Ratio (Random, 95% CI)	0.34 [0.15, 0.80]
2.14 Sore throat (children)	1	Odds Ratio (Random, 95% CI)	1.35 [0.67, 2.75]
2.15 Vomiting (children)	1	Odds Ratio (Random, 95% CI)	0.96 [0.38, 2.44]
2.16 Tiredness (children)	1	Odds Ratio (Random, 95% CI)	1.52 [0.64, 3.61]

Comparison 5. Standardized effect estimates for general health outcomes following rehousing or retrofitting with or without neighbourhood renewal (post-1995) (non-experimental studies)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Poor/fair self-reported health	3		Odds Ratio (Fixed, 95% CI)	Subtotals only
2 Long standing illness	1		Odds Ratio (Random, 95% CI)	0.68 [0.44, 1.05]
3 Health not improved/worse since one year ago	2		Odds Ratio (Random, 95% CI)	0.60 [0.29, 1.26]
4 Health interferes with daily activities	1		Odds Ratio (Random, 95% CI)	1.52 [0.62, 3.73]
5 Lower Physical Component score (SF-36)	1		Odds Ratio (Random, 95% CI)	0.96 [0.44, 2.11]
6 Physical or emotional problems with daily life in past month	1		Odds Ratio (Random, 95% CI)	0.34 [0.14, 0.83]

Comparison 6. Standardized effect estimates for respiratory health outcomes following rehousing or retrofitting with or without neighbourhood renewal (post-1995) (non-experimental studies)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Wheezing in past year	1		Odds Ratio (Random, 95% CI)	1.04 [0.69, 1.57]
2 Asthma (children)	1		Odds Ratio (Random, 95% CI)	1.04 [0.65, 1.66]
3 Breathlessness (children)	1		Odds Ratio (Random, 95% CI)	1.18 [0.46, 3.06]
4 Persistent cough (children)	1		Odds Ratio (Random, 95% CI)	1.09 [0.66, 1.80]
5 Bronchitis (children)	1		Odds Ratio (Random, 95% CI)	0.31 [0.03, 3.02]
6 Sinus/Cattarh (children)	1		Odds Ratio (Random, 95% CI)	0.89 [0.48, 1.65]

Comparison 7. Standardized effect estimates for mental health outcomes following rehousing or retrofitting with or without neighbourhood renewal (post-1995) (non-experimental studies)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Lower mental component score (SF-36)	1		Odds Ratio (Random, 95% CI)	0.73 [0.33, 1.61]
2 Anxiety/depression (self-reported)	1		Odds Ratio (Random, 95% CI)	0.36 [0.15, 0.86]

Comparison 8. Standardized effect estimates for other health related outcomes following rehousing or retrofitting with or without neighbourhood renewal (post-1995) (non-experimental studies)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Smoker	1		Odds Ratio (Random, 95% CI)	1.47 [0.85, 2.55]
2 Heavy drinker	1		Odds Ratio (Random, 95% CI)	0.61 [0.30, 1.24]
3 < 5 portions of fruit/veg per day	1		Odds Ratio (Random, 95% CI)	0.79 [0.52, 1.21]
4 Chronic illness (children)	1		Odds Ratio (Random, 95% CI)	1.04 [0.55, 1.97]
5 Headaches (children)	1		Odds Ratio (Random, 95% CI)	0.99 [0.60, 1.63]
6 Indigestion (children)	1		Odds Ratio (Random, 95% CI)	0.94 [0.06, 15.27]
7 Sleeping problems (children)	1		Odds Ratio (Random, 95% CI)	1.13 [0.62, 2.06]
8 Eczema (children)	1		Odds Ratio (Random, 95% CI)	1.15 [0.68, 1.93]
9 Hay fever (children)	1		Odds Ratio (Random, 95% CI)	0.99 [0.51, 1.91]
10 Pain & discomfort	1		Odds Ratio (Random, 95% CI)	0.40 [0.17, 0.94]
11 Limitations to mobility	1		Odds Ratio (Random, 95% CI)	0.53 [0.22, 1.32]

Comparison 9. Standardized effect estimates for mental health outcomes following rehousing from slums (pre-1975) (non-experimental studies)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Nervousness	1		Odds Ratio (Random, 95% CI)	1.16 [0.89, 1.50]
2 Negative mood	1		Odds Ratio (Random, 95% CI)	0.91 [0.70, 1.18]
3 Dissatisfaction with status quo	1		Odds Ratio (Random, 95% CI)	0.86 [0.66, 1.12]
4 Potency (nothing can be done to improve situation)	1		Odds Ratio (Random, 95% CI)	0.81 [0.63, 1.06]
5 Pessimism	1		Odds Ratio (Random, 95% CI)	0.81 [0.63, 1.06]
6 Emotionality (unable to control temper)	1		Odds Ratio (Random, 95% CI)	0.80 [0.61, 1.03]

Comparison 10. Standardized effect estimates for disability following rehousing from slums (pre-1975) (non-experimental studies)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 At least one day of disability	1		Odds Ratio (Random, 95% CI)	1.14 [0.98, 1.34]

ADDITIONAL TABLES

Table 1. Details of excluded studies (n=36) (ordered by intervention category and alphabetically)

Author, publication year, country,	Study design, final sample size	Intervention summary	Reason for exclusion
Warmth and energy efficiency improvements (after 1980)			
Caldwell 2001, UK	Controlled before & after Final/Baseline sample: 412/929 (43%)	Thermal improvements according to need, they included heating, windows, cavity wall, insulation, fabric repair, re-roofing, loft insulation, external cladding, re-rendering, controlled entry, humidistat fans, close painting, new, balcony rail, new doors, rewiring, new flooring, backcourt lighting	No health outcome data reported, health service use only
El Ansari 2008, 2008, UK	Cross-sectional controlled before & after (routine data)	Assessment for eligibility for warmth grant- unclear what improvements implemented	Area level data, unclear proportion exposed to housing improvement
Warm Front 2008, UK	Retrospective cross sectional controlled Final sample: 2180 indivduals	Grants for insulation (cavity wall and/or loft) draught proofing, hot water tank jacket , and/or central heating, and minor measures, heating repair, energy efficient light bulbs, security measures (up to total value of £2,700)	Direct health outcome assessed but no assessment of change following intervention
Green 1999 UK	Retrospective controlled Final sample: 205 households	Replacement of underfloor electric heating with a small gas-fired central heating plant piping hot water to each apartment, improved insulation, each towerblock was encased in a mineral wool insulation material, with an outer skin of rainscreen cladding using an aluminium cassette-type system. Open balconies were enclosed with glass, new ventilation system to replace vitiated air and remove moisture laden air while minimising heat loss and avoiding draughts. Plus substantial improved security measures	Direct health outcome assessed but no assessment of change following intervention
Heyman 2011, UK	Randomised controlled trial Final/Baseline sample:	Loft insulation (54%), cavity wall insulation (53%) , draught exclusion (29%), heating controls (20%) , central heating (13%), and other measures (not	No data reported

Table 1. Details of excluded studies (n=36) (ordered by intervention category and alphabetically) (*Continued*)

	140/237 (59%)	specified)	
Jackson 2011, New Zealand	Cross sectional controlled before & after Final sample: 9,702	Insulation (26.5%) & ventilation (43.5%) improvements, improved heating system (4.4%), extensions (8.7%), plus housing and health advice, improved links with health and other support agencies	Health service use only
Jones 1999, UK	Case control	No discrete programme of housing improvement: moving house and changes in heating system	Case control study
Keatinge 1989, UK	Case control	No discrete programme of housing improvement: use of domestic heating	Case control study
Roder 2008, Canada	Retrospective uncontrolled Final/Baseline sample: 26/9 (34.5%)	Energy Effciency using "Green indicators" - size according to occupational requirements; heating and cooling efficiency, indoor air quality and resource efficiency (water, electricity)	No data reported
Telfar-Barnard 2011. New Zealand	Retrospective controlled Final sample: 973,710 individuals	Funding for insulation retrofits and clean, efficient heating grants	Direct health outcome assessed but no assessment of change following intervention
Winder 2003, UK	Uncontrolled before & after Final/Baseline sample: 72/210 (34.3%)	Installation of central heating and insulation measures for elderly (70+ years)	No data reported
Rehousing/refurbishment +/- neighbourhood regeneration/relocation			
Smith 1997, UK	Retrospective controlled Final sample: 538 individuals	Medical priority rehousing	Direct health outcome assessed but no assessment of change following intervention
Walker 1999, UK	Cross sectional controlled before & after Final sample: 2 primary care practices with reference practices	Housing led neighbourhood regeneration. Homes renovated with additional improvement to physical and social neighbourhood environment	Health service use only
Woodin 1996, UK	Retrospective uncontrolled Final sample: 112 households.	Mix of neighbourhood and housing renewal. Original housing demolished and replaced with new stock	Health Service use only

Table 1. Details of excluded studies (n=36) (ordered by intervention category and alphabetically) (*Continued*)

Provision of basic housing needs/low or middle income country intervention			
Sedky 2001, Pakistan	Cross-sectional controlled before & after Final sample: 1,359	Installation of roof hatch windows, wall and roof insulation, double glass windows, stove with water warming facility	Direct health outcome assessed but no assessment of change following intervention
Aiga 2002, Philippines	Cross-sectional controlled before & after Final/Baseline sample: 370/402 (92%) households	Provision of private water faucet (with meter) and private toilet, electricity, paved roadways to every household	Direct health outcome assessed but no assessment of change following intervention
Bailie 2012, Australia	Uncontrolled before & after Final sample: 418 children	Unclear. New houses built in each community, mean 10 new houses in each community of around 66 houses. Study sample does not distinguish between those living in new houses and those who are not	Unclear what housing intervention comprised and who received it, less than 17% sample received intervention
Butala 2010, India	Case control Final sample: unclear	No discrete programme of housing improvement	Direct health outcome assessed but no assessment of change following intervention
Cattaneo 2007, Mexico	Retrospective controlled Final sample: 2783 households	Replacing mud floors (up to 50sqm) with cement floors.	Direct health outcome assessed but no assessment of change following intervention
Choudhary 2002, India	Retrospective controlled Final/Baseline sample: 365/373 (98%)	Provision of plot for families previously living in temporary shanty town housing to build own house. Involved relocation to non-shanty area nearby- new houses built were permanent structures of brick & cement	Direct health outcome assessed but no assessment of change following intervention
Meddings 2004, Afghanistan	Case control Final sample 1863 individuals	No discrete programme of housing improvement: latrine improvement	Case control study

Table 1. Details of excluded studies (n=36) (ordered by intervention category and alphabetically) (*Continued*)

Pholeros 1993, Australia	Cross-sectional uncontrolled before & after Final sample: area clinic data n=71 records, 11 houses in study area	Repair and maintenance training in relation to health hardware in house (power, water, cleaning, dust control). Included installation of showers, electrical upgrades, stove replacement, promoting healthy living practices (washing people/clothes, removing waste, improving nutrition, reducing overcrowding, separating dogs and children, controlling dust, temperature control, reducing trauma/accidents). Also provision of shampoo/soap, nutritional programme Outdoor housing conditions: fences around houses, stress, improvement to wet area outside house. Different aspects of the improvements were carried out through out the year (i.e. not all at once). Some people may have been rehoused (unclear)	Health service use only
Wolff 2001, Malawi	Cross sectional controlled before & after Final sample: 529	Rehousing from 2 room tra- ditional mud house with thatched roof and hard packed mud floors to 3 room house with fired mud bricks, tiled roof, concrete foundation (10 year interest free loan to buy house US$550/ UK£370). New houses built by householder and other community members	Direct health outcome assessed but no assessment of change following intervention

Table 1. Details of excluded studies (n=36) (ordered by intervention category and alphabetically) (*Continued*)

Vyas 1998, India	Case study Final sample unclear	Case study of a "Habitat Improvement Programme"	Insufficient information available- author contacted but no response
Rehousing from slums (before 1970)			
Ferguson 1954, UK	Retrospective cross-sectional controlled Final sample: 1,106 households	Rehousing slum dwellers to new build, vermin free housing with own water supply. 56% of new houses had sole use of lavatory	Direct health outcome assessed but no assessment of change following intervention
Wambem 1973, USA	Cross-sectional controlled before & after Final sample: 107 individuals	Rehoused from substandard wooden framed housing in serious need of repair with inadequate sewage and solid waste disposal to new build public housing in planned housing project. New houses were of stucco construction set on landscaped grounds, with paved streets, sidewalks and street lighting, have gas heat and modern kitchens, plumbing and sewage facilities, weekly refuse removal	Health Service use only
Other categories			
Burr 2007, UK	Randomised controlled trial Final sample: 182 indidivudals	Mould removal with fungicide and installation of positive input ventilation fan installed in loft of house	Excluded intervention
Allen 2011, Canada	Randomised controlled crossover trial Final sample: 45 individuals (25 households)	Portable air filters	Excluded intervention
Coggon 1991, UK	Case control study Final sample: 1865 individuals	No discrete programme of housing improvement: sanitary improvements	Case control study

Table 1. Details of excluded studies (n=36) (ordered by intervention category and alphabetically) (*Continued*)

Eick 2011, UK	Randomised controlled trial Final sample: 49 households (withdrawals unclear).	Mechanical Ventilation Heat Recovery (MVHR) , also central heating. Data only reported for MVHR	Excluded intervention
Kahlmeier 2001, Switzerland	Retrospective uncontrolled Final sample: 3870 individuals	No discrete programme of housing improvement: house move	Participants not part of discrete housing improvement intervention
Kovesi 2009, Canada	Randomised controlled trial Final sample: 1344 individuals	Installation of mechanical ventilation and heat recovery	Excluded intervention
Marsh 1999, UK	Longitudinal lifecourse survey data Final sample: 9848 individuals	No discrete programme of housing improvement: house move	Retrospective analysis- participants not part of discrete housing improvement programme
Warner 2000, UK	Randomised controlled trial Final sample: <40 households	Installation of mechanical ventilation and heat recovery	Excluded intervention
Westaway 2007, South Africa	Unclear design & sample Baseline sample ~371 individuals	Unclear	Insufficient information available- author contacted but no response
Wright 2009, UK	Randomised controlled trial Final sample: 101 individuals	Installation of mechanical ventilation and heat recovery systems	Excluded intervention

Table 2. Summary of quality assessment of qualitative studies (ordered by intervention category and publication year)

Author, Country, Year	Sample Size	Are the research questions clear?	Are the research questions suited to qualitative enquiry?	Are the following described?			Are the following appropriate to the research question?			Are the claims made supported by sufficient evidence?	Does the paper make a useful contribution?
				Sampling	Data Collection	Analysis	Sampling	Data Collection	Analysis		

Table 2. Summary of quality assessment of qualitative studies (ordered by intervention category and publication year) (*Continued*)

Warmth and energy efficiency improvements (after 1980)											
Basham et al, UK, 2004 Supplementary to quantitative data (Barton 2007, included in synthesis)	12- also interviewed pre intervention	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Caldwell et al, UK, 2001 Supplementary to quantitative data (Caldwell 2001, excluded from synthesis)	6 focus groups-total numbers not reported	Yes	Yes	Unclear	Yes	No	Unclear	Yes	Unclear	Yes	Yes
Gilbertson et al, UK, 2006 Supplementary to quantitative data (Warm Front 2008,	49 households + 16 refusal	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2. Summary of quality assessment of qualitative studies (ordered by intervention category and publication year) (*Continued*)

excluded from synthesis)											
Harrington et al, UK, 2005 Supplementary to quantitative data (Heyman 2001, excluded from synthesis)	30 (only 17/30 had intervention- all 17 in fuel poverty prior to intervention)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rugkasa et al, Ireland, 2004 Supplementary to quantitative data (Shortt, 2007, included in synthesis)	9 + focus group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rehousing or retrofitting with or without neighbourhood renewal (after 1995)											
Bullen, New Zealand, 2008 Supplementary	30 interviews with householders, also 19 inter-	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2. Summary of quality assessment of qualitative studies (ordered by intervention category and publication year) (*Continued*)

to quantitative data (Jackson 2011, excluded from synthesis)	views with housing providers										
Ellaway et al, UK, 2000 Qualitative data only	28 (16 improved, 12 unimproved flats)	Yes	Yes	No	Yes	No	Unclear	Yes	Unclear	Yes	Yes
Gibson et al, UK, 2011 Supplementary to quantitative data (Kearns 2008, included in synthesis)	22/60 contacted	Yes	Yes	Yes	Yes	Yes	Unclear	Yes	Yes	Yes	Yes
Rogers, UK, 2008 Supplementary to quantitative data (Thomas, 2005, included in synthesis)	20 in depth interviews, and 200 brief interviews	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2. Summary of quality assessment of qualitative studies (ordered by intervention category and publication year) (*Continued*)

Studies excluded from synthesis due to poor quality of data											
Warmth and energy efficiency improvements (after 1980)											
Decent Homes, UK, 2012 Qualitative data only	6	No	Unclear	No	No	No	Unknown	Unclear	Unclear	Unclear	No
Allen, UK, 2005 Supplementary to quantitative data (Allen 2005a, included in synthesis)	16	No	Unclear	No	Yes	No	Unclear	Yes	Unclear	Yes	Unclear
Rehousing or retrofitting with or without neighbourhood renewal (after 1995)											
Kearns et al, UK, 2006 Supplementary to quantitative data (Kearns 2008, included in synthesis)	28	No	Unclear	No	Yes	No	Unclear	Yes	Unclear	Yes	Unclear

Table 3. Summary of reported qualitative data and study characteristics (ordered by intervention category and year of publication)

Author, Publication Year, Country, Reference	Sample	Data collection methods and details of intervention	Overview of findings:
Intervention Category: Warmth and energy efficiency improvements (after 1980)			
Basham et al, 2004, UK Supplementary to quantitative data (Barton, 2007, included in synthesis)	Aim of qualitative data: 1. To promote understanding of the wider social issues of living in cold households and then warmer ones by assessing: energy use, methods of payments and costs; use of the house, the well-being of residents and relationships within the household and beyond; respondents' perception of their dwelling and area 2. To provide evidence to inform housing improvement strategy by assessment of: the factors influencing energy use of the household; residents' knowledge of how to operate the heating system efficiently and effectively, and their perception of the importance of ventilation to the indoor environment Sample Selection: Sub-sample of housing project where people who had no central heating now had it installed Sample Size: 12- also interviewed pre intervention Description of intervention: Warmth- central heating Time since intervention: 1-6 months	Data collection method: In-depth interviews Details of analysis: Grounded theory- thematic analysis Year of interviews: 2002-2003	Householders reported using more of the house which was warmer and drier. Cost remained an issue but varied in importance. Opportunities for leisure and study improved, there was increased motivation to maintain the house and this resulted in more social interaction. There was a perceived improvement in relationships and health. There were also issues around the communication between householders, contractors and housing managers: information on the new systems was variable, the relationship between tenants and contractors reflected the residents' status as tenants
Caldwell et al, 2001, UK Supplementary to quantitative data (Caldwell 2001, excluded	Aim of qualitative data: To gather data on residents attitudes and feelings Sample Selection: Randomly	Data collection method: Focus groups	Residents pleased with improvements. Improved view of home, increased use of space due to warmth, improved family relationships, increased feel-

Table 3. Summary of reported qualitative data and study characteristics (ordered by intervention category and year of publication) (*Continued*)

from synthesis)	from list of intervention and control groups by local authority Sample Size: 6 focus groups- total numbers not reported Description of intervention: Warmth measures- varied depending on baseline condition of house Time since intervention: 2-4 years	Details of analysis: Not reported Year of interviews: 1998	ings of privacy due to increased usable space. Some reports of better quality of diet, explained partly by more money available for food (presumably related to increased fuel efficiency and reduced fuel bills but not clearly stated- quantitative data reports significant reductions in fuel bills among intervention group) and kitchen improved and people more inclined to spend time in kitchen preparing food. Residents in the control group reported feeling let down, resentful, tense and stressed but were hopeful that they would benefit from similar improvements soon. For both groups other problems remained such as the need for improvements to the neighbourhood and immediate external housing environment was reported. Feelings of insecurity included issues such as threat of crime, violence and drugs
Gilbertson et al, 2006, UK Supplementary to quantitative data (Warm Front 2008, excluded from synthesis)	Aim of qualitative data: Assess change in householder's perceptions and behaviours following intervention, satisfaction with intervention, perceived changes in health and well-being, changes in use of living space and social interaction Sample Selection: Purposive to represent 5 intervention areas, half with family member 60+ yrs; half with children under 16yrs. All had high level intervention i.e. installation/upgrade of heating or insulation Sample Size: 49 households + 16 refusal Description of intervention:	Data collection method: Semi-structured interviews carried out by 4 interviewers Details of analysis: Grounded theory- thematic analysis checked and agreed by co-interviewers Year of interviews: 2003	Most householders reported improved and more controllable warmth and hot water. Many also reported improved physical health and comfort, especially of mental health and emotional well-being and, in several cases, the easing of chronic illness symptoms. There were reports of improved family relations, an expansion of the domestic space used during winter months, greater use of kitchens and improved nutrition, increased privacy, improved social interaction, and an increase in comfort and atmosphere within the home. Greater warmth and comfort also enhanced emotional secu-

Table 3. Summary of reported qualitative data and study characteristics (ordered by intervention category and year of publication) (*Continued*)

	Warmth- Insulation and heating installation/upgrade Time since intervention: About a year		rity, and recipients were more content and at ease in their homes. There was little evidence of substantially reduced heating bills. Authors conclude: Intervention was accompanied by appreciable benefits in terms of use of living space, comfort and quality of life, physical and mental well-being, although there is only limited evidence of change in health behaviour. Some reports (around quarter of residents) reported long term negative effects of disruption, lack of control/powerlessness over move and what was done to their house. Most residents (around 2/3) found reported disruption and lack of control and found it tolerable and short lived
Harrington et al, 2005, UK Supplementary to quantitative data (Heyman 2011, excluded from synthesis)	Aim of qualitative data: Explore experiences and nature of fuel poverty and relationship to health, and responses to fuel poverty interventions Sample Selection: Random selection- 10 refusals Sample Size: 30 (only 17/30 had intervention- all 17 in fuel poverty prior to intervention) Description of intervention: Warmth- Tailored intervention to alleviate fuel poverty- loft insulation, cavity wall insulation, draught exclusion, central heating and other measures (average £727, up to £3335) Time since intervention: 6-9 months	Data collection method: Semi-structured interviews Details of analysis: Grounded theory- descriptive open coding Year of interviews: 2000-2002	Some respondents were unable to comment on the benefits of the intervention as they had not experienced a winter with the intervention. Suggestion from quantitative data that benefits of intervention were in reduced fuel bills rather than in increased warmth. Some reports in qualitative data that the intervention increased the effective size of the living area during cold weather. Authors conclude that fuel poor households may assume that heating systems are costly and inefficient and attention is directed towards living with heating system insufficiency. Better understanding of the supply side (of the benefits of cavity wall insulation and of how to use heating controls) would allow the same limited resource to go further

Table 3. Summary of reported qualitative data and study characteristics (ordered by intervention category and year of publication) (*Continued*)

Rugkasa et al, 2004, Ireland Supplementary to quantitative data (Shortt, 2007, included in synthesis)	Aim of qualitative data: To further explore how the intervention impacted on residents' lives asking about subjects not raised in the quantitative survey Sample Selection: Only those with the full intervention Sample Size: 9 + focus group Description of intervention: Warmth- heating/ insulation upgrade/installation Time since intervention: 1-3.5 years	Data collection method: In-depth interview and one focus group Details of analysis: Content and thematic analysis-'data validation followed established academic procedures' Year of interviews: 2003	High levels of satisfaction with intervention. Homes now easier to heat especially for older people who previously found it physically difficult to carry coal. People enjoyed warm home and some reports of this making people feel better mentally and physically
Intervention Category: Rehousing/refurbishment +/- neighbourhood regeneration +/- relocation			
Bullen, 2008, New Zealand Supplementary to quantitative data (Jackson 2011, excluded from synthesis)	Aim of qualitative data: Investigate how housing providers and householders responded to an intervention that addresses the dynamism of the physical and social aspects of housing Sample Selection: Criteria used to select sample to allow comparisons of location, extent & time since intervention- unclear if this was achieved, 24/30 households were known as "successful intervention" cases Sample Size: 30 interviews with householders, also 19 interviews with housing providers Description of intervention: Insulation, ventilation, heating, or extension to existing house or new house; referral to health and social agencies Time since intervention: Between 1 and 6 years	Data collection method: In-depth interviews by ethnically matched interviewer Details of analysis: Inductive analysis to identify and compare emergent themes by location, extent and time since intervention Year of interviews: 2004-2007	Most participants, even those with the minimal insulation/ ventilation intervention, reported improvements in health and wellbeing. Most commonly there was an increased sense of empowerment, reduction in illnesses (in particular asthma), improved comfort in the home, improved family functioning and a heightened sense of social wellbeing. The strongest link between the programme and health was reduced stress, increased happiness and increased connection with family. Reports of improved wellbeing were linked to tangible housing improvements in particular additional space and improved thermal comfort. Those who had benefited from structural changes and increased space reported the greatest benefits, in particular improved family relations,

Table 3. Summary of reported qualitative data and study characteristics (ordered by intervention category and year of publication) (*Continued*)

			privacy, a more peaceful environment, reduced household mess and increased house pride. There were also reports of increased ability to invite people into their own homes to socialise. Increased space outside was reported to provide safe play areas for children, and there was some suggestion that the improved indoor environment facilitated studying/ completing homework for both school children and adults. These positive impacts on family functioning and daily life were linked by residents to improvements in psycho-social wellbeing. For residents with disability needs homes were redesigned to facilitate wheelchair- this was reported to make a big difference to those residents. Obstacles reported by residents to limit the potential benefit were: poor quality modifications, inadequate warmth improvement, increased housing costs (due to increased housing size- rent & fuel bills), inadequate drainage and fencing in outdoor areas Additional data available in two earlier reports reflect the findings reported in this paper
Ellaway et al, 2000, UK Qualitative data only	Aim of qualitative data: Explore residents views of the possible health impact of recent housing improvements Sample Selection: Volunteers selected by housing agency Sample Size: 28 (16 improved,	Data collection method: Interview Details of analysis: Not reported Year of interviews: 1999	Tenants in improved/new housing reported reduced coughs and use of inhalers among children, and less use of tranquilisers, reduced smoking and improved diet. Also reported that they felt better about life and had more money available due to reduced fuel bills. Other re-

Table 3. Summary of reported qualitative data and study characteristics (ordered by intervention category and year of publication) (*Continued*)

	12 unimproved flats) Description of intervention: Refurbished tenemental flats, new build terrace/ semi-detached housing Time since intervention: 2-7 years		ports of links between housing and health included issues of drug users living next door and the general quality of the local environment as well as indoor housing conditions
Gibson et al, 2011, UK Supplementary to quantitative data (Kearns 2008, included in synthesis)	Aim of qualitative data: To explore the impacts of housing and area change on a range of health, community, and social outcomes from the perspectives of the respondents Sample Selection: Purposive to represent different age groups and a mix of tenants who had moved within same area and others who had been relocated to a new area Sample Size: 22/60 contacted Description of intervention: Rehousing into new-build social rented homes Time since intervention: 3.5-5 years	Data collection method: Semi-structured interviews Details of analysis: Thematic analysis- examination of themes, sub-themes, and relationships between and within themes Year of interviews: 2007-2008	Residents reported high levels of housing satisfaction, and benefits of improved warmth as well as reduced problems of noise. Some residents linked these improvements to improved mental health and well-being. Housing type had changed with many (13/22) participants moving from a flat to a house with a private garden; this was associated with increased privacy and reduced exposure to anti-social behaviour. There were some reports of improved physical health for those who had moved to a dwelling more appropriate to their mobility needs, sometimes this involved downsizing from a house to a flat. No clear reports of changes in health behaviour were linked to rehousing. Reports of changes in sense of community and neighbourliness varied, this appeared to depend on an individual's interest in socialising with neighbours
Rogers, 2008, UK Supplementary to quantitative data (Thomas, 2005, included in synthesis)	Aim of qualitative data: To obtain further details about subjective views of the locality, effects of urban regeneration programme, psychosocial well-being and perceptions of mental health Sample Selection: Purposive sample to identify those with	Data collection method: In-depth interviews Details of analysis: Thematic analysis Year of interviews: Unclear	Range of factors reported to influence mental health, these included factors of service provision, employment opportunities and exposure to anti-social behaviour. This was interpreted as implying that the local area and therefore changes in the local area may have an impact on mental health by be-

Table 3. Summary of reported qualitative data and study characteristics (ordered by intervention category and year of publication) (*Continued*)

	significant changes in mental health Sample Size: 20 in depth interviews, and 200 brief interviews Description of intervention: Urban regeneration programme Time since intervention: Unclear		ing a key location for opportunities and threats which affect vulnerability to poor mental health. Ambivalence regarding the experienced and perceived benefits of housing improvement, provision of employment and leisure opportunities. Favourable perception of improved transport. Concern about lack of social control in locality ("nuisance families", vandalism, gangs, threatening behaviour), lack of faith in agencies to make changes considered important to residents, restricted opportunities and entrapment
Studies excluded from synthesis due to poor quality of data (see table regarding qualitative data prompts)			
Intervention Category: Warmth and energy efficiency improvements (after 1980)			
Decent Homes, 2012, UK Qualitative data only	Aim of qualitative data: Not stated but appears to be to gather residents views of changes	Data collection method: In-depth interviews Details of analysis: Not reported Year of interviews: ?2010	Residents reported improvements in warmth, safety of heat source (i.e. not open gas fires) , and reduced draughts. There were also reports of improved health. Previous poor health related to housing conditions was reported by residents to be made worse by housing conditions rather than directly caused by housing conditions
Allen, 2005, UK Supplementary to quantitative data (Allen 2005a, included in synthesis)	Aim of qualitative data: Not reported Sample Selection: Not reported Sample Size: 16 Description of intervention: Warmth- Various- central heating installation/repair, plus general repairs plus health, housing and benefits advice Time since intervention: esti-	Data collection method: Semi-structured interviews Details of analysis: Not reported Year of interviews: 2003-2004	No clear reports of health improvement linked to housing improvement. However, clear benefits more generally of housing improvements were reported with added value of benefits advice and general project support. Author concludes that 'how' the intervention was implemented seems to be as important as the intervention itself and that this may explain why there is no relationship emerg-

Table 3. Summary of reported qualitative data and study characteristics (ordered by intervention category and year of publication) (*Continued*)

	mate 1 year		ing between the intervention and a detectable health impact
Intervention Category: Rehousing/refurbishment +/- neighbourhood regeneration +/- relocation			
Kearns et al, 2006, UK Supplementary to quantitative data (Kearns 2008, included in synthesis)	Aim of qualitative data: Not reported. Open ended questions on recent changes, view of new house, relationships with neighbours, health and wellbeing, strength of attachment to area and wish list of changes to new house Sample Selection: Not reported- moved to new house 1-3 years previous Sample Size: 28 Description of intervention: Rehousing Time since intervention: 12-34 months	Data collection method: In-depth interviews Details of analysis: Not reported Year of interviews: 2004-2005	Having more space was welcomed and linked to improved family living relations and decreased stress. Growing sense of community and attachment to the neighbourhood reported, evidenced by reports of looking out for neighbours and keeping the area well maintained. The process of moving was reported to be unproblematic for most people. But some building delays were associated with distress and expense. Following some difficult periods of settling into a new area, most people were pleased with their new house despite leaving an area where they had strong ties. Residents reported increased pride in their homes and feelings of safety, also that the new houses provided a calming and relaxing home atmosphere. There was little evidence of change in lifestyles

Table 4. Intervention & Population details: Warmth and energy efficiency improvements (after 1980)

Author, Year, Country	Intervention	Study population
Warmth and energy efficiency improvements (after 1980)		
Allen 2005a UK	Central heating installation/repair, plus general repairs (including roofing/guttering), improved bath/shower access, plus health, housing and benefits advice	Owner occupiers (94%) with diagnosed serious heart condition. 60% <65 years, 80% lived in home >10 years, 62% Asian, 60% dependant on benefits
Allen 2005 UK	Heating installation/repair (n=20), reroofing (n=2), replacement windows (n=31), ventilation for those with asthma (n=28), intruder alarm (n=3), general home repair plus health and benefits advice	Residents vulnerable to poor housing referred for health reasons to project (referral criteria- coronary heart disease, cerebrovascular accident, peripheral vascular dis-

Table 4. Intervention & Population details: Warmth and energy efficiency improvements (after 1980) (*Continued*)

		ease, type II diabetes with functional difficulties, chronic obstructive pulmonary disease, asthma children with complex and life limiting diseases). All income derived from welfare 46%, 83% of Pakistani origin
Barton 2007 UK	Upgrading heating provision and energy efficiency according to need. Included installation of full gas fired central heating, upgrading of partial heating and/or renewal of undersized boilers. Installation of extract fans controlled by ambient temperature and humidity. For some houses, roofs were fitted with breathable roofing felt, plus 50mm insulation, Cavity insulation with rockwool fibres, and double glazing. Over ceiling insulation topped up to 200mm (glass fibre quilting), Front and back doors and French windows were replaced with uPVC doors	Social housing tenants in deprived area (Jarman index of socio-economic deprivation 22.7- regional level of 12.8 (Devon)). 58% <20 years, 10% <50 years
Braubach 2008 Germany	Thermal insulation and where required central heating and energy efficient window replacement	Residents of social housing in three neighbourhoods of Frankfurt. Mean age 46 years (range 1-97; 1-17 years 13%, 18-64 years 60%, >64 years 27%); Male/Female 42%/58%. Mix of low and middle income households
CHARISMA 2011 UK	Provision of ventilation (VentAxia HR200XL) and where required improved or replaced central heating tailored to household. Ventilation device delivers filtered fresh air to first floor bedrooms, and removes stale air, replacing moist air with fresh air. System as 70% heat recovery and costs around £15 annually to run	Children aged 5-14 years prescribed >2 steroid inhalers in past year
Health Action Kirklees UK	Installation of heat recovery unit and insulation measures (cavity wall insulation, loft insulation (full or top up), hot water tank jacket and draught proofing)	Private householders, under 60 years/with young children/not in receipt of welfare, who suffer from or are at risk from cold related illness (confirmed by health professional)
Hopton 1996 UK	Improved heating. Heat-with-rent controlled heating central heating system for every room in house, and responds to external temperature. Tenants pay a fixed sum which is incorporated into their rent	Social housing tenants in isolated deprived neighbourhood: 42% household with someone unemployed

Table 4. Intervention & Population details: Warmth and energy efficiency improvements (after 1980) (*Continued*)

Howden-Chapman 2007 New Zealand	Ceiling insulation, draught-proofing of windows and doors, sisalated paper (insulated foil) strapped under floor joists, and polyethylene covering over the ground	Various tenures (24% rented, 76% owner occupier- nationally 32%/68%). At least one person in household suffered from respiratory disease, lived in uninsulated house. 66% in bottom 3 deciles of deprived areas. Ethnicity: 49% Maori migrant pacific. 66% in bottom 3 deciles of deprived areas
Howden-Chapman 2008 New Zealand	Replacing 2kW electric heaters or portable unflued gas heaters with ≥4kW non-polluting alternative Choice of 3 heaters: 131 (73.6%) heat pump, 39 (21.9%) wood pellet burner or 5 (2.8%) flued gas heater. (No indication of proportion of each intervention by Int & Cont group). All homes were (where necessary) brought up to the NZ building code standard before baseline data collection	Four New Zealand cities. Households with child (6-12 years) with Dr diagnosed asthma in house with main form of heating plug in heater or unflued LPG heater. Mean age 9.6 years, ~58.5% male, ~36.5% Maori (compared to 15% general population), 47% NZ European Int/Cont
Iversen 1986 Denmark	Replacement windows	Private low-rise flatted housing in middle income area
Lloyd 2008 UK	Insulation (double skinning of walls) and draught proofing, gas central heating, double glazing, solar panels, dual-purpose heat recovery system, and front and back verandahs within internal living area of the flat	Social housing tenants in deprived neighbourhood
Osman 2010 UK	Replacement/upgrade of central heating, installation of loft, under-floor and cavity wall insulation, and welfare benefit reassessment	Elderly people with recent hospital admission for COPD living in own homes (47% social housing)
Platt 2007 UK	Installation/repair/upgrading of central heating (choice of gas/electric/oil/solid fuel) plus insulation (where possible cavity wall fill, lagging of boiler pipes, loft insulation, draft exclusion measures), safety alarms where appropriate (carbon monoxide detector, smoke alarm, cold alarm), advice on energy use, and benefit entitlement check offered	Social housing tenants (53.5%) and owner-occupiers (41.5%). Mean age 62 years, Male/Female 36%/64%, socio-economically deprived 61%, predominantly pensioners with no children in house
Shortt 2007 Northern Ireland	Energy efficiency measures: included central heating, insulation and/or provision of new electrical appliances. Also promotion of benefit uptake for whole area (Int & Cont)	High percentage >60 years and <5 years. High proportion owner occupiers/private rented housing in rural areas, in receipt of welfare benefits. 78% Int group houses built pre-1950. Low uptake of domestic energy efficiency improvements; Areas in middle range of deprivation index

Table 4. Intervention & Population details: Warmth and energy efficiency improvements (after 1980) (*Continued*)

Somerville 2000 UK	Grant up to £2,500 to improve heating and reduce damp and mould growth in house, intervention agreed according to need. (Gas central heating, n=28 (47%), electric storage heater, n=22 (37%), solid fuel central heating, n=7 (12%), oil-fired central heating, n= 2 (4%))	Asthmatic children under 16 years living in social housing reported to have damp

Table 5. Intervention & Population details: Rehousing or retrofitting with or without neighbourhood renewal (after 1995)

Author, Year, Country	Intervention	Study population
Rehousing or retrofitting with or without neighbourhood renewal (after 1995)		
Ambrose 2000 UK	Rehoused to better accommodation, or had existing accommodation improved plus neighbourhood improvements (Single Regeneration Budget) plus other employment and education initiatives related to regeneration programme	Social housing tenants. High levels of socio-economic deprivation (in receipt of income support 65. 4%; unemployed 9.2%). Bangladeshi 69.2%, White 18.7%
Barnes 2003 UK	Refurbishment or rehousing (some included warmth improvements)	Social housing tenants. Mixed age groups, 32% have some form of disability. Ethnicity: 65% White; 23% Black/Asian
Blackman 2001 UK	Refurbishment or demolition of void dwellings, discretionary renovation grants for individual dwellings, heating and security improvements. Landscaping, environmental improvements- security and road safety measures (traffic calming), footpath improvement	Residents of neighbourhood renewal area, mixed tenure (56.1% owner occupier; 29.6% social rented) , 41.8% in receipt of housing benefit/household with no wage earner; 73.5% 5 years or more lived at this address. 96.4% White; Male/Female 32%/68%; age 0-15 yrs 20.6%; age 16 to 64 yrs 67.5%; age 65+ yrs 12%; Household type (%) n=98 households; Adults plus children 36.1%; Non-pensioner adult(s) only 35.1%; 1+ pensioner household 28.9%
Breysse 2011 USA	Comprehensive programme of "green" interventions in a 3 building 60 unit apartment complex, the programme covered: integrated design process; location & neighbourhood fabric; site; water; conservation; energy conservation; materials & resources; healthy living environment; and operations management. Housing intervention included the following (as well as other components not described): installation of air handling units to duct fresh air to bedroom & living room (to comply with ASHRAE Standard 62. 2); mitigation of radon levels where necessary; use of low VOC products; no smoking in common areas; removal of carpets in wet rooms; installation of fans	Low income (annual household income $28,000) , minority ethnic groups (Adults: White-Hispanic 9%; White-nonHispanic 36%; African 32%, African-American 9%), 67% Female. 57% adults born outside USA

Table 5. Intervention & Population details: Rehousing or retrofitting with or without neighbourhood renewal (after 1995) (*Continued*)

	in kitchen & bathroom; installation of geothermal heating & cooling system; installation of high performance (U-value 0.32) windows; insulation to exterior walls (adding R-value 7.5 to existing R-value 11) and to roof; replacement water fixtures in kitchen & bathroom; installation of dual flush toilets & low water clothes washers	
Critchley 2004 UK	Low-income tenants moved from poor-quality (hard to heat with damp, mould & condensation problems reported to be highly prevalent) tower blocks to high-quality low-rise new build accommodation	Social housing tenants. Predominantly retired and dependent on welfare: 66% > 60 years
Evans 2000 UK	Renovation of housing, include installation of central heating and double glazing according to need	Private householders in socio-economically deprived urban area
Halpern 1995 UK	Housing refurbishment and neighbourhood regeneration. Some housing improvement and with major re-design of estate- to reduce traffic speed, improve visibility of parked cars	Social housing tenants. High number female single parent families; Mean age females interviewed at stage 1, 2 ,3 = 42.4, 39.8, 40.2 yrs respectively. Mean years at present house 8.2; mean number of children <14 years 1.4; 37% employed; mean reported household income £97-134/wk
Kearns 2008 UK	Rehousing into new build socially rented homes (considered to be upgraded conditions to previous homes) in 60 sites in Scotland (47% also relocated to different neighbourhood)	Social housing tenants. Age <30 yrs 15.8%; >60 yrs 14.4%; 77.9% urban resident, 21.4% rural resident
Molnar 2010	Moved to refurbished or new house (previously living in life threatening conditions)	Roma adults living in disadvantaged rural village. Previously living in life-threatening conditions
Thomas 2005 UK	Housing-led neighbourhood regeneration (Single Regeneration Budget) plus other employment and education initiatives related to SRB. Housing improvement mostly improvements to heating, bathrooms, kitchens and windows. Also transfer from housing ownership from local authority to housing trust	Social housing tenants in deprived area. Mean age Int/Cont 51/53, Male/Female 52%/48%
Thomson 2007 UK	Housing-led neighbourhood regeneration. Replacing ex-local authority owned social housing stock reported to have problems with damp and mould with new-build housing in the same locality. Accompanied by improvements in physical and social neighbourhood environment	Social housing tenants. More than half of participants were dependent on housing benefit
Wells 2000 USA	Rehousing (renovation of existing homes n=3) to improved housing with sufficient room. Participation required ability to pay mortgage and contribute	Families on fringe of home-ownership, in need of improved housing and willing to enter commitment of housing partnership including mortgage contri-

Table 5. Intervention & Population details: Rehousing or retrofitting with or without neighbourhood renewal (after 1995) (*Continued*)

	labour hours to house-building/renovation (around 400 hours per family)	butions. 74% female head of household; family size 2 to 8 persons. Mean income/month $1,396, mean income to needs ratio=1.10 (1.0=poverty line). Ethnicity: 61% African-American, 37% White; mean age 33 years

Table 6. Intervention & Population details: Provision of basic housing needs/low or middle income country intervention

Author, Year, Country	Intervention	Study population
Provision of basic housing needs/low or middle income country intervention		
Aziz 1990 Bangladesh	148 water hand-pumps (adding to existing 6 hand-pumps), household double pit water-sealed latrine, plus Hygiene education messages to promote water use and safe water sanitation practices delivered over two years	Children living in agricultural villages in rural Bangladesh. Household data: % Illiterate adults male/female 49/78, 77% Muslim
Rojas de Arias 1999	Two interventions: A- Modifying housing structure to ensure smooth, flat, and crack-free walls and ceiling surfaces and improving opening for ventilation and light. B- Insecticide spraying of house with Lambdacyhalothrin. One group received intervention A, one intervention B, and one intervention A & B	Rural households 50-100km from capital of Paraguay. Housing mainly made of mud walls and thatched roof
Spiegel 2003 Cuba	Repair of external housing e.g. leaking roofs, façade repair. Cheap materials provided for residents who want to carry out internal repairs themselves Wider neighbourhood improvements- repair of public buildings, streets, improvement of water supply & solid waste removal, installation of street lighting Social- new leisure/cultural venues and new social cultural activities (exercise groups, self-esteem groups for elderly, music clubs for youth etc)	Urban neighbourhood with predominantly dilapidated buildings and inadequate basic amenities such as potable water. Male/Female 41%/59%, mean age 45.1 years, education 11.2 years (mean), Ethnicity: White 58%, Mulatto/Black 36%

Table 7. Intervention & Population details: Rehousing from slums (before 1970)

Author, Year, Country	Intervention	Study population
Rehousing from slums (before 1970)		
Chapin 1938 USA	Rehousing and relocation from slum housing/neighbourhood to housing/neighbourhoods with slightly better living conditions	Residents of housing with inadequate facilities in neighbourhood with high crime rate. Many households foreign born with large families. Ethnicity: Black 62% Jewish 23% White 15%

Table 7. Intervention & Population details: Rehousing from slums (before 1970) *(Continued)*

McGonigle 1936 UK	Moved from slum housing estate (demolished) to new build houses on self-contained municipal housing estate	Residents of slum areas with higher mortality rates than rest of England and local borough; 18.75 & 22.15 deaths per 1,000 compared with 12.00 & 13.96
Wilner 1960 USA	Rehousing (moving into new public housing) with better facilities regarding water, heat, kitchen and toilet	Black families living in slum areas.

Table 8. Combined Risk of Bias & Hamilton Critical Appraisal (ordered by intervention category, study quality (Hamilton) and year of publication)

		Cochrane Risk of Bias												Hamilton Tool						
Author, publication year, country	Study Design, Sample Size (Int/Cont)	Random sequence generation (selection bias)	Allocation concealment (selection bias)	Blinding of participants and personnel (performance bias)	Blinding of outcome assessment (detection bias)	Blinding of analysts	Incomplete outcome data (attrition bias)	Selective reporting (reporting bias)	Baseline outcome characteristics similar	Baseline characteristics similar	Contamination	Baseline response	Implementation of intervention	Selection bias	Confounding	Withdrawa	Data collection	Blinding	**Overall Grad**	Performance bias
Intervention : Warmth and energy efficiency improvements (post 1980)																				
CHA 2011, UK	19/19*	L	L	?	?	L	?	L	L	?	?	H	H	C	A	A	A	B	**A**	C
Osman et al, 2010, UK	RCT 45/133	?	?	?	?	?	?	L	L	L	H	?	?	C	A	A	B	C	**A**	C
Howden-Chap	RCT 175/174	?	L	?	?	?	L	?	L	L	?	?	?	C	A	A	A	C	**A**	C

Table 8. Combined Risk of Bias & Hamilton Critical Appraisal (ordered by intervention category, study quality (Hamilton) and year of publication) (*Continued*)

man et al, 2008 New Zeala																				
Braul et al, 2008, Ger-many	CBA ~210/ 165	H	H	?	?	?	?	?	L	?	?	H	?	C	B	B	A	C	**A**	C
Bar-ton et al, 2007, UK	RCT 14/ 13	L	L	?	?	?	?	H	L	L	?	L	H	A	A	A	A	C	**A**	C
How-den-Chap man et al, 2007, New Zeala	RCT 1689/ 1623	?	L	?	?	?	L	?	L	L	?	?	H	C	A	B	A	C	**A**	C
Platt et al, 2007, UK	CBA 1281/ 1084	H	H	?	?	?	?	?	L	?	H	?	L	C	B	B	A	C	**A**	B
Lloyd et al, 2008, UK	CBA 9/ 27	H	H	?	?	?	?	?	L	?	?	L	?	B	C	C	B	C	**B**	C

Table 8. Combined Risk of Bias & Hamilton Critical Appraisal (ordered by intervention category, study quality (Hamilton) and year of publication) (*Continued*)

Short et al, 2004, Northern Ireland	CBA 46/54	H	H	?	?	?	?	?	H	H	?	?	H	C	C	B	A	C	**B**	C
Some et al, 2000, UK	UBA 72	H	H	H	H	H	?	?	H	H	H	L	?	B	C	B	A	C	**B**	B
Hopton & Hunt 1996, UK	CBA 55/77	H	H	?	?	?	?	?	?	?	?	L	?	A	C	C	B	C	**B**	C
Allen 2005, UK	UBA 16	H	H	H	H	H	?	?	H	H	H	H	H	C	C	C	A	C	**C**	C
Allen 2005, UK	UBA 24	H	H	H	H	H	?	?	H	H	H	?	H	C	C	C	A	C	**C**	C
Healt Action Calde Kirkl and Wake field, 2005, UK	RU 102	H	H	H	H	H	H	?	H	H	H	L	?	B	C	C	A	C	**C**	B

Table 8. Combined Risk of Bias & Hamilton Critical Appraisal (ordered by intervention category, study quality (Hamilton) and year of publication) (*Continued*)

Iverse et al, 1986, Den-mark	CBA 106/535	H	H	?	?	?	?	?	L	L	?	?	L	C	C	C	B	C	**C**	B
Intervention : Rehousing or retrofitting with or without neighbourhood renewal (post 1995)																				
Kearr et al, 2008, UK	CBA 262/284	H	H	?	?	?	?	?	L	H	?	?	?	C	C	B	A	C	**A**	C
Thon son et al, 2006, UK	CBA 50/50	H	H	?	?	?	?	?	L	L	?	H	L	C	B	B	A	C	**A**	B
Critcl ley et al, 2004, UK	CBA 246	H	H	?	?	?	?	?	?	?	?	H	L	C	B	B	A	C	**A**	B
Thon et al, 2005, UK	CBA 585/759	H	H	?	?	?	?	?	L	?	H	H	H	C	B	C	A	C	**B**	C
Barne 2003, UK	CBA 45/45	H	H	?	?	?	?	?	H	H	?	L	H	A	C	C	A	C	**B**	C
Evans et al,	CBA 17/17	H	H	?	?	?	?	?	?	?	?	?	H	C	B	C	A	C	**B**	C

Table 8. Combined Risk of Bias & Hamilton Critical Appraisal (ordered by intervention category, study quality (Hamilton) and year of publication) (*Continued*)

2002 UK																				
Breys et al, 2011, USA	RU 41	H	H	H	H	H	?	?	H	H	H	?	?	C	C	C	A	C	**C**	C
Molnar, 2010, Hungary	UBA 19/ 42	H	H	H	H	H	?	?	H	H	H	?	H	C	C	B	C	C	**C**	C
Blackman and Harvey, 2001, UK	UBA 166	H	H	H	H	H	?	?	H	H	H	L	?	B	C	C	A	C	**C**	C
Wells 2000, USA	UBA 31	H	H	H	H	H	?	?	H	H	H	H	L	C	C	B	A	C	**C**	B
Ambrose 1999, UK	UBA 227	H	H	H	H	H	?	?	H	H	H	L	H	A	C	C	A	C	**C**	C
Halp 1995, UK	XUB 27	H	H	H	H	H	?	?	H	H	H	?	H	C	C	C	A	C	**C**	C
Intervention : Provision of basic housing needs/low or middle income country intervention																				
Rojas de Arias 1999 Parag	CBA (3 inter- ven-	H	H	?	?	?	?	?	H	H	?	?	?	C	C	A	A	C	**B**	B

Table 8. Combined Risk of Bias & Hamilton Critical Appraisal (ordered by intervention category, study quality (Hamilton) and year of publication) (*Continued*)

	tions 229/ 260/ 132																			
Spieg et al, 2003, Cuba	XCBA 896/ 807	H	H	?	?	?	H	?	?	?	?	?	H	C	C	C	A	C	**C**	C
Aziz et al, 1990, Bangl	XCBA >200/ 200	H	H	?	?	?	?	?	L	L	?	?	?	C	B	C	B	C	**C**	B
Intervention : Rehousing from slums (before 1965)																				
Wiln et al, 1960, USA	CBA 1891/ 2893	H	H	?	?	?	?	?	?	?	?	?	L	B	B	A	A	C	**A**	B
Chap 1938, USA	UBA 23	H	H	H	H	H	?	?	H	H	H	H	L	C	C	A	B	C	**C**	B
Mc-Go-nigle & Kirby 1936, UK	XCBA 152/ 289	H	H	?	?	?	?	?	?	?	?	?	?	C	B	C	B	C	**C**	C

Study designs: RCT = randomised controlled trial; CBA = controlled before and after; XCBA = cross sectional controlled before and after; UBA = uncontrolled before and after; RU = retrospective uncontrolled. Risk of bias: H = high, L = low, ? = unclear
*only this sub-group of whole sample (n=89/89) who received warmth improvements, with controls matched for timing of intervention

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait

Author, publication year, country	Study design, final sample size, number and times of follow-up	Summary						
Summary of results		Selection	Confounding	Withdrawals	Data collection	**Overall grade**	No. of items at low Risk of Bias	Intervention integrity
Intervention : Warmth/energy efficiency improvements (post 1988)								
CHARISMA 2011, UK	Randomised controlled trial Final/ baseline: Sub group of 36 (Int/ Cont 19/ 19) at follow-up Int/Cont 88/89 12 months since baseline Twice: 3 & 11 months after intervention	C	A	A	A	**A**	5	C
	Health: Time I/II (4 months/12 months since baseline) Sub-group analysis by type of improvement: Mean difference adjusted for baseline (95% CI) Ventilation only (Int/Cont n=69/70)/ Ventilation & central heating (Int/Cont n=19/ 19) overall asthma scale 6.8 (2.1 to 11.5)*/9.3 (-1.9 to 20.6)/; physical scale 3.7 (-1.8 to 9.1)/10.3 (-1.7to 22.4); overall psychosocial scale 2.7 (-1.8 to 7.2)/0.6 (-10.1 to 11.3) Whole sample analysis comparing intervention not included in review (mould removal & installation of fan) with control. Mean difference in PedsQL subscales and overall scales (scores out of 100- higher values indicate better health) adjusted for baseline (95% CI) asthma subscales: symptoms 9.0 (3.8 to 14.3)/9.6 (4.0 to 14.9); treatment 4.4 (0.4 to 8. 4)/4.7 (10.2 to 9.2); worry 6.6 (-0.3 to 13.4)/ 6.2 (-0.5 to 12.9); communication 2.1 (-6.0 to 10.2)/10.1 (2.2 to 18.0); overall asthma scale 6.3 (2.1 to 10.4)/7.1 (2.8 to 11.4). Physical scale 7.2 (2.6 to 11.8)/4.5 (-0.2 to 9.1). Psychosocial subscales- emotional 5.8 (0.6 to 11.0)/3.6 (-1.5 to 8.8); social 1.2 (-4.0 to 6.5)/2.5 (-2.5 to 7.6); school 2.3 (-2.7 to 7. 4)/ 1.8 (-3.2 to 6.7); overall psychosocial scale 3.0 (-1.3 to 7.2)/ 2.2 (-1.9 to 6.4) Other (whole sample): Mean number of parent reported days absent from school Int/Cont- all causes 9.2 (median 7)/ 13.2 (median 9) p=0.091; asthma related 3.0 (median 0)/6.4 (median 2) p=0.053. Economic analysis reports costs of health service use but no data on health service use reported						

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

Osman et al, 2010, UK	Randomised controlled trial Final/ Baseline: 96/118 individuals (81.4%) Once: 20 months since baseline, 5 months since intervention	C	A	A	B	**A**	4	C
	Health : ITT analysis n= 59/59 (Int/Cont Before v After) (difference at follow-up between Int & Cont adjusted for baseline score, 95% CI) St Georges Respiratory Questionnaire Total (SGRQ) 68/68 v 69.8/68.9 (-0.9, -6.7 to 4.9); SGRQ Symptom score 73.8/76.5 v 73.2/77.1 (-3.5, -11.3 to 4.3); SGRQ Impact score (56.7/57.1 v 61.0/58.8 (3.0, -4.3 to 10.2)); SGRQ Activities score 85.5/83.0 v 83.5/82.6 (-1.4, -7.7 to 4.8); Visual Analogue Scale 50.3/47.1 v 48.5/48.5 (-0.3, -1.2 to 0.6). Hospital admission for Chronic Obstructive Pulmonary Disease (COPD) in past year 1.1/1.1 v 1.5/1.1 (0.4, -0.4 to 1.1). TOT conducted- see full study findings in appendix Housing: ITT analysis (Before v After Int/Cont) (difference at follow up between Int & No Int adjusted for baseline value, 95% CI) NHER 5.1/5.5 v 5.5/5.7 (0.2, -0.1 to 0.6); estimated Annual Fuel Costs (EAFC) £696/533 v £647/580, (-12.1, -52.4 to 28.7); hours at 21oC in one week (Oct-May) living room 55.9/73.1 v 59.4/64.0 (7.4, -11.0 to 25.8); bedroom hours at 18oC 100.2/109.5 v 111.9/102.2 (22.4, 1.6 to 43.4)*; Living room Average humidity g-kg-1 46.4/60.0 v 43.8/43.0 (-1.7, -4.9 to 1.6); Bedroom Average humidity g-kg-1 50.0/65.4 v 49.5/48.7 (-0.8, -3.5 to 1.9)							
Howden-Chapman et al, 2008, New Zealand	Randomised controlled trial Final/ Baseline sample: 349/409 (85.3%) children Once: 4-5 months since intervention.	C	A	A	A	**A**	5	C

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	12 months since baseline							
	Health : (OR for Int group adjusted for baseline measure where available) (95% CI) Parent reported measures- poor/fair health (as opposed to good/very good/excellent) (n=346, ~50% Int group) OR 0.48 (0.31 to 0.74)***; sleep disturbed by wheeze (n=344) OR 0.55 (0.35 to 0.85)**; wheeze limits speech (n=344) OR 0.69, (0.40 to 1.18); wheeze during exercise (n=344) OR 0.67 (0.42 to 1.06); dry cough at night (n=345) OR 0.52 (0.32 to 0.83)*; diarrhoea (n=343) OR 0.72 (0.45 to 1.16). Asthma symptom data from diary (Int/Cont n=178/182) (adjusted for baseline value) Mean Ratio (MR: mean score Int divided by Cont) (95% CI) cough at night (n=333) MR 0.72 (0.59 to 0.89)**; cough on waking MR 0.67 (0.53 to 0.84) ***; cough during the day MR 0.84 (0.70 to 1.01). Mean for Int compared with Cont (adjusted for baseline value) (95% CI); asthma visits to GP (n=323) -0.40 (-0.62 to +0.11) *; other visits to GP (n=333) -0.27 (-0.46 to -0.01)* Housing: At TI Mean temperature over 4 winter months (oC)- living room Int v Cont 17.07 v 15.97, p<0.001 (95% CI 0.54 to 1.67); child's bedroom 14.84 v 14.26, p=0.03 (95% CI 0.05 to 1.08); degree hours per day <10oC (hours per day multiplied by number of degrees below 10oC) 1.13 v 2.31, p=0.001 (95% CI 0.49 to 1.93); hours per day <10oC in child's bedroom 2.03 v 4.29, p<0.001 (95% CI 0.99 to 2.34). Mean NO2 over one month- in child's bedroom (μg/m3) (Int v Cont) 7.3 v 10.9, p<0.001; living room NO2 8.5 v 15.7, p<0.001 (outdoor NO2 levels unchanged) Other : Mean school absence (days of absence reported by school) Int/Cont 7.6/9.6, effect ratio 0.79 (95% CI 0.66 to 0.96). Sub-group analysis reported greater effect ratio for those whose pre-intervention heat source was an unflued gas heater (compared to an electric heat source) effect ratio 0.72 (95% CI 0.55 to 0.93)							
Braubach et al, 2008, Germany	Controlled before & after Final/ Baseline: 375/600 (62.5%) Once: 11-13 months since baseline; 5-8 months since intervention	C	B	B	A	**A**	2	C
	Health: (n=375, proportion of Int/Cont unclear Int ~56%) Self-reported health improved (Int/Cont) 29% v 13%; Depression- strong trend (actual measure unclear but includes self-reported sleep disturbance, loss of appetite, lack of motivation, lack of self-esteem) Before v After (Int/Cont n=179 v 157/130 v 131) 1% v 3.2%/0.8% v 2.4%, OR 1.404 (95% CI 0.329 to 5.987). Respiratory outcomes: Before v After Int/Cont, acute bronchitis in past 3 months 7% v 6.5%/5% v 7%; common cold 35% v 33%/33% v 38%; chronic bronchitis/emphysema 10% v 9.5%/5% v 8%; asthma 10% v 10%/5% v 6%.							

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	Housing (n not consistently reported): Living conditions unchanged at follow-up (Int/Cont) 32.8%/93.3%; housing satisfaction (want to stay in flat forever) Before v After (Int/Cont) 3.0% v 3.1%/3.9% v 3.7%; house less cold since renovation (Int/Cont) 68.7%/34.6%. Problems reduced since renovation (householder reported) (Int/Cont n=234): draughts 21%/2%; dampness/condensation 18%/4%; mould 12%/4%; frequent noise disturbance Before v After (Int/Cont) 23% v 16%/23% v 27%. Physical housing measures also reported							
Barton et al, 2007, UK	Randomised controlled trial Final/ Baseline sample: 426/481 (92%) Twice: Total Follow-up maximum of 2 years since intervention	A	A	A	A	**A**	6	C
	Health : (Time I Int/Cont n=193/254) Int/Cont (TI) change in prevalence of asthma -7%/-3%, ns, OR (95% CI) ~0.95 (0.60 to 1.50); bronchitis+4%/0%, ns, OR ~1.00 (0.48 to 2.13); 'other respiratory' (includes bronchitis but not asthma) -1%/+4%, ns, OR ~1.00 (0.55 to 1.80); arthritis 0%/-2%, ns, OR ~1.31 (0.73 to 2.34); rheumatism +3%/+2%, ns, OR ~0.52 (0.16 to 1.67). Paired analysis (Int/Cont n=14/13 adults, n=25/27 children) No significant difference in changes (Before-After(TI) Int/Cont) for six individual respiratory symptoms; summed score of six respiratory measures: adults -2.3 v +1.1, p=0.006, children -1.8/-1.0, p=0.17. Data on second follow-up when Int & Cont had received intervention not extracted Housing : Change (Before-AfterTI) mean temperature (oC) (bedroom) (Int/Cont n=49/69) Int v Cont +2 v +1, (living room) 0 v 0. No significant changes in environmental measures of air quality- particles (coarse and fine) or airborne microbes or relative indoor humidity							
Howden-Chapman et al, 2007, New Zealand	Randomised controlled trial Final/ Baseline sample: 3312/ 4407 (75. 2%)	C	A	B	A	**A**	4	C

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	Once: <12 months since baseline							
	Health : (Int/Cont n=1689/1623 individuals): Change Before v After in Int compared to Cont: 3 SF-36 domains (adjusted for baseline outcome value, household & region) % (95% CI): social functioning +6.2% (3.8 to 8.4)***/role emotional +10.9% (7.1 to 14.6)***/role physical +11.8% (8 to 15.5)***; likelihood of reporting fair or poor health; (adjusted for baseline outcome value, region & household) OR=0.50 (0.38 to 0.68)***; self-report symptoms colds or flu (adjusted for household) OR 0.54 (0.43 to 0.66) ***; wheezing in last 3 months (adjusted for baseline outcome value & household) OR 0.57 (0.47 to 0.70) ***; sleep disturbed by wheezing (child 0-12 years) (adjusted for household) OR 0.57 (0.40 to 0.81)**; hospital admission for respiratory condition (adjusted for region) OR 0.53 (0.22 to 1.29) Housing : Before v After (Int/Cont n=563/565 households) Int compared to Cont at Time I OR (95% CI): house cold most/all time OR 0.62 (0.04 to 0.09)***; mould OR 0.24 (0.18 to 0.32)***; condensation OR 0.16***; energy use OR 0.81(0.72 to 0.91,p=0.0006). Sub-group (n=140): change in temperature (oC) Int/Cont +0.6/+0.2, p=0.05; % change in relative humidity +3.8/-1.4, p=0.05; difference in average hours per day indoor temperature falls below 10oC -0.99/+0.45, p=0.007. Other : Days off work (adjusted for region, non-working & working adults in house) Incident Rate Ratio 0.618 (0.466 to 0.818), p=0.001 Economic analysis : Current value of benefits per household (NZ $) at 7% discount rate, reductions in: hospital admissions $1801; days off school $196; days off work $145; energy costs $635 N.B: All results control for age group, sex, ethnicity- plus other variables where stated. Unclear about missing data in analysis- 80% data for hospital data, 82% for GP data. Little change in weather between assessment years							
Platt et al, 2007, UK	Controlled before & after Final/ Baseline sample: 2365/ 3849 (61%) Twice: 1 year and 2 years since baseline	C	B	B	A	**A**	2	B
	Health : Before v After (Int/Cont n=1281/1084): (adjusted for attrition, and adjusted for Int/Cont, baseline value, gender, tenure, household composition, serious life event in past year, change in tobacco smoke exposure since baseline, socio-economic group) Since baseline; first diagnosis of heart disease (2 years) OR 0.69, p=0.01; first diagnosis of hypertension OR 0.77, p=0.02; first diagnosis of nasal allergy OR 1.52, p=0.03. No significant change in Int compared							

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	to Cont for: other cardiac & respiratory symptoms, health service use, medication, longstanding illness, smoking, alcohol consumption. Small increase (improvement) in 2/6 SF-36 domains (general health & physical functioning- but unlikely to be clinically significant Housing : Int compared to Cont group: home warm enough in winter (n=2289) OR 3.5**; more than half of rooms permanently unheated in cold weather (n=2149) OR 0.22 (0.16 to 0.29)**; average hours of heating (n=2149) 1.12 (0.6 to 1.64)**; any rooms in home not used due to damp/condensation (n=300) OR 0.39*; 'would not want to move home if able to do so' (n=2207) OR 0.83 (0.69 to 0.99)* Other : friends/relatives dissuaded from visiting due to poor housing conditions (n=2322) OR 0.4 (0.23 to 0.70)**; financial difficulty (n=2318) (not adjusted for tobacco smoke exposure) OR 0.77 (0.6 to 0.99)*. See full data extraction for details of independent variables in analysis							
Lloyd et al, 2008, UK	Controlled before & after Final/ Baseline sample: 36/68 (52.9%) Once: at least 1-2.5 years since intervention; 4 years since baseline	B	C	C	B	**B**	2	C
	Health: (Int/Cont n=27/9) Mean change in blood pressure (mmHg) (Int/Cont- paired means, 2 sample t test): systolic -19.36/+2.78, difference in change 22.14 (95% CI 13.77 to 31.12)*** ; diastolic -11.85/+8.22, difference in change 20.07 (95% CI 12.70 to 27.44)***. At least 4 years after time of intervention (Int/Cont n-75/40), Intervention group report improvements in respiratory health and some other improvements in health and illness, and reduced need for medical attention. (unclear how these data were obtained) Housing: At least 4 years after intervention (Int/Cont n=75/40), Intervention group report heating costs reduced from £35 per week to £7 per week, no change in rent. Control group do not report any changes in housing costs. (unclear how these data were obtained)							
Shortt et al, 2004, Northern Ireland	Controlled before & after Final sam-	C	C	B	A	**B**	0	C

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	ple: 245/378 (65%) households. Data presented for 46/54 households Int/Cont (144 households received partial intervention- data not presented) Once: 1-3.5 years since intervention							
	Health : (Int/Cont n=46/54 households) Prevalence of specific illnesses (%) Before v After (Int/Cont), ~OR (Compares Int v Cont): angina 17.4 v 4.3, ns/ 0.0v 1.8, ns, OR ~0.2*; arthritis/rheumatism 34.8 v 8.7*/10.9 v 5.5, ns,OR ~1.62; asthma 15.1 v 4.3, ns/10.9 v 6.5, ns, OR ~0.57; chest infections/bronchitis 26.0 v 13.0, ns/1.8 v 7.3, ns, OR ~1.88; pneumonia/hypothermia 2.1 v 2.1, ns/0.0 v 0.0, ns, ~OR 3.60; stress/mental illness 10.8 v 4.3, ns/1.8 v 14.5*, OR ~0.26; other illnesses 28.2 v 4.3*/3.6 v 7.2, ns, OR ~0.57; mean number of illnesses per head 1.43 v 0.91*/0.17 v 0.23, ns. Housing : Mean satisfaction with house temperature during cold periods Before v After (Int/Cont) (10 pt score) 3.57 v 9.18***/8.19 v 8.35, ns; mean number of rooms with householder-reported condensation/mould/damp Before v After (Int/Cont) 2.1 v 0.7***/1.5 v 1.1, ns Economic: mean number of welfare benefits awarded Before v After (Int/Cont) 1.78 v 1.87, ns/0.02 v 0.71***							
Somerville et al, 2000, UK	Uncontrolled before & after Final/ Baseline sample: 72/114 (63%) Once: 3 months since inter-	B	C	B	A	**B**	1	B

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	vention							
	Health : (n=72 children, 59 households) Before v After (median) cough by day 2 v 1***; cough by night 3 v 1***; wheeze by night 2 v 0***; breathless with exercise 2 v 1**; breathless 1 v 0***; runny nose 2 v 0***; blocked nose 2 v 0***; hay fever 0 v 0, ns; diarrhoea 0 v 0. Housing: (n=72 children, 59 households) Children sleeping in unheated /damp/damp & mouldy bedrooms 92% v 14%*/61% v 21%*/43% v 6%* ; children living with furred/feathered pets 63% v 78% ns, living with at least one smoker 71% v 64% ns Other: Days lost from school due to asthma (rate per 100 school days) Before v After 9.3 v 2.1, mean difference (paired) 7.27 (95% CI 3.32 to 11.21 ***), mean difference for days off school due to other causes -1.8 (95% CI -3.86 to 0.26). Economic analysis (n=47): Net benefits per year considering cost of improvement (£3061), savings on fuel bills, saving on NHS treatment costs, prescribing costs, increase value of school attendance: £413.32 per household per year							
Hopton & Hunt, 1996, UK	Controlled before & after Final/ Baseline sample: 258/532 (48.5%) Twice: 6-12 months since baseline; 5-11 months since intervention	A	C	C	B	**B**	1	C
	Health : (Int/Cont n=55/77 households) Before v After Int/Cont. Children's symptoms: mean number symptoms 3.69 v 3.72/3.09 v 3.89. Regression analysis (adjusted for smoking, changes in other housing conditions, unemployment, perceived financial situation) change in reported level of dampness was the only significant predictor of change in reporting of runny nose**, intervention not independent predictor or mean number of symptoms Housing: (Int/Cont n=55/77 households) Before v After Int/Cont House too cold 65.5% v 10.9% ***/55.8% v 46.8%, ns; problem with dampness 74.5% v 32.7%***/58.4% v 57.1%, ns; one or more rooms not heated in past 2 weeks 78. 2% v 3.6%***/68.8% v 75.3%, ns; one or more rooms prefer not to use due to dampness 20.0% v 9.1%, ns/26.0% v 35.1, ns; estimated weekly heating cost (£) 4.45 v 1.86/3.33 v 3.49							
Allen, 2005, UK	Uncontrolled before & after	C	C	C	A	**C**	0	C

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	Final sample: 29/49 (59%). Once: <12 months since intervention & baseline							
	Health: (n=16) Before v After mean GHQ score 6.5 v 2.6 paired t-test p=0.001 Housing: (n=29) After self reported housing conditions 'a lot better' 83%; 'a little better' 17%; Before v After sufficient heating to keep everyone warm 35% v 90%; winter temperature in living rooms 'is about right' (n=26) 31% v 92%; draughtiness 'in the winter my living rooms are usually about right' (n=26) 17% v 75%							
Allen, 2005, UK	Uncontrolled before & after Final/ Baseline sample: 32/64-71(50%-45%) Once: <3 years since intervention & baseline	C	C	C	A	**C**	0	C
	Health: Before v After paired analysis (n=24) Mean SF36 Physical Component Score (PCS) 36.1 v 35.8, ns; Mental Component Score (MCS) 39.7 v 45.9, p=0.013; Mean HADS anxiety 11.9 v 9.8 p=0.028; HADS depression 10.9 v 9.5, p=0.106 Housing: (n=33) Before v After have adequate heating 36% v 73%; temperature in living room 'about right' 39% v 72%; damp 73% v 54%; housing conditions 'a lot/little better' 86%							
Health Action Calderdale Kirklees and Wakefield, 2005,	Retrospective Uncontrolled Final sample: 102	B	C	C	A	**C**	1	B

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

UK	Once: 2-8 months since intervention							
	Health: 78% reported improvement in medical condition; 56% reported reduced medication use; 30% reported reduced GP visits due to improved medical condition Housing: 94% reported improvement in dwelling warmth; 56% reported reduced housing costs/bills							
Iversen et al, 1986, Denmark	Controlled before & after Final/ Baseline sample: 641/1013 (63%) Three times: 1-4 months; 2-5 months; 3-6 months since intervention & baseline	C	C	C	B	**C**	3	B
	Health: (Int/Cont n=106/535) Normalised Odds Ratios (OR) (odds for Int group divided by the Cont group odds, normalised to baseline & adjusted for smoking, age, and colds) by month Dec/Jan/Feb. Symptoms related to mucosal surfaces- eye irritation 0.33/0.00/0.00 (sic); dry throat 0.44/0.52/0.67; rheumatic symptoms- joint pains 0.79/0.41/0.28; neck/back pain 0.38/0.11/0.18. Symptoms reduced but ns different from baseline (% estimated from graphs) Aug v Feb (Int/Cont): dry throat 7% v 7%/15% v 20%; neck pain 12% v 8%/9% v 24% Housing: (Int/Cont n=106/535) Normalised OR (normalised to August) for Int divided by Cont group OR: Dec/Jan/Feb low temp 0.15/0.14/0.17; high temp 1.32/1.22/0.79; cold floor 0.15/0.16/0.18; draughts 0.07/0.08/0.06; noise from outside 0.04/0.02/0.03; noise from building 0.33/0.26/0.35							
Intervention : Rehousing/retrofitting +/- neighbourhood renewal (post 1995)								
Kearns et al, 2008, UK	Controlled before & after Final/	C	C	B	A	**A**	1	C

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	Baseline sample: 547/723 (75.7%) Twice: 9-12 & 21-24 months since intervention							
	Health: (Int/Cont n=262/284) (OR: compared to control group, adjusted for baseline value) good health (self reported) OR (95% CI) 1.30 (0.85 to 2.00) p=0.23; health compared to 1 year ago (Int/Cont n=262/284) OR 1.27 (0.86 to 1. 85) p=0.23; long standing illness (Int/Cont n=262/283) OR 0.68 (0.44 to 1.05), p=0.08; SF-36 physical functioning (Int/Cont n=261/284) mean change +0.39/-0.55, p=0.36. Wheezing in past year (Int/Cont n=262/284) OR 1.04 (0. 69 to 1.56), p=0.85; current smoker (Int/Cont n=262/284) OR 1.47 (0.85 to 2.55), p=0.17; heavy drinker (Int/Cont n=261/283) OR 0.61 (0.30 to 1.24), p=0.18; fruit & veg (5+ portions a day) (Int/Cont n=262/284) OR 1.26 (0.82 to 1.92), p=0.29. Mental health: change in mean SF-36 domain scores, Before v After Int/Cont (n=333 v 261/386 v 283) mental health+1.1 v +2.1, p=0.36; vitality (Int/Cont n=333 v 261/385 v 282) +0.1 v +0.3, p=0.87; social functioning (Int/Cont n=331 v 259/387 v 281) +0.9 v +1.5, p=missing; role-emotional (Int/Cont n=333 v 260/387 v 283) +1. 3 v +1.2, p=0.94. Child Health: Chronic illness (Int/Cont 221/208): asthma 20.8%/20.2%, p= 0.873; eczema* 16. 7%/14.9%, p= 0.602; bronchitis * 0.5%/1.0%, p= 0.527. Health problems in past month (Int/Cont n=222/209); breathlessness 4.5%/3.8%, p= 0.726; sinus/catarrh 9.9%/11.0%, p= 0.710; persistent cough 18.0%/16.8%, p= 0.728 Housing : (Int/Cont) Change in housing: private sector -26.5%/+9.2%; social sector +26.6%/-9.0%; house +34.8%/ +3.2%; flat -34.6%/-3.3%; no access to outside space change -19.6%/-2.7%; damp -32.5%/+0.8%; condensation -34. 1%/-0.4%; draughts -31.0%/-3.7%; not enough privacy -17.2%/+2.5%; neighbourhood satisfaction 64.5% v 77.9%/ 82.0% v 79.6%. Affordability: often difficult to pay: rent/mortgage 21.52% v 7.22%; utility bills 25.94% v 7.25% Other : Mean score from 10 psycho-social measures (include measures of privacy, control, safety, identity) (Int v Cont n=257/278) +7.0 v -0.1 ***. Mean change in size of social network- close friends/relatives (Int/Cont n=262/284) -1.9/- 1.4, p=0.52. Neighbouring: visit neighbours in own homes (Int/Cont n=262/284) OR 1.40, p=0.09; borrow/exchange favours with neighbours (Int/Cont n=262/284) OR 1.17, p=0.40							
Thomson et al, 2006, UK	Controlled before & after Final/ Baseline sample: 100/143 (69.9%) Once: 12 months since intervention	C	B	B	A	**A**	3	B

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	Health: (Int/Cont n= 50/50) Before v After Int/Cont % reporting excellent-good health: 32.6% v 34.8%/40.0% v 46. 0%, change +2.2%/+6.0%, ns, OR of better health in Int 0.78; change in SF36 physical component score Int v Cont -1.41 v +0.35 (Int: paired t=1.010; 95%CI -1.42 to 4.24/Cont paired t=-0.238; 95%CI -3.01 to 2.372) OR of higher PCS score in Int 1.04; change in SF-36 mental component score Int v Cont -2.08 v +0.22 (Int paired t=1.094; 95%CI -1.756 to 5.922/Cont paired t=-0.143; 95% CI -3.41 to 2.96), OR of higher MCS score in Int 1.36 Housing: (Int/Cont n=50/50) Before v After Int/Cont Change in 'no problem with..': dampness/condensation +24%/ +2%, (95% CI 8.82 to 35.18); draughts or leaky windows +28%/+10%, (95% CI 2.62 to 33.38); keep warm in winter +20%/+6%, (95% CI 0.82 to 27.18); heating system +22%/+4%, (95% CI 4.82 to 31.18); 'other' housing problems +10%/+12%, (95% CI -10.27 to 14.27) ns; change in mean number of neighbourhood problems Int -1.02 (paired t= 1.639, 95% CI -0.231 to 2.271) Cont +0.14 paired t=-0.279 (95% CI 1.148 to 0.868)							
Critchley et al, 2004, UK	Controlled before & after Final/ Baseline sample: 268/407 (66%) Once: ~1-12 months since intervention; 2-3 years since baseline	C	B	B	A	**A**	1	B
	Health : (Int/Cont n=~109/137) Change in SF-36 general health (data estimated from graph) Int Area I/Int Area 2/ Cont Area I/Cont Area 2 (Men n=29/19/40/13, Women n=35/26/57/27), Men -3/-0.5/0/-8; Women +0.5/+4/-1.5/-1; SF-36 mental health Men -2/0/0/-1 Women +0.5/+4.5/-1/-1.5 no changes statistically significant at 95% level. GP use in past two weeks reduced in each group- greatest reduction in Int; increase in hospital attendance across all groups. Energy efficiency ratings (SAP) changed in both groups Sub-group analysis by change in SAP: Greatest improvement in remaining seven SF-36 domains reported for residents moving from low to high SAP homes (no data reported) Housing & Neighbourhood : Mean SAP ratings (energy efficiency) Before v After IntA/IntB/Cont 62 v 91/19 v 87***/ 24 v 36. Affordable adequate heating Before v After Int/Cont 75% v 100%/64% v 85%; fuel costs similar in Int and Cont both before and after intervention. Change in mean temperature oC (n=33 v 34) (living room) Int v Cont +4.7 v +0.1; 'very satisfied/satisfied with overall comfort' Before v After (Int n=128) 48% v 92%							
Thomas et al, 2005, UK	Controlled before & after	C	B	C	A	**B**	1	C

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	Final/ Baseline sample: 1, 344/2596 (51.8%) Once: 22 months since baseline							
	Health : (IntCont n= 585/759) Mean GHQ score After Int/Cont 2.621/2.528; Mean diff in GHQ score between Before & After for Int/Cont 0.093/0.057, p=0.647/0.747. Sub-group analysis of all householders (i.e. both those in and outside neighbourhood regeneration area) comparing those with and without housing improvement (With/Without treat as Int/Cont n=585/759). Mean diff in GHQ score between Before & After for 'one housing improvement' +0. 053 paired t=0.121, p=0.904 and for 'no housing improvement' +0.092 paired t=0.620, p=0.535							
Barnes, 2003, UK	Con- trolled be- fore & af- ter Final/ Baseline sample: 90/212 (42%) Three (six attempted) : Analysis conducted on 3 fol- low-ups to 18 months since inter- vention	A	C	C	A	**B**	1	C
	Health : (Int/Cont n=45/45 30% of baseline sample- only follow-up data reported here) % Change Int v Cont (Time III- 18 months since intervention) (% estimated from graphs). Self reported fair/poor health 22% v 50%**, OR for Int compared to Cont ~0.273 (95% CI 0.110 to 0.682); health problems affecting daily activities 35% v 26%, ns, OR ~0. 52 (0.62 to 3.73); health worse/somewhat worse compared to 1 year ago: 76% v 83%, ns, OR for Int compared to Cont ~0.356 (95% CI 0.135 to 0.942); mobility problems 25% v 38%, ns OR ~0.53 (0.22 to 1.32) ; pain and discomfort 33% v 56%*, OR ~0.40 (0.17 to 0.94); anxiety and depression 32% v 56%*, OR ~0.36 (0.15 to 0.86); health service use- visit to GP in past month 47% v 60%, ns Housing & Neighbourhood : (Int/Cont n=45/45, only follow-up data reported here) % Change (baseline to Time III							

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	18 months since intervention) (% estimated from graphs). Very/fairly satisfied with housing Int v Cont 82% v 70%, ns; very/fairly satisfied with local area as a place to live 82% v 77%, ns; fear of crime affects health of your family a lot/to some extent 61% v 57%, ns; feel very/quite safe in home 80% v 81%, ns; very/quite safe outside home 79% v 67%, ns							
Evans et al, 2002, UK	Controlled before & after Final sample: 67 Once: 6-18 months since intervention; ~2 years since baseline	C	B	C	A	**B**	0	C
	Health: Changes in median of SF-36 domains (100 point scales): physical function (Int/Cont n=17/17) -30/-1; general health (n=19/15) +7/-6 Housing: Change in mean household temperature (Int v Cont, n=22) -0.1oC v +0.14oC, some reduction in those reports of cold homes							
Breysse et al, 2011, USA	Retrospective Uncontrolled Final sample: 24 adults & 17 children Twice: 1-4 & 12-18 months since intervention	C	C	C	A	**C**	0	C
	Health: (T1 1-4; TII 12-18 months after intervention) Recalled health better/same/worse since intervention (TI adults n=29) 10/17/2, p=0.042; (TII adults n=18) 5/9/4, p=0.786; (TI child n=30) 7/19/4, p=0.476; (TII child n=15) 5/8/2, p=0.358. General health excellent/good/poor (TI adults n=21) 7/10/4; (TII adults n=21) 13/5/3, p=0.052; (TI child n=17) 9/6/2; (TII child n=17) 11/6/0, p=0.206. Percentage recalled self-reported change 12 to 18 months since renovation (adults n=22/children n=13): asthma -4%/0%, p=0.317/na; injury 0%/+18%, p=na/0.083 ; non-asthma respiratory illness -23%/-15%, p=0.025/p=0.317 Housing : Percent recalling housing conditions comparing pre-intervention condition with 12-18 months since intervention (n=17): water dampness -26%, p=0.102; musty smell -25%, p=0.046; dehumidifier use -25%, p=0.046; humidifier use +7%, p=0.157; cockroaches -12%, p=0.414; mice/rats -25%, p=0.046; insecticides -19%, p=0.083; smoke							

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	inside home -13%, p=0.157; clean >1 time per week +31%, p=0.025. Radon Before v After 3.1 v 0.7 pCi/litre. Energy use (electricity & gas: British Thermal Units per Heating Degree Days per square foot per year) Before v After 9.76 v 5.05. Air quality data reported but no change data to confirm improvements							
Molnar, 2010, Hungary	Uncontrolled before & after Final/ Baseline sample: 9/ 12 households (75%) Once: 5 years since intervention	C	C	B	C	**C**	0	C
	Health: Before v After No of people with functional limitation 2 v 2; cardiovascular disease 3 v 5; hypertension 2 v 4; thrombosis 1 v 1; varicsositas 1 v 1; mentally retarded children 5 v 3; epilepsy 2 v 2; brain tumour 1 v 1; spinal hernia 2 v 2; families with children with scabies/louse/impetigo 3 v 2							
Blackman and Harvey, 2001, UK	Uncontrolled before & after Final/ Baseline sample: 208/209 (99%) Once: 5 years since intervention	B	C	C	A	**C**	2	C
	Health : Before v After (n=166 adults) self-reported health 'not good' 9.7% v 22.0%**; respiratory condition chronic 31.9% v 44.0%*; mental health problems 52.4% v 41.0%*; no significant changes in health service use; prescribed medication for month or more 36.4% v 47.0%*; smoker 71.6% v 27.9% ***. Children (n=43): self-reported health good 73.8% v 79.1%, ns; respiratory condition- chronic 23.3% v 25.6%, ns; mental health problems 20.9% v 2.3%*; visit to GP in past 2 wks 15.9% v 0.0% **; changes in hospital use or prescribed medication for month or more, ns Housing : Before v After (n=98 households): Dwelling has no draughts 50.0% v 73.5%*; dwelling has draughts that affect health 11.2% v 6.1%, ns; dwelling has no damp 76.0% v 85.7%, ns; dwelling has draughts that affect health 3. 1% v 4.1% ns; unable to keep warm last winter 15.4% v 14.3%, ns; happy with present home 85.7% v 84.7%, ns							

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

Wells, 2000, USA	Uncontrolled before & after Final/ Baseline sample: 23/31 (74.2%) Twice: 5-12 months & 2-3 years since intervention	C	C	B	A	**C**	1	B
	Health: (n=23) Before v After (Time I) (n=31) PERI (mental health) 31.00 v 22.26***; Before v After (Time II) (n=23) PERI 31.00 v 22.26**; Baseline PERI predicts 31%**; baseline housing quality predicts 12%**; baseline house crowding predicts 12%**; indoor climate predicts 21%** of variance of PERI at Time I Housing: (n=31) Before v After (Time I) crowding 1.39 v 2.24***; indoor climate 1.79 v 2.30***; cleanliness 1.41 v 1.79***; structural quality 2.79 v 3.00***; hazards 1.29 v 1.46*; overall housing quality 1.73 v 2.14***							
Ambrose, 1999, UK	Uncontrolled before & after Final/ Baseline sample: 227/525 (43%) Once: ~4-4.5 years since baseline	A	C	C	A	**C**	1	C
	Health: (Before v After n=525 v 227) Before v After (in previous 4-6 weeks but unclear) cough/cold 41.9% v 66.7%***; aches/pains 22.6% v 11.5%***; asthmatic/bronchial 17.0% v 5.7%***; dietary/digestive 12.4% v 14.9%, ns; stress/depression 6.1% v 1.2%** Housing: (Before v After n=525 v 227) self reported damp 68.2% v 34.0%***; heating keeps everyone warm 30.8% v 68.0%***; heating not used due to cost 25% v 2%***; infestation 33.6% v 22.0%**; repairs needed 72.9% v 40.0%***; very/fairly satisfied with house 34.6% v 76.0%***; repairs needed 72.9% v 40.0%***; feel quite safe in home 46.7% v 72.0%***							

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	Socio-economic status & other: (Before v After n=525 v 227) Before v After unemployed > 6 months 7.5% v 7.5%, ns; received income support 65.4% v 76.0%**							
Halpern, 1995, UK	Cross sectional Uncontrolled before & after Final/ Baseline sample: 27/55 (49.1%) Once: 10 months since intervention; 3 years since baseline	C	C	C	A	**C**	0	C
	Health : No panel data- analysed by stage of intervention: T0: no intervention; TI: intervention started in some areas; TII: intervention complete (T0/TI/TII n=28/57/27). Hospital Anxiety & Depression Scale (HADS) proportion of anxiety cases (score 8+) 57.1%/45.6%/22.6%, change T0-TII p=0.008; proportion depression cases (score 8+) 25.0%/ 21.2%/3.7%, change T0-TII p=0.025 Neighbourhood : Sometimes bothered by noise T0/TI/TII 59%/50%/50%, T0-TII ns; data from one area residents 'very concerned about safety from traffic' (TI v TII) 65% v 39%*; 'very concerned about attack' (T0/TI/TII) 48%/ 50%/35%, T0-TII ns; describing estate as 'very safe' or 'safe' 41%/34%/81%, T0-TII **; 'good' or 'very good' place to bring up children 22%/34%/52%, T0-TII *; rate area as very friendly 7%/18%/26%, T0-TII ns							
Intervention : Provision of basic housing needs/low or middle income country intervention								
Rojas de Arias, 1999, Paraguay	CBA (3 intervention groups) Final: 621/ 762 individuals (81.5%) Once: 3-36 months	C	C	A	A	**B**	0	B
	Health: Intervention A- Insecticide, B-Housing improvement. Before v After % Triatomine serology Int A/B/A+B (n=172 v 132/265 v 229/325 v 260) 28.5 v 17.4 p=0.02/14.0 v 12.7 p=0.67/19.4 v 16.9 =0.39. Sub-group analysis by gender: Int A/B/A+B Male (n=103 v 72/138 v 112/154 v 127) 23.3 v 7.6 p=0.121/13.0 v 14.3 p=0.776/19.5 v							

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	22.8 p=0.492; Female (n=69 v 60/127 v 117/171 v 137) 36.2 v 21.7 p=0.070/15.0 v 11.1 p=0.374/19.3 v 14.6 p=0.278. Analysis by 17 age groups presented graphically- suggests no clear age where most likely to observe change in seropositivity Housing: Before v After % Households with Triatomine infestation Int A/B/A+B (n=51 v 41/61 v 59/70 v 55) 45.1 v 2.4 p<0.000/32.8 v 3.4 p<0.000/48.6 v 16.4 p<0.000							
Spiegel et al, 2003, Cuba	Cross sectional Controlled before and after Final sample: 1,703 Once: between 1-4 years since intervention, 5 years since baseline	C	C	C	A	**C**	0	C
	Health : (Int/Cont n=896/807) Before v After Int/Cont self-reported excellent-very good health (%) Male (all ages) 31.3 v 78.6***/24.7 v 15.6, ns, Female: no statistically significant change in health; mixed changes in smoking prevalence across male/female and across age groups. Housing : (Int/Cont n=328/307) Although substantial improvements reported, with some improvements in control group, after intervention (Int/Cont) 77.8%/76.9% reported unmet need for internal housing repair; 79.7%/87.1% for external housing repair							
Aziz et al, 1990, Bangladesh	Cross sectional controlled before & after Final/ Baseline: Cannot tell Three times: 2- 3 & 9 years since baseline, 1-2, 2-3 & 8-9 years since	C	B	C	B	**C**	2	B

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	installation of pit latrines							
	Health : Before v After (1984 v 1987) Int/Cont (Incidence Density Ratio (IDR), 95% CI) Incidence of all diarrhoea episodes per child per year 3.85/3.75 (1.02, 0.96 to 1.09) v 2.34/3.12 (0.75, 0.70 to 0.80**); Incidence of dysentery 0.62/0.54 (1.16, 1.0 to 1.34) v 0.27/0.36 (0.73, 0.61 to 0.88***). Diarrhoea incidence by age in months: 0-5 months 2.46/2.27 (1.09, 0.87 to 1.36) v 2.43/2.26 (1.08, 0.87 to 1.32); 6-11 months 4.11/4.63 (0.89, 0.78 to 1.01) v 3.33/4.25 (0.78, 0.68 to 0.90***); 12-23 months 4.79/5.17 (0.93, ns) v 3.13/4.12 (0.76, 0.68 to 0.84***); 24-35 months 4.44/4.15 (1.07, ns) v 2.36/3.34 (0.62 to 0.80***); 36-59 months 3.32/2.73 (1.22, 1.10 to 1.34**) v 1.66/2.46 (0.68, 0.60 to 0.75***). Episodes of diarrhoea per child (under 60 months) per year by disposal of faeces in latrine/Not in latrine (intervention group only) 1986 v 1987 2.10/2.40** v 2.12/2.61***. (Some data reported at 9 years post intervention, see full data extraction table for details)							
Intervention : Rehousing from slums (before 1970)								
Wilner et al, 1960, USA	Controlled before & after Final/ Baseline sample: 4784/ 4805 (99.6%). Six times: ~18 months since baseline	B	B	A	A	**A**	2	B
	Health : (Int/Cont Time V (18 months after baseline) n=1891/2893) At least 1 day disability in past 2 months OR ~1.145 (95% CI 0.98 to 1.34). Change (Time I-After (Time V)) illness episodes in past 2 months (rate per 1000) Int v Cont (all ages), Time I-Time V -431.1 v -362.3. Change (Before-After (Time VI), Int/Cont n=396/633-377/583) Int v Cont nervousness +1.0% v +2.3%***, OR ~1.16 (0.89 to 1.50); negative mood -13.6%*** v -10.6%***, OR ~0.91 (95% CI 0.70 to 1.82); dissatisfaction with status quo -23.3%*** v -19.5%***, OR ~0.86 (0.66 to 1.12) ; potency -4.9% v -11.5%***, OR ~0.81 (0.63 to 1.05); pessimism -8.8%* v -11.2%***, OR ~0.82 (0.63 to 1.06); emotionality -3.0% v +4.8%, OR ~0.80 (0.61 to 1.03). Among Cont group who had moved (n=195, large/moderate/no housing improvement 52/75/68) there was a dose-response relationship demonstrated for morale measures directly linked to degree of housing quality improvement between Baseline and Time VI: optimism scale (large/med/no change in housing quality, ~OR compares large & no housing improvement) +25.0%/+16.0%/+5.9%, ~OR 5.33 (this analysis includes 33% of Cont group at Time VI and appears to include only half of the 'control group movers' this may be due to movers who were untraceable) Housing : Change (Before-After, Int/Cont 396/633-377/583) 'how do you like apartment?' Int v Cont +55.3%*** v +16.5%***; "deficiencies such as lack of hot water, sharing of facilities, crowding, lack of central heating, and infestation							

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	were wiped out" Other : Change (Before-After, Int/Cont 396/633-377/583): 'places where children play are not safe' -39.8%*** v +0. 5%, ns; 'family often sit and talk' +11.1%** v +1.9%, ns; feel 'better off' compared to 5 years ago +19.0%*** v +4.0%, ns							
Chapin, 1938, USA	Uncontrolled before & after Final/ Baseline sample: 171/198 (86.4%) households. Once: 8-19 months since intervention	C	C	A	B	**C**	2	B
	Health: (n=171 families) Before v After mean morale score 65.5 v 63.52 (improvement). Sub-group analysis of % change in mean morale score by change in overcrowding (fall indicates improvement): Improvement not clearly related to overcrowding. Before v After overcrowded before & after move (n=18) -2.5%; moved from overcrowded to not-crowded (n=23) -3.8%; moved from not overcrowded to overcrowded (n=24) -8.5% Housing: Before v after mean no of rooms 5.22 v 4.78; person to room ratio 0.82 v 0.83; mean dwelling unit rental $15.68 v 17.98							
Mc-Gonigle & Kirby, 1936, UK	Cross-sectional controlled before & after Final/ Baseline sample: unclear/ 441 households Once: 5 years since intervention	C	B	C	B	**C**	0	C

Table 9. Summary of included study characteristics and findings (ordered by study quality (Hamilton Overall Grade), date of publication and study design) portrait (*Continued*)

	Health: (Routine area based data includes study households Int/Cont n=152/289) Before v After Int Area/Cont Area Standardised death rates per 1000: 22.91/33.55 v 26.10/22.78 (Borough 12.32 v 12.07). Increased death rates reported to affect those from 10-65 years rather than those at the extremes of life. Infant Mortality Rates (unclear if these were standardised) per 1000 live births 172.6/173.2 v 117.8/134.0. No report of infective epidemic Other: (Int/Cont n=35/30 families) Before v After Int/Cont rent as % of income 20.5%/14.7% v 31.3%/20.8%. Survey reports shortage of main dietary constituents except carbohydrates. Shortages greater in families in Int area. 90% unemployment in Int area after rehousing

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author)

Intervention: Warmth & energy efficiency improvements (post 1980)										
Author Year	**Study grade**	**Housing condition**	**General health**		**Respiratory**		**Mental**		**Illness/ symptoms**	
CHARISMA 2011 ***(subgroup: central heating/ ventilation only)***	A		Physical health	▲	Overall asthma scale (PedsQL)	▲	Overall psychosocial scale (PedsQL)	▲		
Osman et al 2010	A	▲	Euroqual analogue	▼ [a]	SGRQ total	▼ [a]				
					SGRQ impact	▼ [a]				
					SGRQ activities	▼ [a]				
Osman et al 2010 ***(subgroup: no/ some intervention)***		▲			SGRQ symptoms	▲ [a]				
			Euroqual	▲ [a]	SGRQ total	▲ [a]				

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

				ana-logue							
						SGRQ impact	▲ [a]				
						SGRQ activities	▲ [a]				
						SGRQ symp-toms	▲ [a]				
Howden-Chapman et al 2008 *(children)*		A	▲	Poor/ fair health	▲	Sleep dis-turbed by wheeze	▲			Diarrhoea	▲
						Wheeze limits speech	▲			Twisted an-kle	▼
						Wheeze during exercise	▲			Vomiting	▲
						Dry cough at night	▲			Ear infection	▼
						Cough at night (diary)	▲				
						Cough on wak-ing (di-ary)	▲				
						Cough during day (di-ary)	▲				
						Cough overall	▲				

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

					Lower resp symptoms	▲			
					Upper resp symptoms	▲			
					Wheeze overall	▲			
Barton et al 2007 *(adults & children,*	A	◄ ►	SF-36 domains	<>	Asthma prevalence	▲	GHQ	Arthritis	▼
					Bronchitis	▼	SF-36 domains <>	Rheumatism	▼
					'other' respiratory conditions	▼			
Barton et al 2007 *(adults only-paired 14/13 Cont)*	A	◄ ►			Breathless on exercise	▲ [a]			
					Breathless	▲ [a]			
					Wheeze (day)	▲ [a]			
					Wheeze (night)	▲ [a]			
					Cough (day)	▲ [a]			
					Cough (night)	▲ [a]			
					Mean asthma score	▲ [a]			

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

Barton et al 2007 *(children)*	A	◂ ▸			Breathless on exercise	▲ [a]				
					Breathless	▲ [a]				
					Wheeze (day)	▲ [a]				
					Wheeze (night)	▲ [a]				
					Cough (day)	▲ [a]				
					Cough (night)	▲ [a]				
					Mean asthma score	▲ [a]				
Howden-Chapman et al 2007	A	▲	Fair/ poor health	▲	Wheezing in last 3 months	▲	Role emotional (SF-36)	▲ [a]		
			Social functioning (SF-36)	▲	SR cold/ flu symptoms	▲	Happiness (SF-36)	▲		
			Role physical (SF-36)	▲ [a]	Morning phlegm	▲	Vitality (SF-36)	▲		
Howden-Chapman et al 2007 *(children)*	A	▲			Sleep disturbed by wheezing	▲				
					Speech disturbed by	▲				

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

					wheezing					
Braubach et al 2008	A		Self-reported health		Asthma	<>	Depression customised score	▼		
					Common cold					
					Acute bronchitis					
					Chronic bronchitis					
Platt et al 2007	A	▲	General health (SF-36)	▲ [a]	Ever diagnosed nasal allergy	▼	Mental health (SF-36)	▼ [a]	Ever diagnosed heart disease	▲ [a]
			Physical functioning (SF-36)	▲ [a]	Other respiratory symptoms	<>	Vitality (SF-36)	▲ [a]	Ever diagnosed hypertension	▲ [a]
			Role physical (SF-36)	▲ [a]	Ever diagnosed asthma	▲ [a]	Social function (SF-36)	▲ [a]	Bodily pain (SF-36)	▼ [a]
					Ever diagnosed bronchitis	▼ [a]	Role emotional (SF-36)	▼ [a]	Circulation problems	▼ [a]
									Eczema	▼ [a]
Lloyd et al 2008	B								Blood pressure	▲ [a]
Shortt et al 2007	B	▲			Asthma	▲	Stress/ mental illness	▲ [b]	Angina	▲
					Chest infect'n/	▼			Arthritis / rheuma-	▼

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

					bronchitis				tism	
					Pneumonia/ hypothermia (prevalence)	▼			'other illness	▲
Somerville et al 2000 *(children*	B	▲			Cough by day	▲ [b]			Diarrhoea	◄► [b]
					Cough by night	▲ [b]				
					Wheeze by night	▲ [b]				
					Breathless with exercise	▲ [b]				
					Breathless	▲ [b]				
					Runny nose	▲ [b]				
					Blocked nose	▲ [b]				
					Hay fever	◄► [b]				
Hopton et al 1996 *(children)*	B	▲			Persistent cough	▲	Feeling down	▲	Mean number of symptoms	V
					Runny nose	▲	Irritability	▼	Tiredness	▼
					Wheezing	▼	Temper Tantrums	▲	Aches & pains	▼
									Vomiting	▲
									Fever	▲

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

									Headaches	▲
									Poor appetite	▲
									Diarrhoea	▲
									Earache	▲
									Sore throat	▼
Allen 2005	C						GHQ score	▲ [b]		
Allen 2005 a	C		Physical component SF-36	▼ [b]			Depression (HADS)	▲ [b]		
							Anxiety (HADS)	▲ [b]		
							SF-36 Mental component	▲ [b]		
Health Action Kirklees Calderdale & Wakefield 2005	C								Improvement in medical condition	[b]
Iversen et al 1986	C				Dry throat	▲ [b]			Rheumatic symptoms	▲ [b]
									Neck/back pain	▲ [b]
									Eye irritation	▲ [b]
Intervention: Rehousing/retrofitting +/- neighbourhood improvement (post 1995)										

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

Kearns et al 2008	A	▲	Good health	▲	Wheeze in past year	▼	Mental health (SF-36)	▲	Smoker	▼
			Long standing illness	▲			Vitality (SF-36)	▲	Heavy drinker	▲
			Health improved since last year	▲			Social function (SF-36)	▲	5+ fruit & veg/day	▲
							Role emotional (SF-36)	▲	Walked recently in n'hood	V
Kearns et al 2008 ***(subgroup:*** *Some/ No improved dwelling condition)*			Physical functioning (SF-36)	▲			Mental health (SF-36)	◄► [a]		
							Vitality (SF-36)	◄► [a]		
							Social function (SF-36)	<> [a]		
							Role emotional (SF-36)	◄► [a]		
Kearns et al 2008 ***(subgroup:*** *Some/ No improved dwelling suitability)*							Mental health (SF-36)	▲		
							Vitality (SF-36)	▲		
							Social function (SF-36)	▲		

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

							Role emotional (SF-36)	▲		
Kearns et al 2008 *(children)*	A	▲			Asthma	▼			Sleeping problem	▼
					Bronchitis	▲				
					Breathlessness	▼			Eczema	▼
					Persistent cough	▼			Chronic illness	▼
					Sinus/ Catarrh	▲			Indigestion	▲
					Hay fever	▲			Headaches	▲
Thomson et al 2007	A	▲	Fair/poor health	▼ [a]			Mental component (SF-36)	◄► [a]		
			SF-36 Physical component	▲ [b]						
Critchley et al 2004	A		General health (SF-36 domain)	<> [b]			Mental health (SF-36 domain)	<> [b]		
Critchley et al 2004 ***(subgroup:*** *No/ Some improvement*			SF-36 domains	[a]			Energy & vitality (SF-36 domain)	<> [a]		

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

in SAP)										
Thomas et al 2005	B	<>					GHQ-12	▼ [b]		
Thomas et al 2005 ***(sub group:*** *No/ Some housing improvement)*							GHQ-12	▼ [b]		
Barnes et al 2003	B	◄ ►	Fair/poor health	▲			Anxiety/ depression	▲	Pain & discomfort	▲
			Mobility problems	▲			Optimism for future better than 1 year ago	▲		
			Health problems affecting daily activities	▼						
			Health problems better compared to 1 yr ago	▲						
Evans et al 2002	B	<>	General health (SF-36 domain)							
			Physical function (SF-36)	V						
Breysse et al 2011	C	▲	Health better since intervention	◄ ►	Asthma	▲			Injuries	◄ ►
					Non-asthma	▲				

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

					respiratory symptoms					
Breysse et al 2011 (children)		▲	Health better since intervention	◄►	Asthma symptoms	◄►			Injuries	▼
					Non-asthma respiratory symptoms	▲				
Molnar et al 2010	C								Functional limitation	<>[b]
									Hypertension	V [b]
									Thrombosis	<>[b]
									Varicositas	<>
Molnar et al 2010 *(children)*									Epilepsy	<>[b]
									Brain tumour	<>[b]
									Spinal hernia	<>[b]
									Scabie/ louse/ impetigo	[b]
Blackman et al 2001	C	<>	Health 'not good'	▼ [b]	Chronic respiratory	▼ [b]	Mental health problem	▲ [b]		

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

					condition				
					Acute respiratory condition	▼ [b]			
Blackman et al 2001 *(children)*	C	<>	Parent reported good health	▲ [b]	Parent reported chronic respiratory condition	▼ [b]	Parent reported mental health problem	▲ [b]	
					Parent reported acute respiratory condition	▲ [b]			
Wells 2000	C	▲			Parent reported acute respiratory condition	▲ [b]	PERI	▲	
Ambrose 1999	C	▲			Asthma / bronchial condition	▲ [b]	Stress/ depression	▲ [b]	Aches & pains	▲ [b]
					Cough/ cold	▼ [b]			Dietary/ digestive problem	▼ [b]
									Illness episodes per day	V [b]
Halpern 1995	C						Depression (HADS)	▲ [b]		

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

							Anxiety (HADS)	▲ [b]		
Intervention: Provision of basic housing needs/low or middle income country intervention										
Spiegel et al 2003	C		Self-reported health	◄ ►						
Rojas de Arias 1999 *(housing improvement only group)*	B	▲							Triatomine +ve	▲ [b]
Rojas de Arias 1999 (**subgroup:** male, housing improvement only)									Triatomine +ve	▼ [b]
Rojas de Arias 1999 ***(subgroup: female, housing improvement only)***									Triatomine +ve	▲ [b]
Aziz et al 1990 * *(children)*	C								Diarrhoea episodes in past year	▲
									Dysentery incidence	▲
									Height for age	
									Weight for age	V

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

									Height for weight	V
Aziz et al 1990 * *(children)* ***(s group: don't use lat for de cation)***									Diarrhoea episodes in past year	▲
									Height for age	▲
									Weight for age	▲
									Height for weight	▲
Intervention: Rehousing from slums (pre 1970)										
Wilner et al 1960	A						Positive mood	▲ [a]	Disability	▼ [a]
							Nervousness	▲ [a]	Illness episodes	[b]
							Optimism	▲ [a]		
							Satisfaction with status quo	▲ [a]		
Wilner et al 1960 ***(subgroup:*** *no/ some/ consider housing proveme*							Satisfaction with status quo			
							Optimism			
							Feel better than 5 yrs ago			
McGonigle et al 1936 *	C								Mortality rates (adult)	V

Table 10. Visual summary of effect direction for individual outcomes (correct version available from author) (*Continued*)

									Infant mortality rate	
									Children died	
Chapin 1938	C	<>					Morale	[b]		
Chapin 1938 ***(subgroup:*** *no/some reduction in overcrowding)*							Morale	<>[a]		

* area level data not relating only to study population
Effect direction: upward arrow= positive health impact, downward arrow= negative health impact, sideways arrow= mixed effects/conflicting findings
Sample size: Final sample size (individuals) in intervention group Large arrow >300; medium arrow 50-300; small arrow <50
Statistical significance: Black arrow p<0.05; grey arrow p>0.05; empty arrow= no statistics/data reported
Statistical tests: Controlled studies- Difference between control and intervention group at follow-up (unless stated); [a] Difference in change between control and intervention group; [b] Change within intervention group only; [c] Regression identifying predictor of change: Uncontrolled studies: Change since baseline
Outcomes reported for adults unless stated

Important formatting features re size and colour of arrows have not been imported with this table- correct version available from author

Table 11. Follow-up times where more than once (since intervention unless stated)

Study	Study quality	1	2	3	4
Warmth & energy efficiency studies					
Experimental studies					
CHARISMA 2011	A	3 months	**11 months**		
Non-experimental studies					
Platt 2007 (since baseline)	A	1 years	**2 years**		

Table 11. Follow-up times where more than once (since intervention unless stated) (*Continued*)

Iversen 1986	C	1-4 months	2-5 months	**3-6 months**	
Rehousing/retrofitting					
Non-experimental studies					
Kearns 2008	A	9-12 months (no control group data)	**21-24 months**		
Barnes 2003	B	6 months	12 months	**18 months**	
Breysse 2011	C	1-4 months	**12-18 months**		
Wells 2000	C	5-12 months	**2-3 years**		
Provision of basic housing needs/low or middle income country					
Non-experimental studies					
Aziz 1990	C	1-2 years	**2-3 years**	8-9 years	
Rehousing from slums					
Non-experimental studies					
Wilner 1960 (since baseline)	A	9 months	11 months	13 months	15/16/**18 months** (<1 year since intervention)

Bolded times indicate timepoint prioritised in narrative synthesis. All data extracted and reported in full data extraction (see Appendix 2)

Table 12. Summary of standardised effect estimates

Study	Study size Int/Con (Time since intervention)	Study grade	Specific outcome	Odds ratio for intervention group (95% CI)
Intervention: Warmth & Energy Efficiency improvements (post 1985)				
General health*: Experimental studies (n=2)*				
Howden-Chapman 2008(children)	175/174 (4-5 months)	**A**	Poor/fair self-reported health	0.480 (0.310 to 0.740)*** *adj*
Howden-Chapman 2007	1689/1623 (<1 year)	**A**	Poor/fair self-reported health	0.589 (0.467 to 0.743)*** *adj*

Table 12. Summary of standardised effect estimates (*Continued*)

Respiratory*: Experimental studies (n=3)*				
Howden-Chapman 2008 (children)	175/174 (4-5 months)	**A**	Sleep disturbed by wheeze	0.550 (0.350 to 0.850)*** *adj*
			Speech disturbed by wheezing	0.690 (0.400 to 1.180) *adj*
			Dry cough at night	0.520 (0.320 to 0.830)* *adj*
			Wheeze during exercise	0.670 (0.420 to 1.060) *adj*
Barton 2007 (adults & children)	193/254 (<2 years)	**A**	Asthma	~0.946 (0.598 to 1.496)
			Bronchitis	~1.007 (0.477 to 2.127)
			Other respiratory symptoms	~1.010 (0.560 to 1.820)
Howden-Chapman 2007	965/961 (<1 year)	**A**	Morning phlegm	0.640 (0.523 to 0.784)*** *adj*
Howden-Chapman 2007 (children & adults)	1689/1623 (<1 year)	**A**	Wheezing in past 3 months	0.570 (0.467 to 0.696)*** *adj*
			Cold/flu	0.545 (0.430 to 0.691)*** *adj*
Howden-Chapman 2007 (children)	512/471 (<1 year)		Sleep disturbed by wheeze	0.570 (0400 to 0.812)** *adj*
			Speech disturbed by wheezing	0.514 (0.310 to 0.852)* *adj*
Respiratory: *Non-experimental studies (n=2)*				
Platt 2007	1281/1084 (1-2 years)	**A**	Ever diagnosed nasal allergy	1.520 (1.050 to 2.200)* *adj*
			Ever diagnosed asthma	0.92 (0.63 to 1.34) *adj*
			Ever diagnosed bronchitis	1.29 (0.97 to 1.72) *adj*
Shortt 2007	46/54 (1-3.5 years)	**B**	Asthma ∓	~0.568 (0.099 to 3.254)
			Chest infection/bronchitis ∓	~1.875 (0.495 to 7.102)
			Pneumonia/hypothermia ∓	~3.593 (0.143 to 90.361)
Hopton 1996 (children)	55/77 (5-11 months)	**B**	Persistent cough	~0.973 (0.441 to 2.149)

Table 12. Summary of standardised effect estimates (*Continued*)

			Runny nose	~0.686 (0.337 to 1.394)
			Wheezing	~1.125 (0.467 to 2.708)
Mental health*: Experimental studies (n=1)*				
Howden-Chapman 2007	977/964 (<1 year)	**A**	Low happiness (SF-36)	0.560 (0.409 to 0.767)*** *adj*
		A	Low vitality (SF-36)	0.510 (0.408 to 0.637)*** *adj*
Mental health*: Non-experimental studies (n=2)*				
Braubach 2008	~210/165 (5-8 months)	**A**	Depression	1.404 (0.329 to 5.987)
Shortt 2007	46/54 (1-3.5 years)	**B**	Stress/Mental illness	~0.261 (0.053 to 1.299)
Hopton 1996 (children)	55/77 (5-11 months)	**B**	Feeling down	~0.663 (0.233 to 1.891)
			Irritability	~1.545 (0.569 to 4.196)
			Temper tantrums	~0.973 (0.441 to 2.149)
Illness/symptom*: Experimental studies (n=2)*				
Howden-Chapman 2008 (children)	175/174 (4-5 months)	**A**	Diarrhoea	0.720 (0.450 to 1.160) *adj*
			Ear infection	1.160 (0.680 to 1.990) *adj*
			Vomiting	0.880 (0.550 to 1.400) *adj*
			Twisted ankle	1.86 (1.03 to 3.35)*
Barton 2007	193/254 (<2 years)	**A**	Arthritis	~1.058 (0.533 to 2.100)
			Rheumatism	~1.908 (0.829 to 4.395)
Illness/symptom*: Non-experimental studies (n=2)*				
Platt 2007	1281/1084 (1-2 years)	**A**	Ever diagnosed hypertension	0.770 (0.610 to 0.972)* *adj*
			Ever diagnosed heart disease	0.690 (0.520 to 0.916)* *adj*
			Ever diagnosed circulation problem	1.06 (0.83 to 1.34) *adj*

Table 12. Summary of standardised effect estimates (*Continued*)

			Ever diagnosed eczema	1.43 (0.89 to 2.28) [adj]
Shortt 2007	46/54 (1-3.5 years)	**B**	'Other' illnesses [∓]	~0.568 (0.099 to 3.254)
			Arthritis [∓]	~1.619 (0.343 to 7.641)
			Angina [∓]	~0.200 (0.041 to 0.966)*
Hopton 1996 (children)	55/77 (5-11 months)	**B**	Aches & pains	~1.537 (0.664 to 3.555)
			Diarrhoea	~0.735 (0.254 to 2.123)
			Earache	~0.977 (0.347 to 2.749)
			Fever	~0.784 (0.328 to 1.875)
			Headaches	~0.681 (0.233 to 1.986)
			Poor appetite	~0.342 (0.146 to 0.803)**
			Sore throat	~1.355 (0.668 to 2.747)
			Vomiting	~0.963 (0.380 to 2.443)
			Tiredness	~1.524 (0.644 to 3.607)
Intervention: Rehousing/Retrofitting +/- neighbourhood renewal (post 1995)				
General health*: Non-experimental studies (n=3)*				
Kearns 2008	262/284 (2 years)	**A**	Self-reported poor health	0.769 (0.500 to 1.176) [adj]
			Long standing illness	0.680 (0.440 to 1.050) [adj]
			Health not improved since 1 year ago	0.787 (0.541 to 1.163) [adj]
Thomson 2007	50/50 (1 year)	**A**	Fair/poor health	1.757 (0.777 to 3.973)
			Lower SF-36 Physical Component Score	0.960 (0.437 to 2.110)
Barnes 2003	45/45 (18 months)	**B**	Fair/poor health	~0.273 (0.110 to 0.682)*
			Health somewhat/much worse than 1 year ago	~0.356 (0.135 to 0.942)

Table 12. Summary of standardised effect estimates (*Continued*)

			Health interferes with daily activities	-1.516 (0.617 to 3.730)
			Physical/emotional problems with daily life (in past 4 weeks)	-0.338 (0.138 to 0.829)
Respiratory*: Non-experimental studies (n=1)*				
Kearns 2008	262/284 (2 years)	**A**	Wheezing in past year	1.040 (0.690 to 1.560) *adj*
Kearns 2008 (children)	221/208 (2 years)	**A**	Asthma	1.039 (0.650 to 1.661)
			Breathlessness	1.185 (0.459 to 3.063)
			Persistent cough	1.093 (0.663 to 1.800)
			Bronchitis	0.311 (0.032 to 3.010)
			Sinus/catarrh	0.890 (0.480 to 1.650)
Mental health*: Non-experimental studies (n=2)*				
Thomson 2007	50/50 (1 year)	**A**	Lower SF-36 Mental Component Score	0.733 (0.333 to 1.613)
Barnes 2003	45/45 (18 months)	**B**	Anxiety/Depression self reported	-0.361 (0.152 to 0.856)*
Illness/symptom*: Non-experimental studies (n=2)*				
Kearns 2008	262/284 (2 years)	**A**	Smoker	1.470 (0.849 to 2.546) *adj*
			Heavy drinker	0.610 (0.300 to 1.240) *adj*
			Less than 5 portions fruit/veg per day	0.794 (0.519 to 1.215) *adj*
Kearns 2008 (children)	221/208 (2 years)	**A**	Chronic illness	1.039 (0.549 to 1.966)
			Headaches	0.991 (0.604 to 1.626)
			Indigestion	0.941 (0.058 to 15.145)
			Sleeping problems	1.128 (0.618 to 2.059)
			Eczema	1.148 (0.683 to 1.931)

Table 12. Summary of standardised effect estimates (*Continued*)

			Hay fever	0.990 (0.513 to 1.913)
Barnes 2003	45/45 (18 months)	**B**	Pain & discomfort	~0.400 (0.170 to 0.940)
			Mobility	~0.533 (0.215 to 1.322)
Mental health*: Non-experimental studies (n=1)*				
Wilner 1960	1891/2893 (<1 year)	**A**	Nervousness	~1.157 (0.890 to 1.504)
			Negative mood	~0.912 (0.704 to 1.182)
			Dissatisfaction with status quo	~0.863 (0.663 to 1.122)
			Potency (nothing can be done to improve situation	~0.814 (0.628 to 1.055)
			Pessimism	~0.815 (0.628 to 1.056)
			Emotionality (not able to control of temper)	~0.796 (0.613 to 1.034)
Illness/symptom*: Non-experimental studies (n=1)*				
Wilner 1960	1891/2893 (<1 year)	**A**	At least 1 day disability	~1.145 (0.977 to 1.342)

* p<0.05, ** p<0.01, *** p<0.001 $\overline{+}$ proportion of households as opposed to individuals ^{adj} adjusted for key confounders (listed in data & analysis section)

Inadequate control for confounding Grade C/key confounder emerged in analysis

~ estimated OR as no indication of missing data for specific outcomes, or estimated sample size

WHAT'S NEW

Last assessed as up-to-date: 13 August 2012.

Date	Event	Description
27 February 2013	Amended	Formatting of Table 9 changed - no new information added

CONTRIBUTIONS OF AUTHORS

HT was the lead review author and led all aspects of the review. ST and ES are co-reviewers and screened, critically appraised, extracted data, and approved the final synthesis for the review. MP advised 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 editorial advisor, and HT is an editor on the Cochrane Public Health Group (but not involved in the editorial approval of this review). HT and MP are authors on two of the included studies in this review (Kearns 2008; Thomson 2007) and are also involved with one of the ongoing studies (GoWell).

SOURCES OF SUPPORT

Internal sources

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

External sources

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

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

The changes described here are marked in the text or appendices where relevant by an asterisk.

Terms used to describe study designs

The terms controlled before and after, uncontrolled before and after, cross-sectional controlled before and after, and cross-sectional uncontrolled before and after were used to describe study designs in preference to prospective controlled or prospective uncontrolled (see Appendix 1).

Assesment of study quality: quantitative studies

Some minor clarifications were made to the Hamilton quality assessment tool (selection bias, study design, and overall grade) to improve clarity and to reflect the new terms used to describe study design (Appendix 7).

Assesment of study quality: qualitative studies

Following examination of some appraisal tools for qualitative research it was agreed that a brief tool to enable a systematic and independent assessment by two reviewers of study quality, which allowed for diverse methods and study approaches, was required. We adapted a series of prompts (Appendix 8) used in a previous review of tobacco control (Thomas 2008). The tool was developed by a team in the ESRC Research Methods Programme following extensive discussion within a multi-disciplinary team and evaluation of two existing appraisal tools (Dixon-Woods 2004).

Included and excluded interventions

Studies which assessed changes in direct health outcomes following installation of mechanical ventilation heat recovery (MVHR housing improvement intervention) were excluded. These studies did assess changes in direct health outcomes but while MVHR may result in small improvements in domestic warmth, MVHR is primarily aimed at improving air quality. These studies assessed the health impacts among asthmatic occupants. Two earlier Cochrane reviews (Gøtzsche 2008; Singh 2002) have focused on the health impacts of allergen reduction and air quality improvement among atopic and asthmatic groups and for these reasons this intervention was excluded from this review. Three studies of MVHR were identified and excluded from the review (Kovesi 2009; Warner 2000; Wright 2009).

Synthesis of qualitative data

The qualitative studies were grouped according to the intervention categories developed for the quantitative studies, reflecting intervention type, context, and time period. Following agreement between the two review authors of the data extraction for the qualitative studies a logic model mapping the impacts and links between impacts reported in the qualitative data was prepared independently by two review authors (ST and HT). The two logic models were then compared and discussed to resolve any discrepancies before preparing a final logic model to represent the nature of the impacts and links between impacts emerging from the qualitative data.

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), 2004. The modified protocol for this current review, co-published with The Campbell Collaboration, was published on *The Cochrane Library* in September 2010. A version of this review appears on the Campbell Library in Issue 2, 2013.

INDEX TERMS

Medical Subject Headings (MeSH)

*Health Status; Health Promotion [*methods]; Heating [standards]; Housing [*standards]; Mental Health; Quality Improvement [*standards]; Respiration Disorders [rehabilitation]

MeSH check words

Humans