Centre for Homelessness Impact

Discharge programmes for individuals experiencing, or at risk of experiencing homelessness: a systematic review

Jennifer Hanratty, Sarah Miller, Jayne Hamilton, Ciara Keenan, Christopher Coughlan

Campbell UK & Ireland

Centre for Evidence and Social Innovation

Queen's University Belfast

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QUEEN'S UNIVERSITY BELFAST

August 2020

About the Centre for Homelessness Impact

The Centre for Homelessness Impact champions the creation and use of better data and evidence for a world without homelessness. We are a member of the UK What Works Network and launched in 2018 to act as a catalyst for evidence led change to enable people working in and around homelessness to achieve breakthrough results.

About Campbell UK & Ireland

Campbell UK & Ireland is a national centre of the international Campbell Collaboration, established and hosted by the Centre for Evidence and Social Innovation at Queen's University Belfast. Our role is to promote the work of the Campbell Collaboration across the UK and Ireland.

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Foreword

The Centre for Homelessness Impact launched in 2018 to act as a catalyst for evidence-informed change in the homelessness field. Our focus is on transforming policy and practice, and an important foundation of that work is to help identify the most pressing problems in the field and then putting the best evidence available on those issues at people's fingertips.

But if evidence is to inform policy and practice decisions, it is vital that synthesis is of high quality. This is why we invest in systematic reviews such as this one which is essentially a study of studies on discharge models. The benefit of this type of review is that it is a one-stop shop summary of the evidence about a research question. In the Pyramid of Evidence Based Practice, a Systematic Review of Randomized Control Trials is located at the top; because so many studies are used, it greatly reduces bias. By ensuring methodological quality, systematic reviews reduce the risk of bias as well as improve the reliability and accuracy of conclusions. At the Centre we also follow internationally recognised methods for systematic reviews, guaranteeing they meet the highest quality standards

This systematic review explores the existing evidence of the effectiveness of discharge programmes for people living institutional settings such as in-patient health facilities, the armed forces, and prison. Even if the number of studies included is small, of mixed methodological quality and exclusively from the USA, it underscores that discharge programmes can be effective at reducing homelessness.

There is still much to be learnt around discharge programmes and their impacts on outcomes such as employment and wellbeing, the key traits that make them more effective to support people leaving different institutional settings such as the armed forces, and particularly understand the implications of these programmes in other settings and different policy environments like the UK.

This is only the start, but it is a promising one. As shown by this report, systematic reviews are a valuable tool to summarise the evidence base and provide recommendations both for policy and further research. The Centre, in collaboration with other key partners such as the Campbell Collaboration, will continue to fund a programme of work to put the best available evidence at the fingertips of decision-makers. Our aspiration is that harnessing this existing evidence and highlighting where the gaps are, will be a significant contribution to bring about a much needed evidence-informed change to the homelessness sector.

Dr Lígia Teixeira

Chief Executive
Centre for Homelessness Impact

Executive summary

What is this review about?

Discharge from institutions (e.g. in-patient health services, mental health institutions, armed forces, prison) is recognised as a major cause of homelessness, thus there is a significant need to identify and implement effective policies and interventions, and discontinue ineffective practices in order to reduce homelessness. People who have spent time in an institutional setting may have a higher homelessness risk. They might have been homeless previously, or their accommodation arrangements may break down or become unsuitable by the time they are discharged. They may also have existing challenges to their health and wellbeing which increases the risk of poor outcomes if discharged into homelessness, unstable housing or accommodation that is no longer suitable for their needs.

This systematic review of quantitative and qualitative evidence summarizes i) the findings of evaluations of the effectiveness of discharge programmes intended to improve housing stability and health, but also explores impacts on access to services, crime, cost, employment and income; and ii) the finding of the evaluations on the barriers and enablers for the implementation of these programmes.

Which studies are included in this review?

This systematic review uses evidence already identified in two existing evidence and gap maps (EGMs) commissioned by the Centre for Homelessness Impact (CHI). The search for the EGMs, and thus, for this review, was conducted in September 2018.

The study reports findings from 13 studies of effectiveness, eight are randomised control trials (RCTs) and five are non-randomised control trials (N-RCTs). Eight studies concerned discharge from health facilities, three discharge from prison, and one study each for leaving an addiction clinic and children leaving care. All studies are from the United States, and thus we cannot directly assume that the effects achieved by programmes that are designed and delivered in one country or even state, with its own specific policy, economic, health, justice and social care context, will automatically transfer to another country.

It is not possible to say with any certainty whether the effects observed in this review would translate into similar effects here in the UK, or elsewhere in the world, given the differing social and economic contexts. The studies cover a broad range of discharge programmes acknowledging the variety of institutions that individuals can be discharged from and the complexity of needs each programme aims to address.

The methodological quality of the studies included in this review was mixed (only 31% of studies were rated as having low risk of bias, the remainder were rated as moderate to high risk of bias). Given the small number of studies overall and the variability in rigorous study design as well as methodological quality, it is further reason to be circumspect about the findings of the review.

From 35 potentially relevant studies related to discharge interventions contained in the implementation evidence and gap map, 10 were selected through purposive sampling, which concern discharge from the armed forces (5 studies), hospitals (3 studies), prison (1 study), and children in care (1 study). There are four studies each from United Kingdom and United States, two in Australia and one from Canada.

Do discharge programmes improve housing stability, health and other outcomes?

Considering the caveats above, this review provides suggestive evidence that discharge interventions can be promising. Based on the 13 impact evaluation studies conducted in the United States, the review concludes found that these interventions were likely to improve housing stability (evidence from five studies) and reduce the number of hospitalisations (five studies). There is also some suggestive evidence that they could reduce incarcerations (four studies), but the results remain imprecise given uncertainty around the results. The relevant studies included did not report information on other outcome domains including wellbeing, access to services, employment and income. Moreover, caution is granted because the number of deaths (four studies) was slightly higher in the intervention group but the results remain very uncertain and require additional exploration.

Which are the key considerations for implementation of discharge programmes?

The review also sheds light on key considerations when implementing these policies, emphasising the importance of communication and coordination between all service providers, the need for a clear written discharge plan prepared in consultation with the person, and the need to make the right training and experience available to case-workers, so they are able to deliver tailored support.

Implications for policy, practice and research

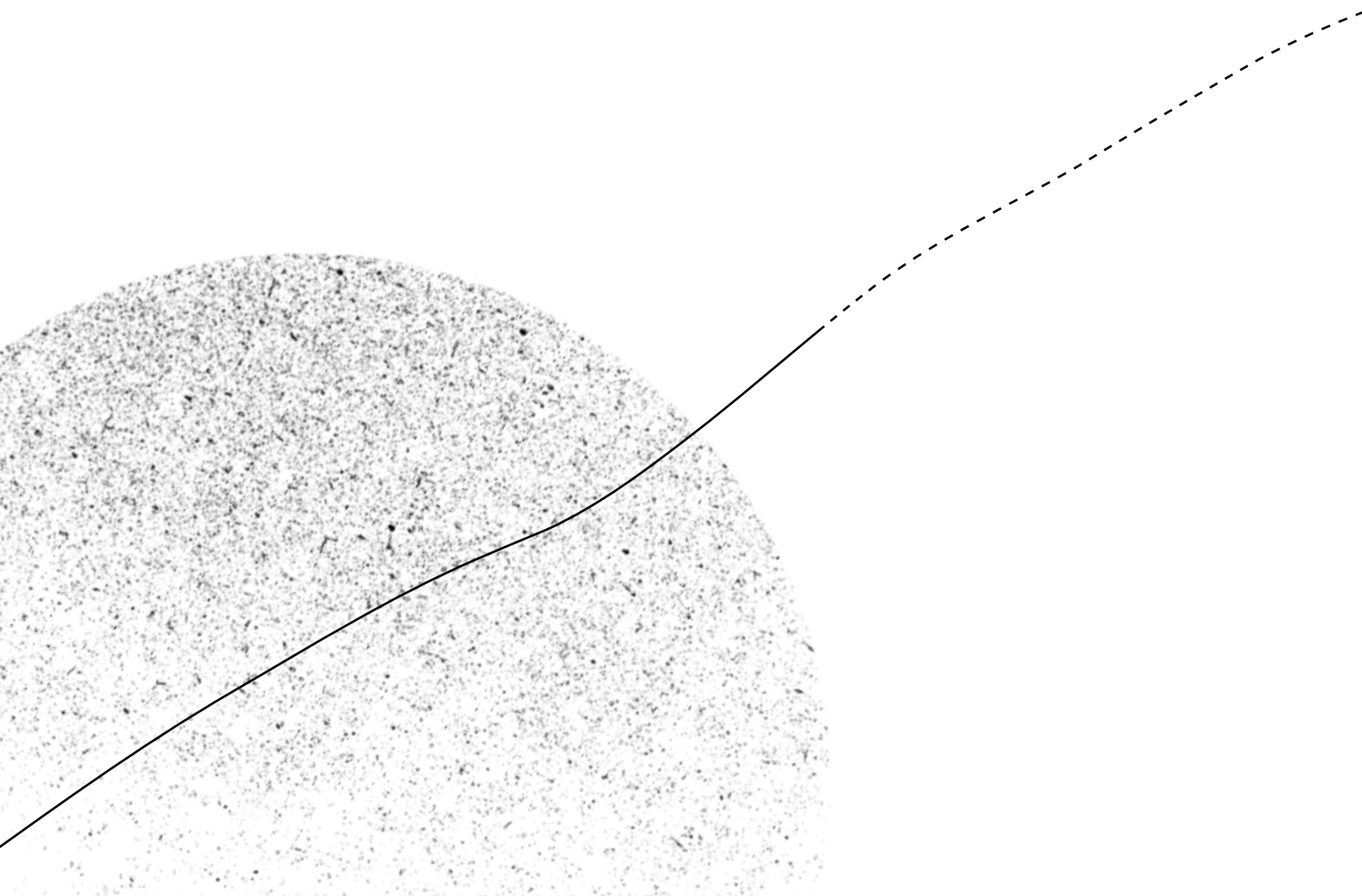
This is the first review to look specifically at the effectiveness of discharge programmes for people who are homeless or at risk of becoming homeless. Therefore, even though the findings are based on a small number of studies, it is currently the best available evidence of the effectiveness of such approaches for improving outcomes for people experiencing homelessness. The results are encouraging as they show that discharge programmes can be effective in reducing homelessness and hospitalisations and may be effective in reducing reincarceration post-discharge. However, the evidence is of mixed methodological quality, exclusively from the USA and limited to only a few outcome domains. Thus, more, high quality research is needed to improve the evidence base for the effectiveness of discharge programmes. This additional evidence should consider the impacts in wider domains beyond housing stability and health, assess the costs of different alternatives, and be adapted to different policy environments beyond the practices implemented in the USA. A more nuanced understanding of local conditions and policies remains an important area that requires exploration.

Given the broad range of institutional settings for which discharge is an issue, the evidence base remains thin. None of the effectiveness studies refer to the military, and there is only one such study for children leaving care. Thus, further evaluations need to explore the effectiveness of programmes designed for

people leaving different types of institutional settings.

Discharge programmes can work, however more research is needed to understand how discharge programmes might also work in a UK context. However, for those implementing them, some key features to consider are including individualised planning, planning before discharge where possible, joint working between agencies with clear lines of accountability to ensure that no one falls through the cracks.

Unfortunately, there were insufficient studies to explore whether the institutional setting people were discharging from had an impact on the effectiveness of the programme. We do not know which settings are likely to produce greatest effects from these programmes. We also were unable to analyse differential effects on different groups of people (such as men and women, younger and older people) so we do not know who these programmes may work best for. All these should be reflected prominently in further research exploring the effectiveness of discharge interventions.

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01

Background

The problem, condition or issue

People who have spent time in an institutional setting, such as prison or in-patient health services, may be at risk of homelessness upon discharge from the institution.

This might be because they were homeless before entering an institutional setting or because previous accommodation arrangements have broken down or are now unsuitable. Those leaving institutional settings are likely to have existing challenges to their health and wellbeing and so this population is especially at risk of poor outcomes if discharged into homelessness, unstable housing or accommodation that is no longer suitable for their needs. This review synthesises the available evidence on programmes aimed at preventing or reducing risk of homelessness for people leaving institutional settings.

Extent of the problem and associated problems

In this review institutional settings refer to any setting where an individual's accommodation is provided by the institution, but provision of accommodation is not the purpose of the institution. Settings can include, but are not limited to, prison, in-patient treatment (for physical or mental health care, addiction treatment), military and youth ageing out of care. Those who have been residing in an institutional setting are known to be at higher risk of homelessness upon discharge than the general population. For example, in the USA, between 31% and 46% of youth ageing out of foster care had been homeless at least once by age 26, compared to just 4% of the general population (Dworsky, Napolitano, & Courtney, 2013). A Canadian study of discharge from psychiatric hospital found that 10.5% of people were discharged into homelessness (Forchuk, Russell, Kingston-MacClure, Turner, & Dill, 2006). Discharge to inappropriate accommodation harms recovery and is a major cause of readmission (Diggle, Butler, Musgrove, & Ward, 2017).

Similarly, those discharged from prison are at higher risk of homelessness, may have restrictions on where they can and cannot live and face difficulties in accessing accommodation because of their criminal record. In the UK one third of prisoners said they had "nowhere to go" when leaving prison (Centre for Social Justice, 2010) and both homelessness prior to incarceration and on discharge have been linked to elevated rates of reoffending (Cooper, 2013)Interventions designed to prevent homelessness in this population aim to interrupt this cycle of incarceration, homelessness and reoffending.

Depending on the institutional setting people have been residing in, different groups of people are likely to have different needs. For example, those discharged

from in-patient addiction treatment are likely to need a stable, drug-free living environment. Whereas youth ageing out of care may need structured practical tapering support to enable them to become independent adults. There are also many individuals with multiple risk factors and complex needs, placing them at even higher risk of homelessness and associated negative outcomes. Discharge into shelter accommodation where overcrowding, lack of privacy, drug use and/or violence may be common, is not suitable for anyone, but can be detrimental for a person with multiple complex needs. After discharge from institutions, unstable or unsuitable living conditions can contribute to relapse, recidivism, deterioration in health and readmission to hospital.

The Intervention

Discharge programmes involve the coordination and provision of services, including accommodation, for people upon discharge from institutions.

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

The programmes currently in use in high-income countries adopt a variety of approaches with different levels of complexity. Programmes primarily seek to address housing needs, either through maintaining previous housing arrangements prior to entry into the institution or to seek new suitable accommodation. Programmes may also offer continued support prior to and following on from discharge, to ensure the person's housing situation is suitable and sustainable. This could be in the form of paying rent for the individual or facilitating family/partner contact to maintain relationships during time away from home. For example, one simple intervention in a prison context is supporting contact with family to maintain relationships so the person has a home to return to on release. Other, more complex models involve the coordination of multiple agencies to enhance the continuity of care and support a person to access services. For example, Herman et al. (2011) studied Critical Time Intervention (CTI) offered to people at risk of homelessness upon discharge from psychiatric

hospital. CTI offers care coordination along with direct emotional and practical support over nine months during the critical discharge period. Another example is a 'transition of care' model, where hospital settings work together with community health and social care colleagues, housing organisations and the voluntary sector to plan for a person's discharge and effectively communicate with each other to facilitate a smooth transition from the institution to community living with the goal of reducing the need for re-admission.

How the intervention might work

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

Why was it important to do this review?

There is a significant need to identify and implement effective policies and interventions and discontinue ineffective practices in order to reduce homelessness and the associated negative effects on individuals. Discharge from institutions is recognised as a major cause of homelessness. People who are approaching the transition from an institutional setting back into the community may be particularly at risk of homelessness on discharge. To ensure that policymakers avail of the most robust and rigorous evidence to date a systematic review of the literature on interventions aimed at reducing risk and/or incidence of homelessness for this vulnerable population was needed.

This systematic review was based on evidence already identified in two existing evidence and gap maps (EGMs) commissioned by the Centre for Homelessness Impact (CHI) and built by White, Saran, Teixeira, Fitzpatrick & Portas. The EGMs presented studies on the effectiveness and implementation of interventions aimed at people experiencing, or at risk of experiencing, homelessness.

The EGMs were constructed using a comprehensive search strategy including searching 17 academic databases, three EGM repositories, seven systematic review databases, web searches for grey literature, searching reference lists and contacting experts to identify relevant studies. The map identified one related systematic review, that focused on people with severe mental illness, and is not a review of the effectiveness of discharge programmes. One possibly overlapping review was by Chambers et al, (on housing interventions for 'vulnerable adults'. While there may be some overlap, our review focused on discharge programmes specifically and included any individuals at risk of homelessness, not limited to only vulnerable adults. Our review was also unique in that we included evidence on both effectiveness and implementation, including qualitative data, to develop a comprehensive synthesis of the existing evidence on which programmes can work, for whom, under what circumstances, alongside a synthesis of the evidence on common barriers and facilitators for effective implementation.

Objectives

The objectives of this systematic review were to investigate:

1. What is the effect of discharge programmes on outcomes for individuals experiencing or at risk of experiencing homelessness?
2. Do the effect of discharge programmes differ depending on;
    - **i.** The institutional setting people are discharged from e.g. prison, hospital, substance abuse treatment?
    - **ii.** Complexity of needs?
    - **iii.** Age?
    - **iv.** The presence of dependent children - are outcomes different for families compared to single individuals?
5. What implementation and process factors impact on programme effectiveness (qualitative synthesis)?
6. Was effectiveness of an intervention related to how well and how faithfully the intervention was delivered?


THE CALCULUS
THE PEOPLE'S ALMANAC
معماری ایرانی
HOMO SAPIENS
GENERAL RELATIVITY
GLACIER PROJECT PAPERS
THE WHITE ISLAND
GRAY'S ANATOMY

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Methods

This systematic review with synthesis of quantitative and qualitative data is based on evidence identified in the two Evidence and Gap Maps (EGMs) on effectiveness and implementation

Criteria for considering studies for this systematic review of quantitative and qualitative evidence

Types of studies

We included all study designs where a comparison group was used. This included randomised controlled trials (RCTs), quasi-experimental designs, matched comparisons and other study designs that attempt to isolate the impact of the intervention on homelessness using appropriate statistical modelling techniques.

As randomised controlled trials are accepted as more rigorous than non-randomised studies, we planned to explore the potential impact of a non-randomised study design on effect sizes as part of the analysis of heterogeneity.

Studies were eligible for inclusion in the review if they included an inactive comparison condition, for example:

- No treatment.
- Treatment as usual where people received their normal level of support or intervention.
- Waiting list where individuals or groups were randomly assigned to receive the intervention at a later date.
- Attention control, where participants received some contact from researchers but both participants and researchers were aware that this is not an active intervention.
- Placebo where participants perceived that they are receiving an active intervention but the researchers regarded the treatment as inactive.

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

Types of participants

People experiencing, or at risk of experiencing, homelessness residing in an institutional setting or system, in high income countries. We included people of all ages and in any institutional setting including but not limited to; military service, social care, in-patient health care, residential treatment for addiction and prison.

People were considered to be homeless, or at risk of homelessness, if they had a significant history of homelessness prior to entry into the institutional setting or they did not have suitable and stable housing to go to following discharge.

Types of Intervention

We included any intervention targeted at people being discharged from any institutional setting, which aimed to avoid discharging into homelessness or reduce the risk of future homelessness through planning for suitable stable accommodation prior to discharge. Typically, interventions involved advanced planning prior to discharge and coordination between institutions and housing services. Some interventions could have provided ongoing support to people to enable them to access appropriate health and social care services to support their transition from an institutional setting to community living.

The control or comparison condition could have included no services/ intervention, services as usual, attention control or waiting list (see types of studies section for more detail).

Types of outcome measures

Given the breadth of possible outcomes and measurement tools that could have been used, we focused on extracting all outcome data relating to seven broad domains. If no useable data was available, we still included the study in the review but not in the meta-analysis.

Primary outcome domains

1. Housing stability
2. Health, including substance abuse, mental health, mortality, morbidity

Secondary outcome domains

3. Access to services, including appropriate ongoing community support for individual needs.
4. Crime/criminalisation
5. Employment and income
6. Capabilities and wellbeing
7. Cost of intervention
8. We also documented any unintended adverse events reported

These domains reflected the seven outcome domains used in two Evidence and Gap Maps (EGMs) commissioned by the Centre for Homelessness Impact (CHI) and built by White, Saran, Teixeira, Fitzpatrick and Portas (2018). These outcome domains were developed in consultation with an advisory group of homelessness policy-makers, practitioners and researchers.

Duration of follow-up

We included studies with follow-up of any duration and data relating to all follow-up points were extracted. We intended to conduct separate analysis for each follow up period as follows; up to one month, six months, one year, two years, more than two years post discharge. The follow-up analysis focused on time post-discharge rather than time post-intervention as interventions were likely to vary substantially in their duration and because the point of discharge is a crucial transition point.

Types of settings

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

Search methods for identification of studies

This systematic review was based on evidence already identified in two existing EGMs commissioned by CHI (White et al., 2018). The EGMs present studies on the effectiveness and implementation of interventions aimed at people experiencing, or at risk of experiencing, homelessness in high income countries.

The maps used a comprehensive three stage search and mapping process. Stage one was to map the included studies in an existing Campbell review on homelessness, stage two was a comprehensive search of 17 academic databases, three evidence and gap map databases, and seven systematic review databases for primary studies and systematic reviews. Finally stage three included web searches for grey literature, scanning reference lists of included studies and consultation with experts to identify additional literature. Sample search terms can be found in the protocol.

We did not undertake any additional searching. However, if during the course of contacting authors for additional information or data they provided us with additional eligible studies, these were included.

Similarly, if we identified additional studies or additional reports relating to included studies these were also included.

To identify studies from the maps that were eligible for inclusion in this review, two reviewers independently screened the title and abstract of all documents in the effectiveness map using EPPI Reviewer 4 software. The full text of studies that met or appeared to meet the inclusion criteria were then screened independently by two reviewers. Any disagreements were resolved in discussion with a third reviewer until a consensus was reached. The same process was applied to screening documents included in the process evaluation maps to identify studies eligible for inclusion in the qualitative synthesis. The flow of studies through the screening process is documented in a PRISMA flow chart (Figure 1).

Data collection and analysis

Description of methods used in primary research

Interventions included any study measuring the effectiveness of discharge programmes compared to a control group or well-matched comparison group.

Criteria for determination of independent findings

It was important to ensure that the effects of an individual intervention were only counted once, and the following conventions applied;

Where there were multiple measures reported for the same outcome, we planned to use robust variance estimation (RVE) to adjust for effect size dependency and implement the correction for small samples when necessary, however, as RVE is not recommended for use with fewer than 20 studies it was not appropriate here. Instead, one effect size per outcome was chosen for each study included in each separate meta-analysis. The choice of outcome was driven by the need to choose common outcome metrics across studies. We extracted all outcomes from all studies relating to the seven pre-specified outcome domains. Within each domain, we then selected the outcome measure that was most consistently reported across studies.

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

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

In the case of multiple cohorts appearing in one study we calculated a simple average for the omnibus meta-analysis. In cases where study authors separated participants into subgroups relating to age, comorbid diagnosis or gender, and it was inappropriate to pool their data, these participants remain independent of each other and are treated as separate studies which each provide unique information. If different cohorts in a study fall into different subgroups, then they are considered separately in subgroup analysis, but no overall summary of effect is calculated combining subgroups in those cases.

Selection of studies

Dual screening of the studies contained within the existing EGMs was undertaken by two independent screeners to identify studies that met the inclusion criteria, first screening titles and abstracts to remove obviously ineligible studies then screening full texts of remaining studies to identify all eligible studies. Any disagreements between screeners was resolved through discussion with the lead author.

Data extraction and management

Once eligible studies were found, we undertook dual data extraction, where two authors completed data extraction and risk of bias assessments independently for each study. Coding was carried out by trained researchers. Any discrepancies in screening or coding was discussed with senior authors until a consensus was reached.

Details of study coding categories

A coding framework was developed and piloted prior to undertaking data extraction for all included studies using EPPI Reviewer software (Appendix 1 and 2).

At a minimum, we extracted the following data: publication details, intervention details including setting, dosage and implementation, delivery personnel, descriptions of the outcomes of interest including instruments used to measure, study design, sample size of treatment and control groups, data required to calculate Hedge's g effect sizes, quality assessment. We also extracted more detailed information on the interventions such as: duration and intensity of the programme, timing of delivery, key programme components (as described by study authors), theory of change. Alongside extracting data on programme components, descriptive information for each of the studies was extracted and coded to allow for sensitivity and subgroup analysis.

This included information regarding:

- Setting, which type of institutional setting(s) are study participants transitioning from?
- Study characteristics in relation to: design, sample sizes, measures and attrition rates, who funded the study and potential conflicts of interest.
- Demographic variables relating to the participants including age, complexity of needs, dependent children, and other relevant population characteristics.

Quantitative data were extracted to allow for calculation of effect sizes (such as mean change scores and standard error or pre and post means and standard deviations or binary 2x2 tables). Data were then extracted for the intervention and control groups on the relevant outcomes measured in order to assess the intervention effects.

Assessment of risk of bias in included studies

Assessment of methodological quality and potential for bias was conducted using the second version of the Cochrane Risk of Bias tool for Randomised controlled trials. Non-randomised studies were coded using the ROBINS-I tool.

Measures of treatment effect

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

Effect sizes were calculated using r and the metaphor package via MAVIS. Where non-standard data was reported the David-Wilson Calculator (Wilson, 2019) was used to calculate effect sizes and Hozo's Formula (Hozo, Djulbegovic and Hozo, 2005) was also used to help calculate effect size when Interquartile range and Median data were provided.

Dealing with missing data

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

Data synthesis

Approach to meta-analysis

Given the diverse range of interventions, populations and settings in included studies, random effects models, using inverse-variance estimation, were used as the basis for pairwise meta-analysis. The analysis was conducted using R and the range of commands externally developed to conduct meta-analysis with R such as metafor.

Main effects

The main effects analysis, synthesising the evidence in relation to the effects of discharge programmes in general, was undertaken using the approach to meta-analysis outlined above for each primary and secondary outcome in turn, with separate analysis for follow-up of different duration (see duration of follow-up) (Objective 1).

Sensitivity analysis

For each outcome, the following sensitivity analyses were planned, to assess whether there are potential influences relating to:

1. Studies that appeared to exert an undue influence on findings.
2. Study quality (studies with a "high" or "unclear" risk of bias on 3 or more of the 7 risk of bias domains in the Cochrane Risk of Bias assessment were coded as low quality).

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

Subgroup analysis and investigation of heterogeneity

Assessment of differential effectiveness in relation to age, complexity of needs, family (dependent children) or single, institutional setting or other subgroups/ populations identified in included studies (Objective 2).

Eligible studies were coded in terms of:

- The institutional setting participants were residing in.
- Age, (under 25 or over 25).
- Complexity of needs, this was defined based on the mental health, physical health, substance use/abuse status, history of incarceration and any other relevant factors). This was coded individually to allow for analysis based on specific needs if sufficient studies and data allowed.
- Dependent children (comparing interventions for families including dependent children and individuals without dependent children).

Subgroup analyses were then planned for each of the factors above (institution, age, complexity of needs and dependent children) in relation to each of the primary and secondary outcomes. The subgroup analyses (based upon random-effects models), would group studies by sub-category and estimate overall effects sizes for each. Subgroup analyses could only be carried out if studies included in the subgroup analysis were sufficiently similar to each other in all other respects, such as whether the interventions delivered to younger and older people were similar enough to be confident that the subgroup analysis reflected differences in the effectiveness for different populations rather than different intervention effects.

Treatment of qualitative research

The qualitative research that was included in this review was drawn from evidence collated in the implementation and process EGM constructed by White et al. (2018) and White, Wood & Fitzpatrick (2018). Studies were selected through purposive sampling to represent a diverse range of institutional settings, populations and geographical locations. The papers included in the EGM had been coded in advance by researchers at Heriot-Watt, in order to categorise each paper into the domains in the map and highlighted data that spoke to that domain. We were then able use that initial coding to select studies with rich data. Studies that provided most data were selected first and additional studies added until we reached saturation. Where possible we included qualitative studies associated with the interventions evaluated in the quantitative studies, however only one such study was identified.

We included process evaluations and other relevant qualitative studies that provided data to enable a deeper understanding of why discharge programmes, in general, do (or do not) work as intended, for whom and under what circumstances. We conducted a framework synthesis using the framework that was developed for the Implementation EGM. The EGM categorises included studies into broad categories of barriers and facilitators to the implementation of interventions. These categories were developed by the original authors of the EGM using an iterative process and were initially based on the implementation science framework. The categories were independently piloted against a small number of process evaluations and agreement was reached by researchers at the Campbell Collaboration, Campbell UK and Ireland, and Heriot-Watt University. The five broad categories, or levels of influence, are contextual factors, policy makers/funders, programme managers/implementing agency, staff/case workers, and recipients. We used this framework to synthesise data from the relevant qualitative studies.

The quality of these mixed methods studies were assessed using a tool developed by White and Keenan. The tool is similar to the fidelity assessment used by Stergiopoulos, Hwang, O'Campo, Jeyaratnam, & Kruk (2013) and aims to provide an accurate account of the eligible qualitative studies. The tool considers methodology, recruitment and sampling, bias, ethics, analysis and findings.

By including an element of qualitative evidence synthesis in this review we hope to provide a more robust and rich review of the evidence base.

03

Results

Included Studies

Types of studies

We identified 225 articles from the effectiveness map on 12th April 2019. Of these 225 articles, we identified 30 articles that appeared to meet our inclusion criteria following dual independent screening of titles and abstracts by two reviewers working independently. Full text screening was then carried out, again with dual independent screening, and 12 articles were excluded. The remaining 18 papers reported on 13 eligible studies of discharge programmes (see also Figure 1 for PRISMA flow diagram):

Study ID: Buchanan 2006

- The Effects of Respite Care for Homeless Patients: A Cohort Study (Buchanan, Doblin, Sai and Garcia, 2006).

Study ID: Buchanan 2009

- The health impact of supportive housing for HIV-positive homeless patients: a randomized controlled trial (Buchanan, Kee, Sadowski and Garcia, 2009).

Study ID: Conrad 1998

- Case Managed Residential Care for Homeless Addicted Veterans (Conrad, Hultman, Pope, Lyons, Baxter, Daghestani, Lisiecki, Elbaum, McCarthy and Manheim, 1998).

Study ID: Duwe 2013

- An Evaluation of the Minnesota Comprehensive Offender Reentry Plan (MCORP) Pilot Project: Final Report (Duwe, 2013).

Study ID: Gulcur 2003

- Housing, hospitalization, and cost outcomes for homeless individuals with psychiatric disabilities participating in continuum of care and housing first programmes (Gulcur, Stefancic, Shinn, Tsemberis and Fischer, 2003).

Study ID: Herman 2011

- A Randomized Trial of Critical Time Intervention to Prevent Homelessness in Persons with Severe Mental Illness following Institutional Discharge (Herman, Conover, Gorroochurn, Hinterland, Hoepner and Susser, 2011).
- Community Integration of Formerly Homeless Men and Women With Severe Mental Illness After Hospital Discharge (Baumgartner and Herman, 2012).
- The impact of critical time intervention in reducing psychiatric rehospitalization after hospital discharge (Tomita and Herman, 2012).

- Mediation Analysis of Critical Time Intervention for Persons Living With Serious Mental Illnesses: Assessing the Role of Family Relations in Reducing Psychiatric Rehospitalization (Tomita, Lukens and Herman, 2014).

Study ID: Kertesz 2009

- Post-hospital medical respite care and hospital readmission of homeless persons (Kertesz, Posner, O'Connell, Swain, Mullins, Shwartz and Ash, 2009).

Study ID: Lim 2017

- Impact of a Supportive Housing Programme on Housing Stability and Sexually Transmitted Infections Among Young Adults in New York City Who Were Ageing Out of Foster Care (Lim, Singh and Gwynn, 2017).

Study ID: Lipton 1988

- Housing the Homeless Mentally Ill: A Longitudinal Study of a Treatment Approach (Lipton, Nutt and Sabatini, 1988)

Study ID: Lutze 2014

- Homelessness and reentry: A multisite outcome evaluation of Washington State's reentry housing programme for high risk offenders (Lutze, Rosky and Hamilton, 2014).

Study ID: Nyamathi 2015

- Nursing Case Management, Peer Coaching, and Hepatitis A and B Vaccine Completion Among Homeless Men Recently Released on Parole: Randomized Clinical (Nyamathi, Salem, Zhang, Farabee, Hall, Khalilifard and Leake, 2015).
- Impact of an intervention for recently released homeless offenders on self-reported re-arrest at 6 and 12 months (Nyamathi, Salem, Farabee, Hall, Zhang, Faucette, Bond and Yadav, 2017)

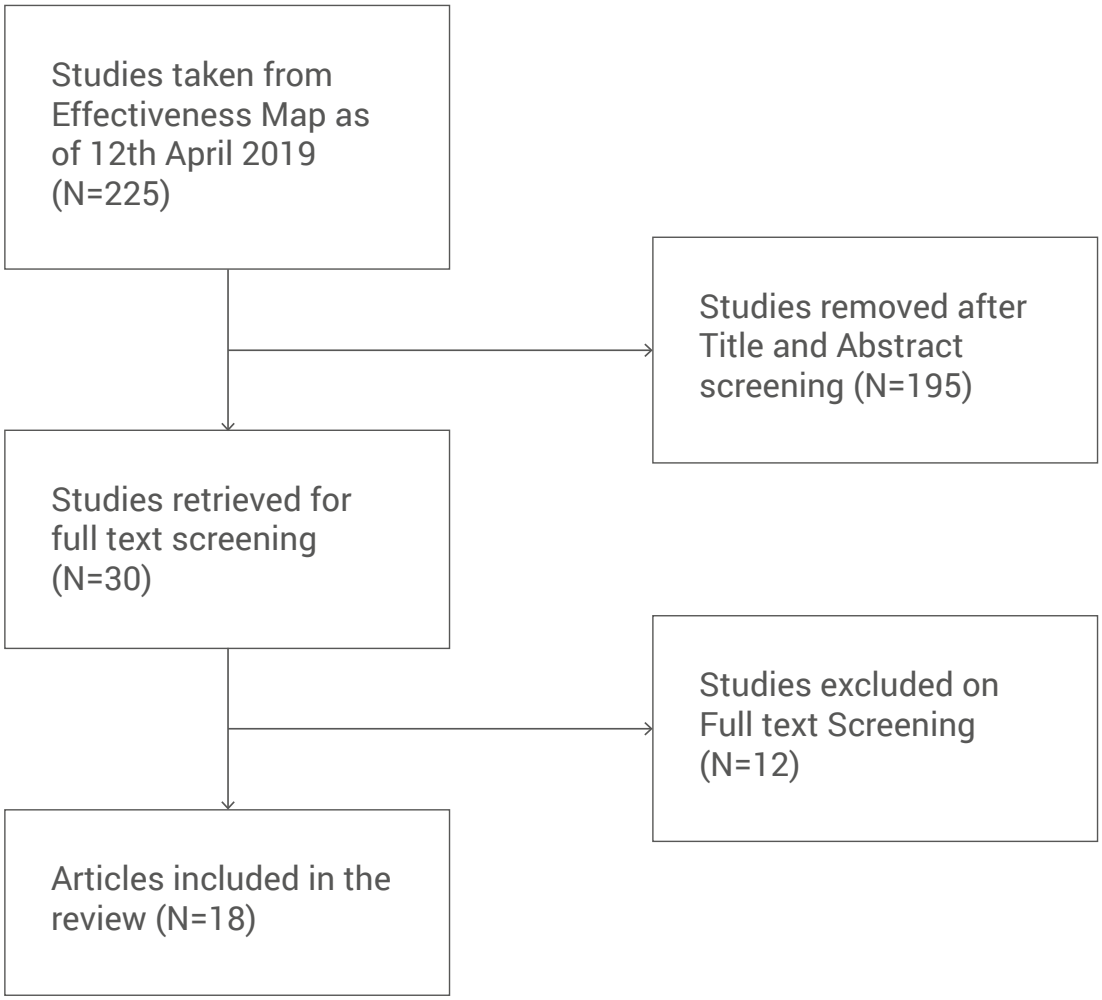
Study ID: Sadowski 2009

- Effect of a Housing and Case Management Programme on Emergency Department Visits and Hospitalizations Among Chronically Ill Homeless Adults A Randomized Trial (Sadowski, Kee, VanderWeele and Buchanan, 2009).
- Comparative Cost Analysis of Housing and Case Management Programme for Chronically Ill Homeless Adults Compared to Usual Care (Basu, Kee, Buchanan and Sadowski, 2012).

Study ID: Sosin 1996

- Serving street-dwelling individuals with psychiatric disabilities: Outcomes of a psychiatric rehabilitation clinical trial (Sosin, Bruni and Reidy, 1996).

Fig 1: PRISMA flow diagram



Characteristics of included studies

We identified 13 studies, eight of which were randomised controlled trials (RCT, 61.5%) and five non-randomised studies (non-RCT, 38.5%).

Twelve of the 13 studies were published in peer reviewed journals. We identified 13 studies, eight of which were randomised controlled trials (61.5%) and five were quasi-experimental studies 38.5%). In total 5279 people participated in the 13 included studies, with 4909 analysed, as two studies contained ineligible intervention arms. Studies evaluated programmes in hospitals (4), prison/jail (3), psychiatric hospitals (4), foster care (1), and addiction treatment (3, 1 addiction treatment only, 1 addiction treatment and prison, 1 from psychiatric hospital including substance abuse treatment). All studies were carried out in the USA. The location of the studies was largely urbanised, with 11 of the 13 studies conducted in large cities. Two studies did not specify their location. Twelve of the 13 studies were published in peer-reviewed journals. The weighted average age of all participants was 35.2 years, ranging from 18-70 years

The majority of participants were men, 79% on average in each study ranging from 57% - 100%. Interventions were delivered to individual participants and none of the interventions were designed to support whole families. Given the nature of discharge programmes all studies included people with complex needs, namely poor physical health (3), poor mental health (5), history of incarceration (3), substance abuse issues (9), care leavers (2), other complex histories (gang affiliation (1), high risk of harm and/or exploitation (2). 'high needs' (1)). Further details of the characteristics of included studies are reported in Table 1.

Detailed information about the interventions evaluated in each study is provided in Tables 2a and 2b.

Interventions varied considerably between studies. The most common aspect of the interventions was providing some form of housing, usually on top of some other form of additional service such as case management (e.g. Sosin et al., 1996), continuum of care (e.g. Gulcur et al., 2003), and other supportive housing (e.g. Lipton et al., 1988). Theories of change also varied between studies, with some focusing on addressing adherence to medical care services (e.g. Buchanan 2006) and others focused on improving housing stability first in order to give people the best chance of recovery/successful community living.

Table 1: Characteristics of included effectiveness

Study ID	Study Design	COUNTRY	Number of participants		
			TOTAL	INTERVENTION	Control
Buchanan (2006)	Non-RCT	USA	225	161	64
Buchanan (2009)	RCT	USA	105	54	51
Conrad (1998)	RCT	USA	358	178	180
Duwe (2013)	RCT	USA	689	415	274
Gulcur (2003)	RCT	USA	225	99	126
Herman (2011)	RCT	USA	150	77	73
Kertesz (2009)	Non-RCT	USA	743	136	Other care 174 (not analysed) Own care 433
Lim (2017)	Non-RCT	USA	895	251	644
Lipton (1988)	RCT	USA	49	26	23
Lutze (2014)	Non-RCT	USA	416	208	208
Nyamathi (2015)	RCT	USA	600	195 - PC-NCM 196 - PC (not analysed)	209
Sadowski (2009)	Non-RCT	USA	405	201	204
Sosin (1996)	Non-RCT	USA	419	136 Housing & case management 96 Case management	187

Institutional Setting	Complexity of Needs	Age (years) Mean (SD) range	Gender	
			Male	Female
Hospital/Physical Health Treatment	Poor physical health	Intervention 43 (9) Control 44 (10)	Intervention 78% (125) Control 81% (52)	Intervention 22% (36) Control 19% (12)
Hospital/Physical Health Treatment	Poor physical health, Substance abuse issues	Intervention 45 (6.9) Control 43 (7.7)	Intervention 72% (39) Control 84% (43)	Intervention 28% (15) Control 16% (8)
Psychiatric Hospital & substance abuse units	Poor mental health, Substance abuse issues	40, (NR), 25-70	100%	0%
Prison / Jail	Incarceration	Intervention 36.1 (NR) Control 33.4 (NR)	Intervention 94.9% (394) Control 90.1% (247)	Intervention 5.1% (21) Control 9.9% (27)
Psychiatric Hospital	Poor mental health, Substance abuse issues	NR, (NR), 43 aged 18–30, 182 aged 31-70	76.9% (173)	23.1% (52)
Psychiatric Hospital	Poor mental health Substance abuse issues	37.5 (9.5)	71% (107)	29% (43)
Hospital/Physical Health Treatment	Poor physical health Substance abuse issues	46.9 (11.0)	80% (594)	20% (149)
Foster Care	Substance abuse issues, Care leaver High risk of harm/ exploitation	18.6 (NR)	56.9% (510)	43.1% (385)
Psychiatric Hospital	Poor mental health	37 (NR)	65% (32)	35% (17)
Prison/Jail	Incarceration	36.67 (12.17)	81% (335)	19% (81)
Prison/Jail & Addiction treatment	Incarceration Substance abuse issues, Care leavers (57%)	40 (10.4)	100%	0%
Hospital/Physical Health Treatment	Poor mental health, Substance abuse issues	Intervention 47 (8.2) Control 46 (9.1)	75% (310)	25% (95)
Addiction Treatment	Substance abuse issues	35 (NR)	74.5% (312)	25.5% (107)

Table 2a: Description of Interventions

Study	Name of Intervention(s)	Description of the intervention
Buchanan (2006)	Respite Care	Providing short term shelter along with access to additional services. Conditional upon being capable of living in a drug and alcohol free environment.
Buchanan (2009)	The Chicago Housing for Health Partnership (CHHP)	Two strands within the intervention, one is respite care and case management, the other is referral to shelter with case management.
Conrad (1998)	Case Managed Residential Care (CMRC)	A social model program implemented in a traditional medical environment where transitional residential care was provided for up to 6 months with ongoing and follow-up case management for a 1-year treatment period.
Duwe (2013)	Minnesota Comprehensive Offender Reentry Plan (MCORP)	MCORP emphasized increased collaboration between prison caseworkers and supervision agents to provide planning, support, and direction for ex-prisoners to address their strengths and needs in both the institution and the community. More specifically, the core programmatic theme of this project was the development of dynamic case planning and case management that provided continuity between the offender's confinement and return to the community.
Gulcur (2003)	Pathways to Housing, Housing first + Assertive Community Treatment	The experimental Housing First programme offered immediate access to independent housing without requiring psychiatric treatment or sobriety, along with Assertive Community Treatment, allow clients to choose the frequency and type of services they receive and integrate a harm reduction philosophy to address the complex needs of individuals with dual diagnosis
Herman (2011)	Critical Time Intervention	CTI offers tailored tappering support to access and maintain housing and gradually transition to community living. The core goal is to bridge the gap between institutions and community supports by providing direct emotional and practical support and strengthening the individuals long term ties to family and friends. The interventions requires careful planning prior to discharge, communication between agencies and working with clients to support their transition to community living. 3-Phases each lasting 3 months, 1 is transition to community with intensive support to access existing services/resources, 2 tests and adjusts systems of support established in phase 1. 3 transfer of responsibility fully to the established and tested community supports.

	Duration of Treatment Period from Start to Finish	Control/comparison condition
	Ranged from hours to many days. Average number of days in respite care was 42 days.	No intervention.
	Unclear	Usual treatment - referrals to overnight shelters or to interim housing providers. All usual care participants were eligible to receive case management through an existing Ryan White program in the hospital-affiliated HIV/AIDS clinic.
	The expected length of stay was from 3 to 6 months, with the actual observed length of stay averaging 3.4 months	Usual treatment - customary care in the hospital and the chance to be seen by a social worker.
	Not specified. Follow up took place 18-53 months after. Average 35 months	Usual treatment, no further details
	24 months	Continuum of Care - clients begin with outreach programmes and drop-in centres that place few demands on them, and then progress through a series of congregate living arrangements with varying levels of on-site support.
	9 Months	Usual treatment - basic discharge planning services and access to psychiatric treatment.

Table 2a: Description of Interventions (continued)

Study	Name of Intervention(s)	Description of the intervention
Kertesz (2009)	Respite Care or Other planned care	Boston's respite program provides 24-hour nursing supervision, daily visits by nurse practitioners or physician assistants, onsite physician supervision, in-house dental and psychiatric care, and case management. Equipped for patients in more substantial need, the program has helped free up acute inpatient services in local hospitals since 1987. Other planned care participants were discharged to other supervised recuperative care (e.g., skilled nursing facilities, chronic care hospitals, or home health care)
Lim (2017)	New York City/ New York State–Initiated Third SupportiveHousing Program (NYNY III)	The NYNY III program for young adults ageing out of foster care provides affordable housing and access to various supportive services to help achieve independent lives, including case management, job training, and education support, and provides connections to physical and mental health services.
Lipton (1988)	Supportive Housing	Provides individuals with single rooms, case management, assistance with social security benefits, medication monitoring, money management, meals, activity therapy, referals to psychiatric care.
Lutze (2014)	Reentry Housing Pilot Program (RHPP)	"The goal of the program was to reduce recidivism by providing access to stable housing for up to 1 year and coordinating resources across agencies including the police, community corrections officers, social service providers, employers, and housing managers."p472 "Averting homelessness or transience by providing stable housing is likely to reduce exposure to deviant peers, social stigma, and the violation of public order laws related to living and working on the street and increase exposure to pro-social networks, constructive activities, and a sense of safety and well-being conducive to participating in treatment and other services." p473
Nyamathi (2015)	Peer coaching (PC) with or without Nursing Case Management (NCM)	Peer coaching over 8 weeks designed to act as a positive role model and work on building effective coping skills, personal assertiveness, self-management, therapeutic nonviolent communication (NVC), and self-esteem. Nurse case management focused on health promotion, reducing risky drug and sexual behaviours and encouraging adherence to treatment.

	Duration of Treatment Period from Start to Finish	Control/comparison condition
	Mean length of stay 31.3 days (SD = 32.6, median= 20).	No treatment - discharged to 'own care'
	At least 7 days	Usual Treatment - government-subsidized housing programs.
	12 months	Usual treatment, no further details
	Unclear but appears to be 12 months	Not specified

Table 2a: Description of Interventions (continued)

Study	Name of Intervention(s)	Description of the intervention
Sadowski (2009)	Respite Care or Other planned care	Early supportive housing with case management integrated into the medical system. Clients are offerred respite care/interim housing upon discharge from enrolling hospitalizations, followed by stable housing within 90 days. They have a case manager at each stage (hospital, respite/interim housing, and stable housing).
Sosin (1996)	New York City/ New York State–Initiated Third SupportiveHousing Program (NYNY III)	Housing and case management. This is conditional on abstinance and complying with treatment plan. The housing intervention provided the case management model along with supported housing in one of three blocks of twenty apartments, found in recently renovated buildings serving those with low incomes. The case management aspect appears to be limited to referral to services without direct provision of services.

Table 2b: Description of Interventions

Study	Were individual needs assessed?	Was discharge planned in advance?
Buchanan (2006)	No, but may have been undertaken with case manager	No
Buchanan (2009)	Yes, clients work with individual case managers to focus on their specific needs.	Yes, but not over a long time frame and planning appears to be referrals to overnight shelters or to interim housing providers
Conrad (1998)	Yes, clients work with individual case managers to focus on their specific needs.	Yes, the planning commenced on entry to the transitional housing
Duwe (2013)	Yes, caseworkers and supervision agents to provide planning, support, and direction for ex-prisoners to address their strengths and needs in both the institution and the community.	Yes - 60 days prior to release in phase 1, or immediately on entry into prison in phase 2. Phase 2 was more successful than phase one in reducing recidivism

	Duration of Treatment Period from Start to Finish	Control/comparison condition
	Not specified, follow-up data taken 18 months after	Usual treatment - patients receive usual social services for hospital discharge planning.
	Designed to be 8 months but participants typically withdrew earlier	Usual treatment - clients in control condition were referred to an outpatient or inpatient substance abuse agency, to welfare offices.

	Did the intervention coordinate stakeholders and services?	Was housing provided?	What additional services and support were offered?
	None specified	Yes, short term accommodation in respite care	Food, health services, case management and referrals to permanent housing.
	Coordination between hospital social worker and eight agencies providing respite care or supportive housing	Yes, supportive housing defined as housing without time limits combined with services to help participants to live more stable, productive lives	Intensive case management.
	Case manager facilitated integration of services	Yes, transitional residential care was provided for up to 6 months	Case management including substance abuse counselling and relapse prevention, basic living skills training, vocational services and referral to multiple services
	Case manager and supervision agents (parole officers or similar)	No, not directly provided	Case management

Table 2b: Description of Interventions (continued)

Study	Were individual needs assessed?	Was discharge planned in advance?	
Gulcur (2003)	Yes, ACT clients choose what services they need	No	
Herman (2011)	Yes, tailored tapering support to access and maintain housing and gradually transition to community living.	Yes, the planning commenced on entry to the transitional housing	
Kertesz (2009)	No	No	
Lim (2017)	Not reported	Not reported	
Lipton (1988)	Not reported	Yes, discharge planning begins immediately upon admission to intervention	
Lutze (2014)	Yes	Unclear	

	Did the intervention coordinate stakeholders and services?	Was housing provided?	What additional services and support were offered?
	None specified	Yes, participants were given permanent independent housing	Assertive community treatment
	None specified	No, not directly provided but housing arrangements were typically coordinated by discharge planning staff located at the transitional residence. These arrangements ranged from community residences and other structured programs to supported apartments and independent housing, either alone or with family members	CTI worker provides practical and emotional support over 9 months, with intensive support initially, tapering over time.
	None specified	Yes, respite care provided temporary accommodation for, on average, 30 days but no housing provided after that	24-hour nursing supervision, daily visits by nurse practitioners or physician assistants, onsite physician supervision, in-house dental and psychiatric care, and case management
	None specified	Yes, provides affordable housing	Access to various supportive services to help achieve independent lives, including case management, job training, and education support, and provides connections to physical and mental health services
	None specified	Yes, supported housing in single room accommodation	Participants provided case management, assistance with social security benefits, medication monitoring, money management, meals, activity therapy, referrals to psychiatric care
	RHPP was designed to promote interagency collaboration and information sharing between multiple stakeholders including the police, community corrections officers, social service providers, employers, and housing managers.	Yes, provided for up to 12 months	Co-ordination with additional services such as community corrections officers, social service providers, employers, and housing managers was provided.

Table 2b: Description of Interventions (continued)

Study	Were individual needs assessed?	Was discharge planned in advance?	
Nyamathi (2015)	No	No	
Sadowski (2009)	Yes, services tailored to participants needs	Yes, intervention includes plans for discharge to a respite care facility for transitional care between hospitalization and stable housing	
Sosin (1996)	Not reported	No	

	Did the intervention coordinate stakeholders and services?	Was housing provided?	What additional services and support were offered?
	None specified	No	Nursing specific case management was additionally given.
	The intervention was developed by a consortium of 14 hospitals, respite care centres, and housing agencies in Chicago.	Yes, respite care/ interim housing upon discharge from enrolling hospitalizations, followed by stable housing within 90 days	Case management integrated into the medical system
	None specified	Yes, supported housing in one of three blocks of twenty apartments	Case management

Risk of bias in included studies

Tables 3 and 4 provide a summary of the overall risk of bias assessment for RCTs and non-RCTs respectively

Risk of Bias in RCTs

Assessment of methodological quality and potential for bias was conducted using the second version of the Cochrane Risk of Bias tool for Randomised controlled trials. The eight randomised controlled trials were assessed for risk of bias and placed into one of three categories, low risk of bias, some concerns and high risk of bias. Three studies were assessed as having low risk of bias (Buchanan 2009, Conrad 1998, Herman 2011), four having some concerns (Duwe (2013), Gulcur (2003), Nyamathi (2015), Sadowski (2009)) and one high risk of bias (Lipton 1988).

Risk of Bias in non-RCTs

Non-randomised studies were coded using the ROBINS- I tool. The five non-RCT's were assessed in their risk of bias and placed into one of four categories from the ROBINS-I tool, low, moderate, serious and critical. Only one study (Buchanan 2006) was rated as low risk of bias, two were rated as moderate risk of bias (Lutze 2014, Sosin 2016) and two rated as having serious risk of bias (Lim 2017, Kertesz 2009).

Table 3: Risk of bias in included RCTs

Study	Overall risk of bias		
Buchanan (2009)	Low		
Conrad (1998)	Low		
Duwe (2013)	Some concerns		
Gulcur (2003)	Some concerns		
Herman (2011)	Low		
Lipton (1988)	High		
Nyamathi (2015)	Some concerns		
Sadowski (2009)	Some concerns		
Overall Risk of Bias	37.5%	50%	12.5%

Table 4: Risk of bias of included non-RCT studies

Study	Overall risk of bias			
Buchanan (2006)	Low			
Kertesz (2009)	Serious			
Lim (2017)	Serious			
Lutze (2014)	Moderate			
Sosin (1996)	Moderate			
Overall Risk of Bias	20%	40%	40%	0%

Synthesis of Results - Effectiveness of interventions

We provide a synthesis of the evidence relating to each outcome domain including the results of meta-analyses conducted where sufficient suitable data was available in included studies.

Criteria for Conducting Meta-analysis

We extracted all data reported in included studies relating to any of the following seven broad domains:

1. Housing stability
2. Health, including substance abuse, mental health, mortality, morbidity
3. Access to services
4. Crime/criminalisation
5. Employment and income
6. Capabilities and wellbeing
7. Cost of intervention
8. We also documented any unintended adverse events reported

Meta-analysis using RVE was not possible as there were fewer than the recommended minimum of 20 studies. Instead within each domain we conducted meta-analysis when more than two studies reported on the same outcome using a comparable measurement tool. Where meta-analysis was not possible we instead provide a narrative synthesis of the findings.

Interpreting forest plots and meta-analysis

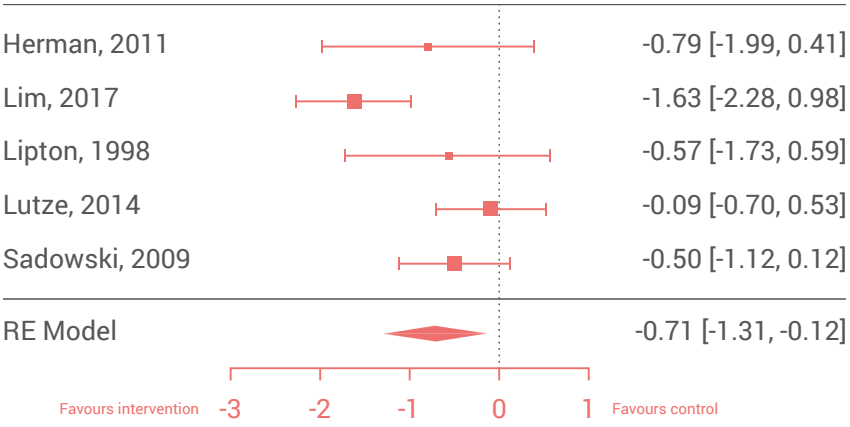
The meta-analyses are visually represented by forest plots. In a forest plot each study is represented by a separate horizontal line with a black square in the centre. The size of the square corresponds to the (sample) size of the study. The bars on either side of each square relate to the precision of the estimated effect – longer bars indicate less precision. If the bars extend across the vertical zero line, the line of no effect, this indicates that the differences between the intervention and control groups (for that particular study) were not statistically significant. The diamond at the base of the forest plot shows the overall effect of the interventions on the outcome. The centre of the diamond denotes the weighted average effect of the synthesised studies and represents the best estimate available with the existing data. The width of the diamond indicates the 95% confidence intervals, which denote how other effects are also reasonably consistent with the data given the statistical assumptions used. In an effort to convert the effect sizes and confidence intervals to meaningful metrics, we have also provided the natural frequencies associated with each outcome meta-analysed.

Housing stability

Five studies (Herman, 2011; Lim, 2017; Lipton, 1988; Lutze, 2014; Sadowski, 2009) measured housing stability as an outcome and were included in a meta-analysis, using a random effects model. The total number of participants included in the analysis was n=1868 (intervention n=738, control n=1130). Three studies were RCTs and two were non-randomised studies. The outcome was measured at various time points between studies but ranged between 12 months and three years.

Results show that the discharge programmes included in this analysis decreased the incidence of homelessness in the intervention group (SMD=-0.71, 95%CI [-1.31, -.12], p=0.02) compared to the comparison group. An effect size of 0.71 indicates a large positive effect of the intervention, compared to usual care, discharge programmes substantially improved housing stability for people leaving institutional settings. However, there is uncertainty around this estimate, as indicated by the 95% confidence interval, such that an effect size as large as -1.31 or as small as -0.12 would also be consistent with the data reported here.

Fig 2: Forest plot: homelessness



It is evident that the effects vary between studies. This is likely to be as a result of the different types of study design, intervention characteristics and sample characteristics that are represented in the pool of studies synthesised and is borne out by the heterogeneity evidenced in the meta-analysis ($I2=64.5\%$, $\chi2=12.1$, $df=4$, $p=0.02$). However, due to the small number of studies included in the meta-analysis, effects were not analysed separately according to study type (RCT/non-RCT), risk of bias or time point of data collection. Neither was it possible to conduct the planned sub-group analysis as we could not be confident that any differences between groups of studies were attributable to the subgroup or to other overlapping differences between the studies, such as study design, risk of bias, population characteristics or setting. For information, the characteristics of the studies included in the analysis are described in Table 5 below.

Table 5: Characteristics of studies included in the housing stability meta-analysis

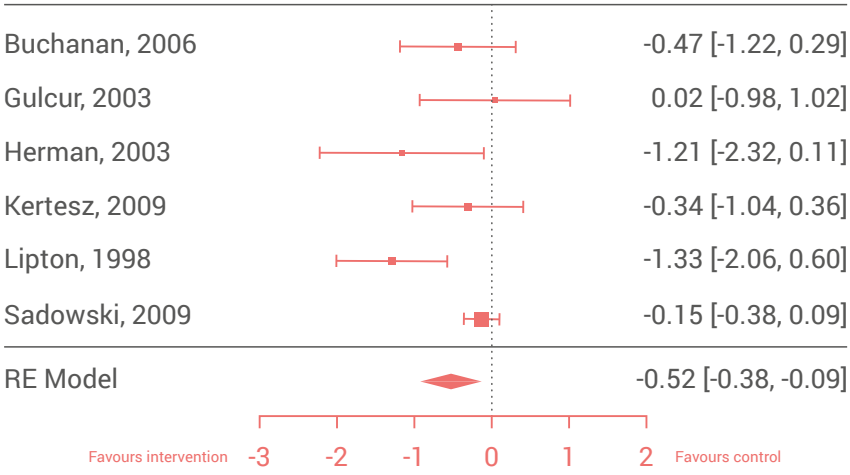
Study	Institutional setting	Study design	Risk of bias	Sample size	Timepoint
Herman (2011)	Psychiatric hospital	RCT	Low	Int n=58 Con n=59	14-18 months
Lim (2017)	Foster care	Non-RCT	Serious	Int n=251 Con n=644	24 months
Lipton (1988)	Psychiatric hospital	RCT	Serious	Int n=20 Con n=15	12 months
Lutze (2014)	Prison	Non-RCT	Moderate	Int n=208 Con n=208	36 months
Sadowski (2009)	Hospital (physical health treatment)	RCT	Moderate	Int n=201 Con n=204	18 months

Health

Hospitalisation

Five studies (Buchanan, 2006; Gulcur, 2003; Kertesz, 2009; Lipton, 1988; Sadowski, 2009) measured the number of hospitalisations (or hospitalised days) post-discharge and were included in a meta-analysis. The total number of participants included in the analysis was n=1288 (treatment n=545, control n=743). Three studies were RCTs and two were non-randomised studies. The outcome was measured during various time intervals but ranged between three and 24 months. Results showed that the discharge programmes included in this analysis decreased the incidence of hospitalisation in the treatment group (SMD=-0.43, 95%CI [-0.87, 0.01]) compared to the control group. One study (Herman 2011) specifically measured psychiatric hospitalisation (total n=150). When this effect was included in the above analysis the effect size increased in magnitude (SMD=-0.52, 95%CI [-0.95, -0.09], p=0.019), indicating a medium effect of discharge programmes on hospitalization, see Figure 3 below. On average, compared to usual care, discharge programmes reduced the average number of days a person spent in hospital after leaving an institutional setting. However, the uncertainty around this estimate, as denoted by the 95% confidence interval, indicates that an effect size as large as -0.95 or as small as -0.09 would also be consistent with the data reported here.

Figure 3: Forest plot: hospitalisations



As with the previous meta-analysis, it is evident that there is some inconsistency between study effects, likely to be due to the varying characteristics that are represented in the pool of studies synthesised and is borne out by the meta-analysis (I2=59.1%, $\chi 2$=12.5, df=5, p=0.03).

Again, due to the small number of studies included in the meta-analysis, effects were not analysed separately according to study type (RCT/non-RCT), risk of bias or time point of data collection. Neither was it possible to conduct the planned sub-group analysis as we could not be confident that any differences between groups of studies were attributable to the subgroup or to other overlapping differences between the studies, such as study design, risk of bias, population characteristics or setting. For information, the characteristics of the studies included in the analysis are described in Table 6 below.

Table 6: Characteristics of studies included in the hospitalisations meta-analysis

Study	Institutional setting	Study design	Risk of bias	Sample size	Timepoint
Buchanan (2006)	Hospital (physical health treatment)	Non-RCT	Low	Int n=161 Con n=64	12 months
Gulcur (2003)	Psychiatric hospital	RCT	Moderate	Int n=28 Con n=30	24 months
Herman (2011)	Psychiatric hospital	RCT	Low	Int n=58 Con n=59	14-18 months
Kertesz (2009)	Hospital (physical health treatment)	Non-RCT	Serious	Int n=134 Con n=430	3 months
Lipton (1988)	Psychiatric hospital	RCT	High	Int n=20 Con n=15	12 months
Sadowski 2009	Hospital (physical health treatment)	RCT	Moderate	Int n=201 Con n=204	18 months

Emergency department visits

Two studies, one RCT (Buchanan, 2006) and one non-RCT (Sadowski, 2009) reported on hospital emergency department visits and both studies evaluated discharge from hospital (physical health). The data reported in these studies was inappropriate for meta-analysis however both studies reported a reduction of emergency department visits in the treatment group.

While in both studies the average or median number of annual visits to the emergency department was roughly two visits per person in the control groups, compared to one per person in the intervention group, in both cases these group differences were not statistically significant at 12 months (Buchanan, 2006) or 18 months (Sadowski, 2009) post-discharge. It appears that discharge programmes might have a positive effect on reducing the number of times a person accesses emergency hospital care, however, this finding should be interpreted with great caution as it is based on just two studies, data in both studies was skewed as many participants did not have any visits to the emergency department and detection of robust group differences in rare events is hindered by small sample sizes.

Mental health

Only two studies, both RCTs of low/some concerns risk of bias (Conrad, 1998; Sadowski, 2009) measured mental health outcomes, unfortunately there were insufficient data reported to synthesise these findings in a meta-analysis. Both studies reported no statistically significant differences between the treatment and control groups in terms of mental health, at 18 months (Sadowski, 2009) and 24 months (Conrad, 1998) post discharge from hospital (physical health and psychiatric respectively).

Physical/general health

Three studies, all RCTs of low/some concerns risk of bias, reported on participants' physical or general health (Conrad, 1998; Nyamathi, 2017; Sadowski, 2009). Insufficient data were reported to conduct a meta-analysis however Sadowski (2009) reported no differences between treatment and control groups on this outcome at 18-month follow up (on discharge from hospital) and Conrad (1998) reported that while the treatment group reported fewer medical problems initially, over a two year period the gap between the treatment and control groups, in terms of physical health, decreased. Nyamathi (2017) listed this outcome as measured, but did not report the results.

Alcohol and drug use

Three studies, two RCTs with low/some concerns risk of bias and one non-RCT with moderate risk of bias, reported on the impact of discharge programmes on alcohol and drug use (Conrad, 1998; Nyamathi, 2015; Sosin, 1996). Insufficient data were reported to synthesise the results in a meta-analysis. Conrad (1998) reported that the treatment group experienced less alcohol and drug abuse compared to the control group during the two year follow up period (post discharge from psychiatric hospital) but that this effect decreased with time and by the end of the two-year study period, there were no statistically significant differences between treatment and control groups on alcohol or drug use.

Sosin (1996) reported that substance abuse was less frequent in the treatment group on discharge from addiction treatment (30 days prior to data collection) but the effect was modest (on average 2-2.5 fewer days of alcohol and drug use, statistically significant). Nyamathi (2015) used drug and alcohol use as a covariate in their analysis but it was not reported as an outcome. Overall, the studies suggest that discharge programmes may have a positive effect on reducing substance use in the short term, but these discharge programmes alone were not sufficient to maintain this effect over the longer term.

Sexually transmitted infections (STIs)

One non-RCT study with serious risk of bias (Lim, 2017) reported that for young adults ageing out of foster care the risk of STIs was 30% lower for those in the treatment group compared to the control group (relative risk =0.3, 95% CI [0.2, 0.7]) at two years post-discharge. However, the intervention group in this study appear to have greater needs prior to intervention than the control group so this finding should be interpreted with caution.

Access to services

No studies reported access to services as an outcome.

Crime and justice

Four studies all with moderate risk of bias (Duwe, 2013; Lutze, 2014; Nyamathi, 2015; Sadowski, 2009) reported data on reincarceration and were included in a meta-analysis. Three of these four studies examined discharge from prison (Duwe, 2013; Lutze, 2014; Nyamathi, 2015), the fourth was discharge from hospital (Sadowski, 2009). The total number of participants included in the analysis was n=1820 (treatment n=974, control n=846). Three studies were RCTs and one was a non-randomised study. The outcome was measured between six and 53 months post discharge across the four studies. Random effects meta-analysis showed a reduction in the odds of the intervention group being incarcerated compared to the control group OR= -0.41, 95% CI [-0.86, 0.04]. Using the median odds ratio in included studies as the assumed comparator event rate, combined data from these four studies suggest that 214 fewer people per 1000 would be incarcerated with treatment (542 in 1000 control vs 328 in 1000 with discharge programmes). Based on the confidence intervals, the results are also consistent with effects where 500 fewer people were incarcerated, and 37 more people were incarcerated.

It appears that discharge programmes can substantially reduce the number of people going to or returning to prison. The high rates of imprisonment reflects the high likelihood of return to prison for those discharging from prison, the effect of discharge programmes on incarceration for those discharging from other settings is unclear as there was only one study measuring imprisonment following discharge from a non-prison setting.

There was substantial heterogeneity between study effects (I2=75.72%, $\chi 2=12.19$, df=3, p=0.0068). Again, due to the small number of studies included in the meta-analysis, effects were not analysed separately according to study type (RCT/non-RCT), risk of bias or time point of data collection. Neither was it possible to conduct the planned sub-group analysis. For information, the characteristics of the studies included in the analysis are described in Table 7 below.

Figure 4: Forest plot: incarceration

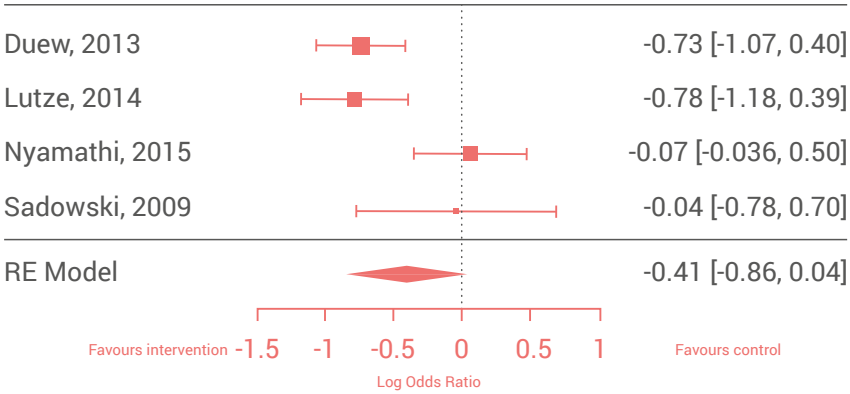


Table 7: Characteristics of studies included in the incarceration meta-analysis

Study	Institutional setting	Study design	Risk of bias	Sample size	Timepoint
Duwe (2013)	Prison	RCT	Moderate	Int n=415 Con n=274	18-53 months
Lutze (2014)	Prison	Non-RCT	Moderate	Int n=208 Con n=208	36 months
Nyamathi (2015)	Prison	RCT	Moderate	Int n=175 Con n=181	6 months
Sadowski (2009)	Hospital (physical health treatment)	RCT	Moderate	Int n=201 Con n=204	18 months

Employment and income

No useable data on employment and income were reported in any of the included studies

Capabilities and wellbeing

Within this outcome domain three RCTs with low/some concerns risk of bias reported relevant outcomes (Herman, 2011; Nyamathi, 2015; Sadowski, 2009). There were insufficient data to combine the results in a meta-analysis. The Herman 2011 study (as reported in Tomita, 2014 and Baumgartner, 2012) measured frequency of family contact, satisfaction with family relationships, physical integration and social integration, post discharge from psychiatric hospital. The study authors found that assignment to the intervention group resulted in improved family contact (d=1.07, indicating a large improvement in family contact, however the uncertainty of this estimate was not reported, p=0.02) (Tomita, 2014) however there were no statistically significant difference between groups in relation to either physical or social integration at 18 months post discharge (Baumgartner, 2012). Nyamathi (2015) measured coping and social support in participants discharged from prison, but these variables were used as covariates in their main analysis and were not reported as outcomes. Similarly, Sadowski (2009) also measured quality of life (at baseline and 18 months) post discharge from hospital, but the variable was used as a covariate in the analysis and was not reported as an outcome (Basu, 2012).

Cost

Five studies reported cost as an outcome (Buchanan, 2006; Duwe, 2013; Gulcur, 2003; Kertesz, 2009; Sadowski, 2009). Reporting on the methods used to calculate costs, including assumptions made and the economic context were absent from almost all studies. Unfortunately, the means of analyzing and reporting cost data, as well as differences in the economic, social and health contexts in each study meant that it was not possible to meta-analyse. These studies are narratively synthesised. In considering cost data it is useful to also consider the type of institution from which participants are being discharged and how this might affect the salience of particular outcomes and costs associated with the intervention being effective or otherwise.

In a study of discharge from psychiatric hospital, Gulcur (2003) reported that at the 24 month follow up, the intervention group had incurred lower costs than the control group ($F(1, 173)=6.1$, $p<.05$, cohen's $d = 0.35$, 95% CI 0.07 – 0.63) but no dollar amount was reported.

Buchanan (2006) reported that at 12 months post-discharge from hospital (physical health), the average cost of respite per hospital-day avoided (by the intervention group) was $706, which was less than half the $1500 per hospital-day costs estimated in the USA during the same time period. Kertesz (2009) calculated 90-day total costs and reported that the intervention cost +$5994 (95% CI, $4,210 – $7,779) to deliver, but this analysis did not take into account the judicial, medical or social care costs of discharging people into homelessness. Sadowski (2009) did take a societal perspective and reported that by the end of an 18 month follow up period, the total annual cost of services used per adult experiencing homelessness was lower in the intervention group compared to the control group, representing an annual cost saving of $6307 per person (95% CI [$16,616, $4,002], p=0.23) (Basu, 2012). The authors broke down this overall cost according to costs specifically associated with case management, residential substance abuse treatment, emergency department visits, outpatient visits, housing, legal costs, nursing home costs, and hospitalisation. The largest cost saving in the intervention group (compared to the control group) was associated with reduced hospitalisation and the largest cost expenditure in the intervention group (compared to the control group) was related to increased housing and respite costs. Finally, Duwe (2013) investigated whether their intervention provided the state with any return on their investment (of $2.24 million) in the pilot project. Follow up times ranged from 18-53 months and the authors demonstrated that their analysis was sensitive to alternative scenarios affecting their cost estimate, namely whether the costs to society of homicide was included or not, illustrating the potential bias in cost estimates depending on the perspective taken. Overall, the authors concluded that, in the most conservative analysis, the intervention yielded a return of $1.80 for every dollar spent on the project.

Adverse outcomes

Death

Four studies reported the number of deaths in the intervention and control groups (Buchanan, 2006; Buchanan, 2009; Kertesz,2009; Sadowski, 2009). The total number of participants included in the analysis was n=1293 (treatment n=545, control n=748), see Figure 5. The odds of death were slightly higher in the intervention group compared to the control group (OR=0.16, 95% CI [-0.35, 0.67]). This translates to 66 more deaths per 1000 in the intervention group (57 in 1000 died without intervention, and 123 in 1000 with intervention). The statistical uncertainty around the results is also reasonably consistent with the interventions causing between 17 fewer and 77 more deaths. There was no heterogeneity in the results (I^2=0%, χ^2=0.74.66, df=3, p=0.86).

Figure 5: Forest plot: adverse outcomes (death)

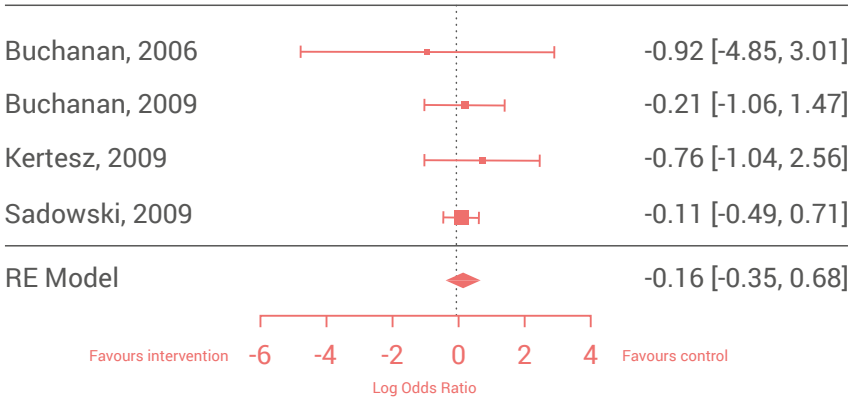


Table 8: Characteristics of studies included in the adverse outcomes (death) meta-analysis

Study	Institutional setting	Study design	Risk of bias	Sample size	Timepoint
Buchanan (2006)	Hospital (physical health treatment)	Non-RCT	Low	Int n=161 Con n=64	12 months
Buchanan (2009)	Hospital (physical health treatment)	RCT	Low	Int n=54 Con n=51	12 months
Kertesz (2009)	Hospital (physical health treatment)	Non-RCT	Serious	Int n=134 Con n=430	3 months
Sadowski (2009)	Hospital (physical health treatment)	RCT	Moderate	Int n=201 Con n=204	18 months

04

Process and implementation data synthesis

Background and aims

The second element of the current review involved synthesising qualitative data extracted from process evaluations included in Centre for Homelessness Impact's implementation and process evaluation Evidence and Gap Maps.

The purpose of this synthesis was to complement the quantitative evidence reported above and provide a better understanding of what factors influence programme effectiveness, exploring whether these are related to implementation fidelity. It focused on the following questions:

- Is implementation fidelity related to the effectiveness of the intervention?
- What implementation and process factors influence intervention delivery?

Analytic approach

The typology used to construct the original EGM (White, et al., 2018) was developed using a grounded theory approach piloted on 25 papers initially. This iterative process was combined with expert knowledge, ensuring that the broad concepts identified would adequately capture all papers included in the map. From the piloted typology, robust categories were created to include all process evaluations found during the searching period. The team at Heriot-Watt University coded each process evaluation under five main analytical categories, or levels of influence, namely: contextual factors, policy makers/funders, programme administrators/managers/implementing agencies, staff/case workers and recipients of the programme. Using a framework synthesis, it is these five analytical categories that have been used to synthesise and organise the data analysis reported in the following section.

In this way, the EGM provided an initial framework around which to synthesise the data; a framework that, for the most part, fits well. This decision also ensured that the EGM structure could be used to inform the synthesis process but also provided the team with a degree of flexibility. It is important to remember however, that because the effectiveness EGM and the process and implementation EGM were (necessarily) constructed separately, this means that the qualitative process evaluations in this section are not necessarily related directly to the specific interventions reported in the meta-analyses above. This notwithstanding the salient points related to implementation of discharge programmes more generally have been extracted and synthesised.

In the effectiveness studies little information was reported on any measurable effect of implementation fidelity. However, where reported, we noted any potential implementation issues that may provide some insight into how these may influence the effectiveness of the intervention and highlight where these influences converge or diverge from the qualitative evidence synthesised.

Results

Included papers

On 10th May 2019 we downloaded all 292 reports contained in the EGM from EPPI Reviewer, with access granted by White et al. Each of these articles were then screened by title and abstract by two reviewers working independently. Of these, 35 papers were identified as potentially relevant to discharge programmes for individuals experiencing or at risk of experiencing homelessness. The full text of these 35 papers were then reviewed. From these 10 papers were selected for synthesis through purposive sampling. Studies were selected on the basis of providing insight into implementing discharge programmes in a diverse range of institutional settings, populations and geographical locations. Studies that provided most data were selected first and additional studies added until we reached saturation. Only one qualitative study examined an intervention included in the effectiveness synthesis, otherwise there was no overlap between the studies in the effectiveness analysis and the qualitative papers.

Five process evaluations focus on military personnel discharged from the armed forces (Austin et al., 2014; Jones et al., 2017; Kertesz et al., 2013; Stoll, 2017 and Trutko et al., 2016). Three of the selected studies concentrate on patients discharging successfully into housing from hospitals (Homeless Link, 2015; Shelter, 2015 and Wood et al., 2017), one discusses the process of discharge from incarceration (Lutze et al. 2009) and one focuses on the discharge of care experienced young people into secure and stable accommodation after 'growing out' of care services (Sewel, 2016). Four of the selected studies were based on interventions conducted in the United Kingdom, one in Australia and the remaining five were carried out in the USA. All evaluations took place between 2009 and 2017. Additional information on the characteristics of the selected studies is included in Table 9 and information describing the intervention being evaluated is reported in Table 10.

The process and implementation synthesis included 10 studies discussing discharge from a range of institutions.

Table 9: Characteristics of included process and implementation studies

Study	Name of intervention	Location	Setting	Popular
Austin (2014)	Housing and Urban Development Veteran's Affairs Supportive Housing	USA	Veteran's Affairs facilities	Veterans
Homeless Link (2015)	Homeless Hospital Discharge Fund	UK	Hospital	Patients vulnerable to homelessness
Jones (2017	Patient Centred Medical Home model	USA	Veteran's Affairs facilities	Veterans
Kertesz (2013	Healthcare for the Homeless Program	USA	Veteran's Affairs facilities	Veterans
Lutze (2009)	Re-entry Housing Pilot Program	USA	Prison/jail	Ex-prisoners
Sewel (2016)	Supported lodgings services	UK	Supported lodgings services for care experienced young people	Young people in care who are yet ready for independent living
Shelter (2015)	Cornwall Homeless Patient Hospital Discharge Project	UK	Hospital	Patients vulnerable to homelessness
Stoll (2017)	Stoll London Outreach service (Transition support	UK	Advice given to veterans when discharging from armed forces	Veterans
Trutko (2016)	Homeless Veterans Reintegration Program	USA	Veteran's Affairs facilities	Veterans
Wood (2017)	St. Vincent's Hospital Melbourne Homeless Services	Australia	Hospital	Patients vulnerable to homelessness

Table 10: Description of interventions evaluated in the included studies

Study Name	Theory of change	Treatment as usual	Time to discharge	Dosage
Austin (2014)	To end veteran homelessness by 2015 using and Housing First model by providing permanent rental vouchers	None reported	Not specified	Not specified
Homeless Link (2015)	Improve hospital discharge procedures for patients who are homeless through training link workers and buying housing	None reported	Implement intervention on admission to hospital	Not described, probably varied throughout the 52 different projects
Jones (2017)	Providing veterans who are homeless with mental health and substance abuse issues with housing will help to improve their future outcomes	None reported (comparison with non-homeless veterans)	Likely post discharge	Not specified
Kertesz (2013)	Intense and tailored health care will produce greater client satisfaction	None reported	Not specified	Not specified
Lutze (2009)	Providing housing for high risk ex-prisoners re-entering the community with enhance outcomes	None reported	Planned before discharge but not specified	2-3 face to face meetings per week with case manager
Sewel (2016)	Providing care experienced young people with supported lodgings will assist in a young person developing the confidence and capability to live an independent adult life	None reported	Up to six months before discharge	2-3 face to face meetings per week with support worker, tapering to once a month as placement continues

Personnel delivering intervention	Personnel interacting with client	Payment of intervention	Organisations involved	Participants
Housing First coordinator, HUD-VASH program manager	Housing specialist, Case workers, substance use or mental health specialist	Federal funding	Department of Housing and Urban Development	95 facility managers and front line staff interviewed
Housing and Nursing Link workers	Housing and Nursing Link workers	Department of Health	Organisations across England e.g. YMCA Crewe, Urban Outreach (Bolton), St. Peter's Night Shelter, Trinity Winchester	Survey data collected from 48 members of staff and 30 semi structured interviews with clients
PACT Team: Primary care provider, registered nurse case manager, clinical staff assistant (i.e., licensed practical nurse, licensed vocational nurse, or medical assistant), and administrative clerk	PACT Team: Primary care provider, registered nurse case manager, clinical staff assistant (i.e., licensed practical nurse, licensed vocational nurse, or medical assistant), and administrative clerk	Not specified, likely federal funding	Veterans Health Administration	4,605 homeless veterans surveyed, 63,061 non homeless veterans surveyed
Homeless focused staff	Medical staff and nurses	Federal funding	Department of Veterans Affairs	601 homeless experienced veterans included in analysis
RHHP Team: Community Corrections Officer (CCO) and a representative from YW Housing (serving women in the program) and Community Services Northwest (CSNW: serving the men in the program)	Case manager	Federal funding	Departments of Justice, Labour, Housing and Urban Development, and Health and Human Services	154 ex-prisoners
Supported lodgings providers	Support worker	Local authority funding	Barnardos	7 service staff surveyed, 11 service staff, 14 young people and 20 supported lodgings providers interviewed

Table 10: Description of interventions evaluated in the included studies

Study Name	Theory of change	Treatment as usual	Time to discharge	Dosage
Shelter (2015)	Prompt assessment and a planned discharge with a multi-agency approach to reduce social exclusion, repeat admissions and improve health outcomes	None reported	Assessed on admission if accommodation needed	Weekly in hospital and twice weekly after discharge
Stoll (2017)	To house all veterans discharged from service into suitable accommodation and employment	None reported	Intervention can commence at any time	Not described
Trutko (2016)	To foster inter-agency and community cooperation in engaging homeless veterans in employment	None reported in this evaluation but recommended for other evaluations	Post discharge	Not described
Wood (2017)	To meet the health needs of people who are homeless in Prague House, The Cottage, CHOPS and ALERT.	None reported	Not described but probably varies across projects	Not described but probably varies across projects

There is little overlap between the included studies in the effectiveness synthesis and the selected implementation studies as many of these did not have a published process evaluation. The only exception is Lutze et al. (2009); this provides a process evaluation for a multisite outcome evaluation of Washington State's re-entry housing programme for high risk ex-prisoners (Lutze, 2014). Nonetheless, this synthesis endeavours to offer insights into factors that more generally may influence the effectiveness of an intervention focused on discharge programmes. Additionally, there can also be specific and significant challenges to successful discharge among discrete populations and we draw out any evidence that suggests particular barriers and facilitators for specific groups of people.

Personnel delivering intervention	Personnel interacting with client	Payment of intervention	Organisations involved	Participants
Housing support development coordinator	Support worker	Public Health funding	Shelter, Cornwall Council, Coastline, Cornwall Partnership NHS, Inclusion Cornwall, Peninsula Community Health, St Peter's Society	169 patients
4 Stoll support staff	4 Stoll support staff	Forces in Mind Trust, Big Lottery Fund, National Lottery	Forces in Mind Trust	130 veterans engaged with service (Jan 2015 – Sept 2016)
HVRP program director	Case workers and intake staff	Federal funding	Department of Labour	Not specified
Service managers and senior staff workers	Nurses, activity staff, pastoral care, personal carers, housekeepers, cooks and administrative staff	Not specified	Not specified	359 clients accessed the services

The following analysis takes each of the five main analytical categories/ levels of influence (described above and reflected in the process and implementation EGM) in turn, namely:

1. Contextual factors,
2. Policy makers/funders,
3. Programme administrators/managers/implementing agencies,
4. Staff/case workers,
5. Recipients of the programme.

Contextual Factors

Interventions are delivered within the context of the housing and labour markets, the welfare system and current legislation. The availability of housing, employment and the welfare safety net clients have access to will affect how well an intervention can be implemented. As Wood et al. (2017) suggest, the lack of stable, affordable accommodation can be a major and common issue for patients being discharged from hospital and is also a fundamental social determinant of health. Specifically, Wood et al. (2017) cite bed availability, sobriety, inflexibility and participant past behaviour as particular challenges to accessing housing, suggesting that it is not meeting the needs of those discharged. Austin et al. (2014), Sewel (2016) and Lutze et al. (2009) report comparable issues for veterans, ex-prisoners and care experienced young people, particularly in the variability of the rental market; for example, in the availability, affordability, desirability and safety of housing. The competition for housing can be so concentrated that apartments are often rented within one hour of public advertisement (Austin et al., 2014).

Trutko et al. (2016) reference a number of issues with the labour market for veterans discharging from service. They suggest that often there is a mismatch or disconnect between the skills held by veterans and what expertise is required by the local job market. Often the available jobs that would promote self-sufficiency require degrees, licenses and certifications and therefore become inaccessible to members of the community who are already vulnerable and suffering from a variety of substance abuse and mental health issues. Similarly, Shelter (2015) explore this welfare issue, stating that the limitations on services due to budgets can create chaos for those with complex and ongoing needs, further compounding the difficulties faced by people who have been sleeping rough in the long term. One potential solution, proposed by Lutze et al. (2009) and Trutko et al. (2016) is to ensure that the intervention is legislated for, thereby providing statutory support for those most in need.

Policy makers and funders

Integrated approaches

An important theme that emerges from many of the included studies is a commitment from policy makers and funders to change the culture and priorities of an institution to enable the successful implementation of discharge programmes. A culture of commitment to this type of intervention, linked to a local commitment to preventing, rather than managing homelessness, for

example, is a major element, even if there are structural challenges to overcome (the housing supply and availability issue). For example, in their 2014 study, Austin et al. wanted to use a Housing First intervention for those discharging from military service. Although facility leaders of the Veteran's Affairs Facilities were interested in the Housing First programme, they had little understanding of the practical challenges in securing accommodation for those being discharged from military institutions. This notwithstanding, the authors acknowledge that without the visible commitment of facility directors and leaders to tackling homelessness through public engagement, the programme would have faced many more issues irrespective of the context regarding housing supply.

Buy-in from leadership can also be impacted by differing ideologies on homelessness, which will influence acceptability of an intervention approach. For example, some leaders may define homelessness in absolute terms as those living on the street or in public places. Others may include those living in inadequate or unstable accommodation as homeless and therefore eligible for intervention. Others still will recognise the need for holistic 'wrap around' support in terms of employment, healthcare and mental health support for those being discharged from hospital, military organisations and prison. At a local level, policy and practice around populations who are homeless will be influenced by people in positions of power; in this case improving leaders' understanding of the issue and the multiple reasons people may find themselves without suitable stable accommodation can help leaders to understanding the importance of pro-active approaches to preventing homelessness, discharge programmes being an example.

Funding

The amount of funding available, the time constraints often placed on the funding (in terms of applying for it and spending it) as well competing services and needs, all impact on the support and delivery of discharge programmes. Lutze et al. (2009) advocate making funding available as soon as possible in order for a programme to become operational. This can otherwise cause significant delays and ultimately impact upon clients at risk of homelessness. Similarly, Wood et al. (2017) found that for emergency departments in particular, there can be harsh fines if a patient is not discharged within a certain time period, causing subsequent issues for both the patient and hospital's funding stream. Homeless Link (2015) found that short project delivery times created a short timeframe to apply for funding.

Homeless Link (2015) and Shelter (2015) both list 'future investment' as their primary recommendation, suggesting that investment in hospital discharge should be a jointly commissioned venture involving all partners. To maximise sustainability they recommend that funding should be ring-fenced for a period of more than six months to recruit, train staff and embed good practice, enabling longer lead times to secure, purchase and refurbish suitable accommodation. In one study on prison discharge conducted in the USA this meant different federal sectors investing in a discharge programme (Lutze et al., 2009) where funding was granted from several departments including the Departments of Justice, Labour, Housing and Urban Development, and Health and Human Services. Wood et al. (2017) concur with this summation, reporting that programmes driven by immediacy compounded with complex participant needs and housing shortages create an unsustainable model.

For many discharge programmes, partnerships with a variety of organisations can result in a significant increase in planning and decision making. If paired with the terms and conditions of funding and statutory obligations, this can create a culture of investment, increasing community capacity. One example of this is Shelter (2015) - as a significant partner in the Cornwall project – who were able to provide a Homeless Patient Advisor to work from the hospital sites and implement the necessary discharge protocols. Comparatively, As Wood et al. (2017) found, it can be the case that when partnerships between the health and homeless sectors are encouraged and successful, further discharge interventions and support can be leveraged for those at risk of homelessness. Partnerships at this level of influence can often impact upon the longevity of a discharge programme and need to be cultivated with sensible funding proposals to ensure sustainability.

Programme administrators, managers and implementing agencies

Buy-in (Leadership, culture, priorities)

Homeless Link (2015) suggest that in order for partnerships to be successful for programme administrators and managers, integration and promotion needs to occur at different strategic levels. For example, in planned discharge programmes, such as those developed by Homeless Link (2015) integration and promotion should be reflected in the local area's overall health and well-being agenda for people at risk of homelessness. On the ground, this should include training and awareness of the homeless hospital discharge protocol, regular multidisciplinary meetings, access to all hospital wards for project staff and clear information available on the project remit and how and where to refer patients. They found

that this initial buy-in enabled key staff to forge positive relationships quickly and easily and that gathering support from medical professionals at the bid writing stage, facilitated hospital buy in. This eventually acted as a catalyst for other discharge projects due to the positive experience for the hospital group.

Sewel (2016) reports that a culture of 'exceptional' support for providers can in turn support young people who are leaving care. Despite this however, it was possible for some young people to fall through the cracks as it was assumed that another service operating in the same area would pick up any outstanding work. Service staff in this report felt that some young people in supported lodgings may have received less assistance from social workers because they were deemed less at risk and it was assumed that another organisation would step in and work with that young person.

Identification of recipient/targeting mechanism

Accessing clients was reported in both positive and negative terms in terms of its impact on project implementation. For example, 84% of projects in the evaluation of the Homeless Hospital Discharge Fund felt that they 'worked well' with local hospitals, participating in team meetings, ward rounds and staff training (Homeless Link, 2015). However, some hospital staff were reluctant to engage with the project due to their short-term nature or were not aware of the existence of the project. This meant that some patients could slip through the net back into homelessness if they were not referred to other wards in the hospital. If they were, it meant that valuable time was lost sourcing and securing appropriate discharge accommodation (Homeless Link, 2015 and Stoll, 2017). Wood et al. (2017) reported that recipients were often referred by external services on a more informal basis through information sharing between services and case management. Similarly, Sewel (2016) suggests that word of mouth is considered the most successful recruitment method into the programme. However, this casualisation of process and criteria can cause issues. Whilst it increases flexibility and staff discretion within the service, it can also create frustration for staff members when a client would be better suited to an alternative service (Wood et al., 2017). In settings for young people ageing out of care, Sewel (2016) further reports that incomplete referral information on the client (for example, about potential risks, challenging behaviours and triggers for behavioural issues) can inhibit the service. This can be a result of information not being recorded by the relevant body, concerns about information sharing or data protection and the challenges presented by the individual.

In a similar vein, Trutko et al. (2016) reported that careful screening and assessment of homeless veterans was necessary to ensure that new recruits were suitable and could benefit from the limited services HVRP funding could provide. For example, targeting resources to those who needed less support to gain employment but who were also interested in working, as opposed to offering intensive retraining to people approaching retirement age. In this case, the intervention did not increase motivation to access employment but assumed participants were already motivated. Sewel (2016) discusses an additional, participatory, dimension to decision-making and reports that providers value the opinions of young people and participants about how they benefit from placements and what should be on offer. This facilitates the decision-making process and informs the expectations of both providers and participants. Nevertheless, providers do agree that identification and referrals should be undertaken by professionals who understand the nature and opportunities of supported lodgings, particularly for those with high or complex needs. Overall, a clear definition of the client group is needed for greater efficiency and impact of any discharge programme (Homeless Link, 2015).

Implementing a successful targeting mechanism (protocol) for each service is essential to make sure the people who can benefit from a discharge programme are targeted. Although stepping outside of the agreed targeting protocol can be beneficial, it is likely to cause issues further down the line as clients may have been better placed in a different service. Finally, when creating and implementing targeting mechanisms, it is important to employ a level of participation from clients to inform the process, resulting in an identification process that satisfies all parties.

Referral route (e.g. defined agency or contact)

Planned pathways to referral are important, particularly those with one clear referral route and a single point of contact. 39 of the 41 projects evaluated by Homeless Link (2015) reported that they had created or developed an existing protocol for referral pathways. Those that demonstrated one clear referral route and a single point of contact were reported to benefit both staff and participants in institutions. However, a lack of data sharing continued to cause issues, particularly with hospitals not passing on salient information to accommodation providers.

Alignment with existing protocol/ procedures/ guidelines

One factor that influenced the successful discharge of participants was fidelity to protocol. Staff surveyed in the Homeless Link (2015) evaluation indicated that there had previously been an inadequate approach to homeless hospital discharge. In some areas, there was no protocol at all, in others the protocol was limited and in others still a protocol existed, but it was often not adhered to.

Sufficiency / adequacy of resources (space, time, staff, budget, appropriateness of services or facilities)

As with many interventions and services, sufficiency and adequacy of resources has a significant impact on their effectiveness. Interventionists working on a smaller project indicated that its size was an advantage as more time was allowed for one-to-one intensive support, tailored for each client. However, this could present issues in maintaining high levels of liaison with different partner organisations and links with hospital staff. This was the case for Barnardo's (Sewel, 2016), matching care leavers to suitable accommodation with providers at near capacity. They also suggested that streamlining and minimising paperwork for hospital staff increased capacity. Accessing accommodation was easier for hospitals that had a self-contained residential service on site. Similarly, Austin et al. (2014) found that permanently stationing teams or individuals ensured adequate support was available to veterans living within the catchment area. However, for this model to succeed, Jones et al. (2017) suggest that communication between health care specialists and project staff is key to co-ordinating efforts for homeless veterans.

Shared accommodation where multiple people discharged live together, due to lack of availability, can be an issue (Shelter, 2015) for some clients. Although supporting each other in shared accommodation may be helpful for some, staff felt that it was not appropriate for patients to feel like they must support each other due to a lack of staff. Other restrictions for some of the discharge programmes described in the process evaluations such as a maximum six-week stay meant that the intervention was not as meaningful or impactful as perhaps it could have been. This is in stark contrast to the interventions evaluated in the effectiveness synthesis, many of which lasted for 6 months or more.

Staff experience and expertise also arose as an issue for Jones et al. (2017) and Shelter (2015). One challenge in the report suggested that some hospital staff felt that they did not have the appropriate expertise for discharging patients who are homeless. Those who had received specific training were not necessarily dedicated to this service only; many worked in multiple wards (Shelter, 2015) and therefore could not provide the high level of support needed by patients.

Staff and case workers

Buy-in (commitment to programme)

As with many programmes and interventions, staff on the ground working with people who are at risk of homelessness are key to the success of an intervention. Homeless Link (2015) found that buy-in from hospital staff was essential for patients to be discharged effectively. They found that project workers with nursing backgrounds had a notable advantage as they were more readily able to obtain support from clinical staff. However, Homeless Link (2015) did report that some staff members or departments, particularly the emergency department, were sometimes unwilling or reluctant to engage, because of negative perceptions of people who are homeless held by hospital staff. Nurses also felt under time pressure and were failing to engage with housing services due to time constraints. Spending time organising housing logistics can often be at the expense of time spent on clinical support (Austin et al., 2014), therefore professionals' ability to provide the therapeutic support necessary to sustain housing, can be severely compromised. Similarly, Wood et al. (2017) provide a number of individual stories of staff working well together and pushing the limits of their work remits to make sure that people who are homeless and have multiple needs were able to access the best support. In this example, staff attitudes and actions at St Vincent's Hospital Melbourne matched that of the intervention philosophy, in their flexibility, knowledge, understanding and ability to empower patients who were homeless. Wood et al. (2017) and Sewel (2016) report that when staff were trusting, non-judgemental, respectful, compassionate and responsive, their clients felt valued and not burdensome to the service.

Communication and engagement with programme recipient

Kertesz et al. (2013) reported that patient experiences were usually influenced by provider and environment characteristics. Patient/provider relationships and perceptions were most favourable when programmes were tailored, whereas unfavourable experiences were up to twice as likely in settings with less personalisation. Similarly, Homeless Link (2015) report that although not all clients wanted support at the point of discharge, those who did were given personalised and individualised support through a support plan developed with the client Link workers tended to work with a small number of clients and were able to arrange weekly visits along with daily phone calls. Trutko et al. (2016) reports comparable data where specialists working with veterans on a one-on-one basis, met the needs of the participant more effectively from intake through to discharge when support was no longer needed.

Communication and engagement with other agencies

Homeless Link (2015) reported that many project staff found it 'easy' to access wards and staff as hospital staff were responsive and engaged, reflecting good relationships at all levels. Project workers were able to involve themselves in hospital processes and were able to connect with patients at an early stage. However, in some cases, staff were very busy and not able to respond fully or able to remember the purpose of the project. This could have been a result of poor communication and led to a lack of referrals. On the other hand, when communication with other agencies is open and clear, client needs can be fulfilled successfully. For example, Trutko et al. (2016) found homeless veterans with criminal records can be challenging to place into jobs and discharge into permanent housing, particularly those convicted of sexual and violent crime. To mitigate this issue, Trutko et al. (2016) found that developing relationships through good communication with local employers was key to job placement and retention.

Technical skills (capabilities, training)

Homeless Link (2015) have suggested that more intensive staff training is needed for those working in emergency departments in order to mitigate against the high turnover and shorter contact time. One suggestion from the report is that introducing the role of a homeless champion within the emergency department could achieve this. Shelter (2015) did implement staff training in the Cornwall project during induction in order for hospital staff to better understand the pathways available to patients who have the potential to become homeless upon discharge.

Recipients of the programme

Buy-in (emotional acceptance of programme)

The ultimate aim for the selected discharge programmes is to improve the lives of the participants involved in the intervention. Along with other factors and levels of influence, participant buy in to the programme is critical to its success. For many discharge programmes, this will mean implementers spending time with participants and at the very least, involving them in decision making processes. For example, in their study of young people leaving the care system, Sewel (2016) recommends that each young person is encouraged to be involved in the decision-making process right from the beginning of their engagement with the service. If this is not achieved, placements can break down, no matter how much support a young person receives. Similarly, Stoll (2017) discusses the importance of engaging with veterans well before they leave the armed forces. In their study,

35 out of the 40 veterans surveyed felt that the project staff had listened and understood what was important to them. Conversely, Trutko et al. (2016) found that some participants were not interested in long term training. This may be correlated with the fact that over 50% of participants were over 45 years of age and nearing retirement. This mismatch between intervention goals, client needs and preferences can lead to participants becoming disgruntled and disengaged with the service. In order for programmes to have a chance of working, the programme needs to match the clients need and preferences. Spending time getting to know clients and involving them in decision making (or better still, clients lead the decision making) can improve engagement.

Access to non-housing support (medical, financial, training etc.)

For people who are vulnerable to homelessness, the immediate goal for many discharge programmes is to house these populations in stable and secure accommodation. However, clients may need further support in other areas of their lives. For example, ex-prisoners released from prison or veterans, may need support in retraining in order to gain employment. For example, Stoll (2017) offered education and employment support to veterans enrolled in their programme, however, Trutko et al. (2016) found that a barrier to employment for homeless veterans was a lack of reliable or inexpensive public transportation. This made employment less available and out of reach for some clients, therefore limiting their progress in other areas of reintegration. Homeless Link (2015) found that clients described the support given as a 'lifeline.' These included welfare benefits, ensuring that GP registration was set up, getting prescriptions, counselling and linking people to adult social services. Homeless Link (2015) view these services with the same importance as housing to improve recovery and the discharge process. Sewel (2016) recommended in her report that mental health providers needed to provide more timely responses to young people in order to prevent breakdowns or reaching crisis point during placement.

Particular groups of individuals have specific needs that need to be met within discharge programmes. For example, ex-prisoners discharging from prison who intend to live with family may encounter significant negative influences that could impact upon their successful reintegration into the community (Lutze et al. 2009). Veterans are another population who can face significant challenges after discharge. If physically injured, they may need extra help in finding appropriate accessible accommodation and further employment (Stoll, 2017). Overall, discharge programmes may need to address more than just housing alone and take a holistic approach to supporting every individual.

Housing-related security

Not only is it important for people who are at risk of homelessness to have access to accommodation, but it is also crucial that this housing is stable and secure. Unstable and insecure accommodation can cause additional issues and anxieties however, discharge programmes can have difficulties mitigating this. For example, Homeless Link (2015) suggest that although the main outcome for all projects in their evaluation was that people at risk of homelessness had suitable accommodation available to them on discharge, it was often difficult to secure long term, permanent accommodation for clients prior to discharge. This was due to a lack of available properties but also clients, who were single and ineligible for council assistance as they tended to be more difficult to house. However, in contrast to a Housing First approach, Trutko et al. (2016) found that placing veterans in permanent housing prior to addressing underlying issues may be counterproductive in retaining employment and housing. They felt that maintaining participants in on-site transitional housing was helpful in providing a structured environment whilst engaging with the service. This is analogous to the respite care (Buchanon 2006, Buchanon 2009, Kertez 2009, Sadowski 2009) or transitional housing (Conrad, 1998) models evaluated in the effectiveness studies. In particular, for those with ongoing health care needs, specialist respite care or transitional housing may be appropriate to enable clients to access the specialist care they need outside of a hospital environment until they are ready to move to more permanent community living. Comparatively, Sewel (2016) provided support for participants and independent living by helping young people to find potential properties online, advising on different areas to live, helping them to apply to council housing lists and helping them to physically move to their new home. So, for some people transitional housing is helpful, while for others moving directly to a permanent home is more suitable.

Adequacy of information provided

For some clients, adequate information was not available and they may have missed out on important discharge programmes. This could extenuate harmful circumstances for those vulnerable to homeless before eventually finding support. Stoll (2017) has suggested that not all military veterans knew about their service, recommending that military personnel pre-enrol for services before leaving the armed forces and that the promotion of pre-discharge agencies should be prioritised in leaving packs via flyers. Homeless Link (2015) similarly suggested that patients had much more positive experiences with the discharge process when they received sufficient notice about when and where they were going. Having a clear housing outcome was key in this process. Often it was a breakdown in communication between staff and patient that resulted in negative experiences. Trukto et al. (2016) reported that having access to wraparound,

comprehensive services is beneficial in meeting the wide variety of needs of homeless veterans and moving them towards self-sufficiency.

Accessibility (time and place)

Often, for people who are homeless, flexibility in when they can access services, in particular facilitating access outside normal working hours is helpful. Shelter (2015) suggest that accessibility outside of normal working hours, Monday to Friday 9am-5pm, was reduced as they could not admit clients outside of these hours. This Cornwall study felt that opportunities were missed in this regard given the likelihood that support would be most needed at weekends and during the evenings. They recommend extending this service to 7 days a week.

Particular groups of individuals have specific needs that need to be met within discharge programmes, that may hinder access to appropriate accommodation. For example, people discharging from prison who intend to live with family may encounter significant negative influences that could impact upon their successful reintegration into the community (Lutze et al. 2009). Veterans are another population who can face significant challenges after discharge. If physically injured, they may need assistance to find appropriate accessible accommodation and further employment (Stoll, 2017).

05

Discussion

Summary of main (quantitative) results

Thirteen studies (represented by 18 reports) were identified from CHI's EGM and were included in the current analysis to investigate the effectiveness of discharge programmes for improving outcomes for individuals experiencing, or at risk of experiencing, homelessness. This represents a very small proportion (6%) of the total number of studies (n=225 reports) included in the effectiveness EGM. Not all 13 studies were included in any one meta-analysis in this review and so the small number of studies – all of which were conducted in the USA - on which the reported analyses are based, should be borne in mind when interpreting the results. The methodological quality of the studies included in this review was mixed (only 31% of studies were rated as having low risk of bias, the remainder were rated as moderate to high risk of bias). Ultimately, the limited number of studies included in this review, as well as their limited methodological quality underscore the need for more high-quality studies providing more, rigorous data to increase our certainty in our conclusions.

Nonetheless, the emerging evidence from this review is positive and promising suggesting that discharge programmes can be effective in reducing homelessness and, for hospital discharge programmes, in reducing post-discharge hospitalisations. In addition, they may be effective in reducing incarceration, particularly reducing reincarceration in those discharging from prison/jail. There is also incipient evidence from a small number of studies suggesting potentially favourable effects in other domains such as alcohol and drug use, visits to emergency departments, family contact and cost. There is currently insufficient evidence to draw any conclusions in relation to mental or physical health, or employment. However, the meta-analyses also uncovered potentially adverse outcomes (higher number of deaths), but the results remain very imprecisely estimated and will require further exploration.

Primary outcomes

The primary outcomes of interest in the current review were housing stability and health. Five studies could be included in a meta-analysis of housing stability and showed that the discharge programmes included in this analysis decreased the incidence of homelessness in the intervention group (SMD=-0.71, 95%CI [-1.31, -.12], p=0.02) compared to the comparison group. An effect size of 0.71 indicates a large positive effect of the intervention i.e. compared to usual care, discharge programmes substantially improved housing stability for people leaving institutional settings. However, there is uncertainty around this estimate, as indicated by the 95% confidence interval, such that an effect size as large as -1.31 or as small as -0.12 would also be consistent with the data reported

here. Three of the five studies evaluated discharge from hospital, one evaluated discharge from foster care and one evaluated discharge from prison.

Within the health domain it was possible to look at separate health outcomes. There were sufficient data (six studies) to conduct a meta-analysis for hospitalisation only, which showed that on average, compared to usual care, discharge programmes reduced the average number of days a person spent in hospital after leaving an institutional setting (SMD=-0.52, 95%CI [-0.95, -0.09], p=0.019). However, the uncertainty around this estimate, as denoted by the 95% confidence interval, indicates that an effect size as large as -0.95 or as small as -0.09 would also be consistent with the data reported here. All six studies evaluated programmes focusing on discharge from hospital (psychiatric (n=3) and physical health (n=3)).

Due to the very small number of studies included in each meta-analysis, effects were not analysed separately according to study type (RCT/non-RCT), risk of bias or time point of data collection. Neither was it possible to conduct the planned sub-group analysis. Due to insufficient data the remaining health outcomes (emergency department visits, mental health, physical health, alcohol and drug use and STIs) were narratively synthesised. No firm conclusions can be drawn with regard to the effectiveness of discharge programmes in improving these particular health outcomes. However, the small number of studies that do exist are promising and suggest favourable effects in other health domains, including reduced alcohol and drug use and reduced visits to emergency departments.

Secondary outcomes

Crime and justice

Four studies reported data on incarceration and were included in a meta-analysis. Three studies examined discharge from prison, the fourth examined discharge from hospital. Results showed a reduction in the odds of the intervention group being incarcerated compared to the control group,

(OR= -0.41, 95% CI [-0.86, 0.04], p=0.07). Combined data from these four studies suggest that 214 fewer people per 1000 were incarcerated with treatment (542 in 1000 with no programme/ services as usual vs 328 in 1000 with discharge programmes). Taking into consideration the uncertainty around this estimate, the results are also consistent with effects where 500 fewer people were incarcerated, and 37 more people were incarcerated.

Capabilities and wellbeing

Within this outcome domain there were insufficient data to include in a meta-analysis. Whilst three studies reported measuring a relevant outcome, only one reported this data (Herman 2011), which measured frequency of family contact, satisfaction with family relationships, physical integration and social integration, post discharge from psychiatric hospital. The study reported that taking part in the intervention resulted in improved family contact (d=1.07, indicating a large improvement in family contact, however the uncertainty of this estimate was not reported, p=0.02).

Access to services

No studies reported on access to services.

Employment and Income

No studies reported useable data on employment and income outcomes

Cost- Economic Commentary

Six studies reported some cost data, however lack of detailed reporting on costs, inconsistent approaches to modeling costs and differences in the prevailing economic, health and social care contexts in each study meant that no quantitative synthesis of the cost or cost-benefit of implementing discharge programmes was possible. However, the included studies do indicate that discharge programmes may be cost-effective compared to usual care. These programmes typically cost more to implement than usual care, which is unsurprising as services as usual may involve little more than referral to homeless shelters, more staff are needed to deliver individualized services common to discharge programmes and there is greater spend on housing or respite care. This initial investment is likely to be offset by substantial cost savings in the longer term associated with reduced homelessness, hospitalisation, reincarceration and associated crime and other costs to society. All of the studies were conducted in the USA and so we do not know if these same benefits would accrue in other countries with very different health, justice and social care contexts. There is a need for a comprehensive cost-benefit analysis, particularly in the UK context, to fully understand the potential return on investment in discharge programmes.

Adverse outcomes

Death

Four studies reported the number of deaths in the intervention and control groups. Of the four studies included in the analysis, on average, the odds of

death were slightly higher in the intervention group compared to the control group (OR=0.16, 95% CI [-0.35, 0.67]). This translates to 66 more deaths per 1000 in the intervention group (57 in 1000 died without intervention, and 123 in 1000 with intervention). The statistical uncertainty around the results is also reasonably consistent with the interventions causing between 17 fewer and 77 more deaths.

Overall completeness and applicability of evidence

The included studies were all conducted in the USA. We cannot assume that the effects achieved by programmes that are designed and delivered in one country or even state, with its own specific policy, economic, health, justice and social care context, will automatically transfer to another country. It is not possible to say with any certainty whether the effects observed in this review would translate into similar effects here in the UK, or elsewhere in the world, given the differing social and economic contexts.

There are a number of studies which did not report useable data. We have contacted the authors to request summary data and any that is forthcoming will be included in a future update of this review. This notwithstanding, the review includes a small number of studies, with fewer still reporting on the same outcomes. For this reason, the meta-analyses reported above should be interpreted with caution bearing in mind that if additional studies were included in a future update of this review, the picture may change. Certainly, as it currently stands there is insufficient data to draw any strong conclusions regarding the majority of secondary outcomes in this review and there is a clear need for more high quality research examining the effectiveness of discharge programmes for improving outcomes for individuals experiencing, or at risk of experiencing, homelessness.

The studies cover a broad range of discharge programmes acknowledging the variety of institutions that individuals can be discharged from and the complexity of needs each programme aims to address. Unfortunately, there were insufficient studies to explore whether the institutional setting people were discharging from had an impact on the effectiveness of the programme. We do not know which settings are likely to produce greatest effects from these programmes. We also were unable to analyse differential effects on different groups of people (such as men and women, younger and older people) so we do not know who these programmes may work best for.

Quality of the evidence

The quality of the evidence varies across the 13 included studies. Five of the 13 were non-randomised studies and four of these five non-randomised studies were of moderate to serious risk of bias (one was low risk of bias). Of the eight RCTs, three were low risk of bias, four were moderate and one was high risk of bias. Given the small number of studies overall and the variability in rigorous study design as well as methodological quality, it is further reason to be circumspect about the findings of the review. Examination of the forest plots from the meta-analyses show reasonably wide confidence intervals, indicating the uncertainty with which the synthesised effects are estimated. Promise of effectiveness is indicated by the analyses for housing stability and hospitalisation outcomes and possibly incarceration. Equally there was an indication that adverse outcomes (death) are a potential risk. More, high quality evaluation research is required, especially in other countries and regions outside the USA.

Limitations and potential biases in the review process

Unlike typical systematic reivews, this review was based on searches conducted by another research team and we relied on two Evidence and Gap Maps already commissioned by CHI to find the eligible studies. These EGMs were built according to Campbell Collaboration standards and guidelines and this is a novel endeavour, for a separate author team to use the studies included in an EGM as the sole source of studies for a systematic review. The advantage of this approach is that making use of well-constructed maps of evidence can reduce research waste in unnecessary duplication of effort. The studies are all from high-income countries and future updates could consider also including studies conducted in low-income countries.

Summary of implementation and process (qualitative) findings

To help deepen our understanding of the factors that may influence the success of discharge programmes for individuals experiencing, or at risk of experiencing, homelessness, the analysis of the qualitative data followed a framework with five main analytical categories or levels of influence: contextual factors, policy makers/funders, programme administrators/managers /implementing agencies, staff/case workers and recipients of the programme.

Contextual factors

The primary issue raised in relation to context was the lack of stable, affordable accommodation and the variability in the rental market, such that actually sourcing accommodation to discharge individuals into can be extremely challenging, regardless of the mechanism (or programme) being used to do so. Accessing employment opportunities was also cited as an important challenge, arising from a lack of relevant skills and expertise required by the local job market.

Policy makers and funders

The importance of integrated, timely funding and partnership working emerged as a key theme to facilitate successful implementation of discharge programmes. A collective approach was especially supported if there was buy-in from those in leadership positions and delivered in the context of a strong and coherent policy framework.

Programme administrators, managers and implementing agencies

Integration and buy-in was also considered key at these strategic levels of ‘on the ground’ implementation. Forging positive relationships and identifying key ‘point people’ to manage and coordinate inter-agency communication was seen as very important. A potential risk however is that multi-agency working can lead to individuals being inappropriately referred or ‘falling through the cracks’ in the absence of adequate communication between agencies to ensure appropriate services are delivered as intended. Clarity around referral procedures is important however having adequate resources, information and training to provide and deliver services to individuals with complex needs, remains a constant challenge to both service providers and clients.

Staff and case workers

Staff and case workers were identified as essential to the success of an intervention especially when staff values align to an intervention's philosophy. Lack of time, experience, training, resources and negative perceptions of people who are homeless were specifically identified as barriers to providing effective housing support. Providing tailored and individualised support was likely to be the most positively perceived support by those in receipt of the programme.

Recipients of the programme

Effective and meaningful engagement with clients and where possible, involving individuals in decisions about their discharge and future placement, was considered essential to avoiding placement breakdown or crisis post discharge. Logistic issues were also considered important to successful discharge, for example, access to reliable and affordable public transportation, increased access to health and social care services, more timely mental health intervention. Transitional housing was considered helpful in providing structure post-discharge and receiving sufficient information and notice about moving was related to having a more positive experience of being discharged.

Authors' conclusions

This is the first review to look specifically at the effectiveness of discharge programmes for individuals who are homeless, or those at risk of homelessness. Therefore, even though the findings are mixed and based on a small number of studies, it is currently the best available evidence of the effectiveness of such approaches for improving outcomes for people experiencing homelessness. It is encouraging therefore that it appears at this early stage of generating rigorous evidence in this particular field, that discharge programmes – certainly in a USA context - can be effective in reducing homelessness and hospitalisations post discharge. This review is also unique in that it includes a synthesis of qualitative data exploring process and implementation issues associated with delivering discharge programmes. This additional data provides valuable insights into the complexities associated with delivering such programmes and the multi-agency working and partnership that is required. The fact remains however, that an important element of any programme's success depends on the capability and enthusiasm of those who work in the area. Given the continued landscape of funding restrictions and a sustained shortage of available stable and affordable housing, delivering and implementing discharge programmes remains an ongoing challenge.

Implications for practice and policy

Discharge programmes focus around a risky point of transition, times which are known to create the risk of (repeat) homelessness and other negative outcomes. They often seek to break or prevent a costly cycle of interactions with institutions.

The quantitative evidence summarised in this review is exclusively from the USA and of mixed methodological quality. However, it suggests that discharge programmes can be promising interventions to reduce homelessness and hospital admissions. As such, they should be considered as part of homelessness provision and strategy, if there is a problem in this area locally or nationally. Commissioners, funders, providers and investors can reasonably expect discharge projects to have positive outcomes in the areas of housing stability and health and possibly incarceration. There is some promising, but not conclusive, evidence that discharge schemes may have positive outcomes in terms of substance misuse in the short term, reduced visits to emergency departments and increased family contact. There is currently insufficient evidence to draw any conclusions in relation to mental or physical health, or employment. The evidence around adverse outcomes (potential increased risk of death) is inconclusive but does require further exploration. Decision-makers may also expect discharge programmes to be cost effective in terms of reducing costly interactions with acute health services and may reduce costly incarceration and associated crime. However, additional, robust evidence is necessary to ascertain the impact of these interventions across a range of outcomes, and more comprehensive cost-benefit analyses are required to quantify the returns on investments made to improve discharge policies.

When considering this evidence, it is important to bear in mind that the impact evaluations are from the USA and there is a lot we still do not know about the effectiveness of discharge programmes. Whilst the evidence is promising, the mechanism by which these programmes might translate to a non-USA setting will need to draw on the local context, and can be informed by the evidence from the qualitative data synthesised in this report. This qualitative evidence - from the USA, UK and other countries - tells us that the success of discharge projects is linked to the prevailing policy environment around homelessness, housing and related areas such as welfare policy. It provides valuable insight into some of the practical service delivery factors which may impact on the success of discharge projects.

Key implementation messages

The following key implementation messages to emerge from this report are separated into three broad categories, below: delivery, local strategy and funding, and broader regional and national policy considerations. The qualitative evidence suggests that many of the principles of Trauma Informed Care are applicable to discharge projects including flexible, compassionate, person-centred, non-judgemental approaches. If working with people with multiple needs small caseloads and regular contact are likely to be more effective than less intensive approaches. These principles are reflected throughout the messages, below.

Delivery

- The essential components of a discharge programme are assessing people's needs and supporting them to meet those needs, planning for the period around discharge, and coordinating services. These need to be clearly reflected in a protocol that identifies the role of different stakeholders.
- The mode of delivery varies widely. Discharge projects can be stand-alone and offer a case management approach with referral to services and overall coordination of care or be integrated with other services to directly provide housing, health care and other support services.
- As well as direct intensive support services discharge projects can seek to influence practice across existing professionals e.g. through creating hospital discharge protocols which can be applied to all patients at risk of homelessness rather than just the beneficiaries of a specific service.
- The most common mode described in the review was tailored individual support. The extent and period of support depends on the support needs of the client. In general, a key component was having adequate staff time to build a relationship with the client to really understand that person's needs. The time this takes will vary depending on the complexity of needs and the population.
- Access to accommodation is a key consideration. Depending on the programme, projects might need: quick access to transitional accommodation, respite care, access to longer term flexible, affordable and sustainable accommodation. The choice of accommodation must reflect the needs of the person, as well as their views and preferences.

- Clear identification of the target group, appropriate referral systems, and the right delivery team are important. Considerations include:
  - Designing the protocol in consultation with the staff delivering it can help ensure the necessary buy-in for it to be implemented with fidelity
  - Stating clearly who the service is appropriate for and ensuring the intensity and duration of the service is appropriate for the target group (e.g. lower caseloads for those with high support needs who need support engaging with other services)
  - Consulting with those to be discharged to consider their needs and preference
  - Ensuring referrals are not burdensome and identifying a single point of contact for referrals when possible, or establishing clear communication mechanisms between agencies when a more coordinated approach between multiple stakeholders is required
  - Ensuring that information about the service gets to those who can identify the target group
  - Promoting parity of professional esteem between staff in institutions and those working in discharge projects, for example, by working together to identify appropriate options post-discharge
  - Identifying the best time to commence the intervention; for example, building up trust and selling the offer will be important to some groups whereas a very quick response to an unpredictable discharge point will be important in other instances
  - Considering multi-disciplinary teams and considering clinical team members for hospital discharge schemes
  - Considering the development of broader protocols for good practice around discharge for people at risk of homelessness; for example, clinical staff being trained in identifying risks and referring to the relevant organization
  - Aligning discharge protocols with other performance management indicators; for example, hospitals pay fines by discharging too late, this could be changed so that they pay fines if they discharge into homelessness instead

Local strategy and funding

- The impact of discharge programmes will depend on the overall network of services that people at risk of homelessness, including those with multiple needs, require. Discharge services are highly dependent on the other services around them.
- Access to housing and housing supply will impact on schemes. It is paramount to ensure sufficient accommodation alternatives are available, and that these are well-suited for the needs and preferences of those being discharged.
- Short term projects are less sustainable because they do not allow projects sufficient time to build in development time to recruit staff and create the right processes for success.

Broader regional and national policy considerations

- Encourage joint commissioning which brings together different departments and stakeholders. Effective discharge policies require the coordination of multiple public services to be effective and thus would benefit from a collaborative approach to fund the interrelated components of these interventions.
- Discharge projects play a coordination role between multiple public services. For them to be effective, the ecosystem of services around which they operate is critical to the success of discharge policies.

Implications for research

The few studies that do exist in this area suffered from inconsistencies in measurement and reporting of important outcomes. This inconsistency would be greatly improved by the development of core outcome reporting sets for interventions in this area alongside adherence to established reporting guidelines for intervention evaluations such as CONSORT guidelines. This should include outcome domains often reported in these studies such as housing stability and health-related outcomes, but also others that have received less attention to date such as the impacts on employment and income. Exploring unexpected adverse outcomes, particularly the number of deaths, should also be an integral part of a more comprehensive framework to understand the holistic impacts of these interventions.

There was not enough evidence to analyse whether the institutional setting had an impact on the effect of discharge programmes. More studies evaluating programmes in different settings will move the evidence base forward and improve our understanding of the applicability of these programmes across different institutional settings. We know that discharge programmes can work, we now need to know in which settings they work best.

Similarly, we were unable to disentangle the effects of intervention for different groups of people. We do not yet know if men and women experience these programmes differently, what effect clients age, ethnicity or complexity of needs has on the effectiveness of these programmes. Future studies investigating these questions would aid our understanding of who these programmes might work best for, and how to tailor programmes to better meet the needs of discrete groups of people.

There is an urgent need to understand the potential return on investment in discharge programmes which would require a more conscientious exploration of both benefits and costs, with particular attention to the context where policies are implemented. All of the effectiveness studies included in this review were conducted in the USA and the findings may not translate to other economic, health, justice, social care or cultural settings such as the UK. We cannot assume that programmes can be 'exported' to another country and so we encourage those interested in implementing discharge programmes to firstly, consider modifications that may be necessary to suit the local context and secondly to include a robust evaluation of the programme effectiveness and implementation. Researchers and practitioners may also consider developing context specific programmes that are based on the existing evidence and recommendations for practice provided.

This review identified eight RCTs and so randomisation and good study design is not a barrier to implementing and testing discharge programmes in a range of institutional settings. We encourage any teams seeking to evaluate a similar programme to use an RCT design to ensure any future evaluations provide robust evidence.

06

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*** denotes inclusion in the qualitative synthesis**

**** denotes inclusion in the quantitative synthesis**

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THE CALCULUS
WITH ANALYTIC GEOMETRY
THE PEOPLE'S ALMANAC
HOMO SAPIENS
GRAY'S ANATOMY
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07

Appendices

Appendix 1

Data collection form for homelessness reviews

1 Sographic information	
Article ID	FREETEXT
Linked articles	FREETEXT
Extracted by	FREETEXT
Checked by	FREETEXT
Year of publication	FREETEXT
Type of publication	1. Journal Article 2. Book/book chapter 3. Government report 4. Conference proceedings 5. Presentation 6. Thesis or Dissertation 7. Unpublished report 8. Other (please specify)
Location of study (The location in which the study is set not where the study authors are based.)	1. UK 2. ROI 3. Rest of Europe 4. USA 5. Canada 6. South America 7. Central America 8. Oceania 9. Middle-East 10. Asia 11. Africa 12. Other (Please Specify) 13. Not Specified
Stoll (2017)	1. Research council funding 2. University scholarships and bursaries 3. Salaried research assistantships from university departments 4. Grants or loans from trusts and charities 5. Local enterprise initiatives 6. Company sponsorship 7. Government loans 8. EU Scholarships 9. Industry sponsorship 10. Other (please specify)
Possible conflicts of interest	1. Yes, possible/definite conflict of interest 2. No, study appears to be free of CoI 3. Can't tell

2 Participant information	
Recruitment setting Where were participants recruited from?	1. Clinical setting 2. Accommodation for individuals experiencing homelessness 3. Family home 4. The street 5. Community setting 6. Referred by friends or family 7. Referred by medical health professional 8. Housing Agency 9. Other (Please specify)
Homelessness Status at intake Describe the housing status of the sample at intake and/or any information given about housing status prior to intake. Tick all that apply and try to extract numbers were available. Homelessness is defined as those individuals who are sleeping 'rough' (sometimes defined as street homeless), those in temporary accommodation (such as shelters and hostels), those in insecure accommodation (such as those facing eviction or in abusive or unsafe environments), and those in inadequate accommodation (environments which are unhygienic and/or overcrowded).	1. Sleeping 'Rough' (or rooflessness) 2. Temporary Accommodation 3. Insecure Accommodation 4. Inadequate Accommodation 5. Involuntary sharing e.g. domestic violence 6. Hidden/concealed homelessness 7. Other (please specify) 8. Not Specified
Geographical context Where participants receive treatment?	1. Urban 2. Rural 3. Suburban 4. Mixed 5. Other (please specify) 6. Not Specified
Gender % (actual number)	FREETEXT
Age Extract mean age, SD and range. Choose multiple options if the analysis is reported separately for different age groups.	1. Under 25 2. 25 and Over
Complexity of needs What other challenges does the individual face, if any, aside from the risk or experience of homelessness? High Risk of Harm and/or Exploitation - For example, women in shelters, newcomer families, refugee/asylum seeker, care leavers	1. Poor Physical Health 2. Poor mental health 3. Incarceration 4. Substance Abuse Issues 5. Care leaver 6. Limited access to integrated support services 7. High Risk of Harm and/or Exploitation 8. Other (please specify) Not Relevant Not Specified

2 Participant information (Continued)	
Mental health status	1. Receiving treatment 2. Not receiving treatment 3. Other (please specify) Not relevant Not Specified
Substance use status	1. Receiving treatment 2. Not receiving treatment 3. Other (please specify) Not relevant Not Specified
Homelessness status Homelessness is defined as those individuals who are sleeping 'rough' (sometimes defined as street homeless), those in temporary accommodation (such as shelters and hostels), those in insecure accommodation (such as those facing eviction or in abusive or unsafe environments), and those in inadequate accommodation (environments which are unhygienic and/or overcrowded).	1. Sleeping 'rough' 2. Temporary accommodation 3. Insecure accommodation 4. Inadequate accommodation 5. Other (please specify) Not Specified
Family vs. No Family Family = any child involved Non-family = single person or couple without children If mixed sample select both and describe	1. Family 2. Non-Family Not Specified
Sample size of treatment group Number of people assigned to treatment. If more than one treatment group extract all and be clear which group is which.	FREETEXT
Sample size of control group Number of people assigned to control. If more than one control group extract all and be clear which group is which.	FREETEXT

3 Intervention information

How many intervention arms in this trial? List how many study arms there are and given each a name. e.g. Intervention = Critical Time Intervention; Control = Treatment as usual If there is more than one intervention arm go to the “Study Arm” tab and add the RELEVANT study arms. You must then extract data for each relevant study arm.	FREETEXT
Name of intervention Write in the name of the programme, intervention, or treatment under study. This may be specific like ‘critical time intervention’ or it may be something more generic like ‘supported housing’	FREETEXT
Briefly Describe the intervention Briefly describe the intervention, what participants are offered and any important factors such as conditionality, nature of housing, case management, substance abuse treatment included etc.	FREETEXT
Theory of change How does the intervention aim to bring about change? What is the underlying theoretical rationale for why the intervention might work to improve outcomes? If not specified write “not specified”	FREETEXT
What is the size of accommodation/ How many beds?	FREETEXT
Duration of treatment period from start to finish In the dosage items, we are interested in the amount of treatment received by the participants. If the treatment was delivered directly to participants, the authors will probably provide at least some information about dosage and you can code these items accordingly. If minimal information is provided, you should try to give estimates for these items if you can come up with a reasonable estimate.	FREETEXT

3 Intervention information (Continued)	
Timing Frequency of contact between participants and provider/ programme activity	1. Once a month 2. Less than weekly 3. Once a week 4. 1-2 times a week 5. 2 times a week 6. 2-3 times a week 7. 3 times a week 8. 3-4 times a week 9. 4 times a week 10. Daily contact Can't Estimate
Length of each individual session	FREETEXT
Study Personnel The primary individual/s who have direct contact with the participants served by the programme. If the report is the author's dissertation (or based on the author's dissertation), then code as "Graduate Researcher". If the delivery is performed by graduate or undergraduate students assisting the author then select "Grad/Undergrad Students". Code "Self-directed" for studies where electronic / computer programmes are used. If the intervention is solely environmental i.e. community housing, then code "environmental change"	1. Graduate Researcher 2. Grad/Undergrad Students 3. Author 4. Homelessness professional 5. Includes case manager, social worker, outreach worker 6. Peers 7. Interventionist (Not Hired by Researcher) 8. Interventionist (Hired by Researcher) 9. Self-Directed 10. Medical Professionals 11. Other (please specify) Not Specified
Did provider receive specialised training? This refers to whether or not the 'interventionist' received specialised training to equip them to deliver the intervention proficiently.	1. Yes 2. The interventionist IS programme developer 3. No Not specified
Resource requirements Time, staff, housing provision etc	FREETEXT
Cost	FREETEXT

4a Study Design

Design

The studies included in all reviews must include an intervention group and at least one untrained control group. Control groups can include placebo, no treatment, waitlist, or treatments vs 'treatment as usual'. Any study which includes one group pre-test/post-test or in which a treatment group is only compared to another treatment group will not be eligible for inclusion.

1. Randomised control trial

Individual or cluster randomised

2. Non-randomised control trial

What do control subjects receive?

1. Placebo (or attention) treatment. Group gets some attention or a sham treatment
2. Treatment as usual. Group gets "usual" handling instead of some special treatment.
3. No treatment. Group gets no treatment at all.

1. Placebo
2. Treatment as usual
3. No treatment

Not specified

Unit of allocation

Individual (i.e., some were assigned to treatment group, some to comparison group)

Group (i.e., whole subsets assigned to treatment and comparison groups)

Regions (i.e., region assigned as an intact unit)

1. Individual
2. Group
3. Regions
4. Other (Please Specify)

Not Specified

Method of assignment

Method of group assignment. How participants/units were assigned to groups. This item focuses on the initial method of assignment to groups, regardless of subsequent degradations due to attrition, refusal, etc. prior to treatment onset.

1. Randomly after matching, yoking, stratification, blocking, etc. The entire sample is matched or blocked first, then assigned to treatment and comparison groups within pairs or blocks. This does not refer to blocking after treatment for the data analysis.
2. Randomly without matching, etc. This also includes cases when every other person goes to the control group.
3. 3. Regression discontinuity design: quantitative cutting point defines groups on some continuum (this is rare).
4. Cluster assigned, this is to be used in cluster assignment studies only, specify the number of clusters in the treatment group and the number of clusters in control.
5. Wait list control or other quasi-random procedure presumed to produce comparable groups (no obvious differences). This applies to groups which have individuals apparently randomly assigned by some naturally occurring process, e.g. first person to walk in the door. The key here is that the procedure used to select groups doesn't involve individual characteristics of persons so that the groups generated should be essentially equivalent.
6. Non-random, but matched: Matching refers to the process by which comparison groups are generated by identifying individuals or groups that are comparable to the treatment group using various characteristics of the treatment group. Matching can be done individually, e.g., by selecting a control subject for each intervention subject who is the same age, gender, and so forth, or on a group basis.

1. Randomly after matching
2. Randomly without matching
3. Regression discontinuity design
4. Cluster assigned
5. Wait list control
6. Non-random, but matched
7. Other (Please Specify)

Not Specified

4a Study Design (Continued)

Was there >20% attrition in either/ both groups? Attrition occurs when participants are lost from an intervention over time or over a series of sequential processes. Studies may describe this as 'lost to follow-up', or 'drop outs'.	FREETEXT

4b Non-randomised studies

How were groups matched? If matching was used prior to assignment of condition, how were groups matched?	1. Matched on Pre-test measure 2. Matched on personal characteristics 3. Matched on demographics 4. Groups weren't matched 5. Other (please specify) Not specified
Was the equivalence of groups tested at pre-test?	FREETEXT
Results of statistical comparisons of pre-test differences	1. No statistically significant differences 2. Significant differences judged unimportant by coder 3. Significant differences judged of uncertain importance by coder 4. Significant differences judged important by coder 5. Other (please specify)
Were there pre-test adjustments?	FREETEXT

5 Qualitative information

Qualitative methods used	FREETEXT
Data analysis technique and procedure	FREETEXT
Was the intervention implemented as intended?	1. Yes 2. No Not specified
How was this measured?	FREETEXT
What implementation and process factors impact intervention delivery?	1. Contextual factors 2. Policy makers / funders 3. Programme managers/Implementing agency, 4. Staff / case workers 5. Recipients

6 Assessing quality in RCTs (Cochranes ROB2 tool)

Domain 1 Risk of bias arising from the randomization process

1.1 Was the allocation sequence random?	1. Yes 2. Probably yes 3. Probably No 4. No
1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?	1. Yes 2. Probably yes 3. Probably No 4. No
1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?	1. Low 2. High 3. Some concerns
Risk-of-bias judgement	1. Yes 2. Probably yes 3. Probably No 4. No
Optional: What is the predicted direction of bias arising from the randomization process?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable

Domain 2 Risk of bias due to deviations from the intended interventions (effect of assignment to intervention)

2.1. Were participants aware of their assigned intervention during the trial?	1. Yes 2. Probably yes 3. Probably No 4. No
2.2. Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?	1. Yes 2. Probably yes 3. Probably No 4. No
2.3. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the experimental context?	1. Yes 2. Probably yes 3. Probably No 4. No
2.4. If Y/PY to 2.3: Were these deviations from intended intervention balanced between groups?	1. Yes 2. Probably yes 3. Probably No 4. No
2.5 If N/PN/NI to 2.4: Were these deviations likely to have affected the outcome?	1. Yes 2. Probably yes 3. Probably No 4. No

Domain 2 Risk of bias due to deviations from the intended interventions (effect of assignment to intervention) (CONTINUED)	
2.6 Was an appropriate analysis used to estimate the effect of assignment to intervention?	1. Yes 2. Probably yes 3. Probably No 4. No
2.7 If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyse participants in the group to which they were randomized?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk-of-bias judgement	1. Low 2. High 3. Some concerns
Optional: What is the predicted direction of bias due to deviations from intended interventions?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable
Domain 3 Missing outcome data	
3.1 Were data for this outcome available for all, or nearly all, participants randomized?	1. Yes 2. Probably yes 3. Probably No 4. No
3.2 If N/PN/NI to 3.1: Is there evidence that result was not biased by missing outcome data?	1. Yes 2. Probably yes 3. Probably No 4. No
3.4 If Y/PY/NI to 3.3: Do the proportions of missing outcome data differ between intervention groups?	1. Yes 2. Probably yes 3. Probably No 4. No
3.5 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk-of-bias judgement	1. Low 2. High 3. Some concerns
Optional: What is the predicted direction of bias due to missing outcome data?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable

Domain 4 Risk of bias in measurement of the outcome	
4.1 Was the method of measuring the outcome inappropriate?	1. Yes 2. Probably yes 3. Probably No 4. No
4.2 Could measurement or ascertainment of the outcome have differed between intervention groups ?	1. Yes 2. Probably yes 3. Probably No 4. No
4.3 If N/PN/NI to 4.1 and 4.2: Were outcome assessors aware of the intervention received by study participants ?	1. Yes 2. Probably yes 3. Probably No 4. No
4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of intervention received?	1. Yes 2. Probably yes 3. Probably No 4. No
4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of intervention received?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk-of-bias judgement	1. Low 2. High 3. Some concerns
Optional: What is the predicted direction of bias in measurement of the outcome?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable
Domain 5 Risk of bias in selection of the reported result	
5.1 Was the trial analysed in accordance with a pre-specified plan that was finalized before unblinded outcome data were available for analysis ?	1. Yes 2. Probably yes 3. Probably No 4. No
Is the numerical result being assessed likely to have been selected, on the basis of the results, from...	
5.2. ...Multiple outcome measurements (e.g. scales, definitions, time points) within the outcome domain?	1. Yes 2. Probably yes 3. Probably No 4. No
5.3 ...Multiple analyses of the data?	1. Yes 2. Probably yes 3. Probably No 4. No

Domain 5 Risk of bias in selection of the reported result (Continued)	
Risk-of-bias judgement	1. Low 2. High 3. Some concerns
Optional: What is the predicted direction of bias due to selection of the reported result?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable
Overall risk of bias	
Risk-of-bias judgement	1. Low 2. High 3. Some concerns

7 Assessing quality in Non-random control trials (ROBINS-I tool)	
Bias due to confounding	
1.1 Is there potential for confounding of the effect of intervention in this study? **If N/PN to 1.1:** The study can be considered to be at low risk of bias due to confounding and no further signalling questions need be considered	1. Yes 2. Probably yes 3. Probably No 4. No
If Y/PY to 1.1: determine whether there is a need to assess time-varying confounding:	1. Yes 2. Probably yes 3. Probably No 4. No
1.2. Was the analysis based on splitting participants' follow up time according to intervention received? **If N/PN,** answer questions relating to baseline confounding (1.4 to 1.6) **If Y/PY,** go to question 1.3.	1. Yes 2. Probably yes 3. Probably No 4. No
1.3. Were intervention discontinuations or switches likely to be related to factors that are prognostic for the outcome? **If N/PN,** answer questions relating to baseline confounding (1.4 to 1.6) **If Y/PY,** answer questions relating to both baseline and time-varying confounding (1.7 and 1.8)	1. Yes 2. Probably yes 3. Probably No 4. No

Questions relating to baseline confounding only	
1.4. Did the authors use an appropriate analysis method that controlled for all the important confounding domains?	1. Yes 2. Probably yes 3. Probably No 4. No
1.5. If Y/PY to 1.4: Were confounding domains that were controlled for measured validly and reliably by the variables available in this study?	1. Yes 2. Probably yes 3. Probably No 4. No
1.6. Did the authors control for any post-intervention variables that could have been affected by the intervention?	1. Yes 2. Probably yes 3. Probably No 4. No
Questions relating to baseline and time-varying confounding	
1.7. Did the authors use an appropriate analysis method that controlled for all the important confounding domains and for time-varying confounding?	1. Yes 2. Probably yes 3. Probably No 4. No
1.8. If Y/PY to 1.7: Were confounding domains that were controlled for measured validly and reliably by the variables available in this study?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk-of-bias judgement	1. Low 2. Moderate 3. Serious 4. Critical
Optional: What is the predicted direction of bias due to confounding?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable
Bias in selection of participants into the study	
2.1. Was selection of participants into the study (or into the analysis) based on participant characteristics observed after the start of intervention? If N/PN to 2.1: go to 2.4	1. Yes 2. Probably yes 3. Probably No 4. No
2.2. If Y/PY to 2.1: Were the post-intervention variables that influenced selection likely to be associated with intervention?	1. Yes 2. Probably yes 3. Probably No 4. No
2.3 If Y/PY to 2.2: Were the post-intervention variables that influenced selection likely to be influenced by the outcome or a cause of the outcome?	1. Yes 2. Probably yes 3. Probably No 4. No

Bias in selection of participants into the study	
2.4. Do start of follow-up and start of intervention coincide for most participants?	1. Yes 2. Probably yes 3. Probably No 4. No
2.5. If Y/PY to 2.2 and 2.3, or N/PN to 2.4: Were adjustment techniques used that are likely to correct for the presence of selection biases?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk-of-bias judgement	1. Low 2. High 3. Some concerns
Optional: What is the predicted direction of bias due to selection of participants into the study?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable

Bias in selection of participants into the study	
2.4. Do start of follow-up and start of intervention coincide for most participants?	1. Yes 2. Probably yes 3. Probably No 4. No
2.5. If Y/PY to 2.2 and 2.3, or N/PN to 2.4: Were adjustment techniques used that are likely to correct for the presence of selection biases?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk-of-bias judgement	1. Low 2. Moderate 3. Serious 4. Critical
Optional: What is the predicted direction of bias due to selection of participants into the study?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable

Bias in classification of interventions	
3.1 Were intervention groups clearly defined?	1. Yes 2. Probably yes 3. Probably No 1. No
3.2 Was the information used to define intervention groups recorded at the start of the intervention?	1. Yes 2. Probably yes 3. Probably No 4. No

Bias in classification of interventions (Continued)	
3.3 Could classification of intervention status have been affected by knowledge of the outcome or risk of the outcome?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk-of-bias judgement	1. Low 2. Moderate 3. Serious 4. Critical
Optional: What is the predicted direction of bias due to classification of interventions?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable
Bias due to deviations from intended interventions	
If your aim for this study is to assess the effect of assignment to intervention, answer questions 4.1 and 4.2	
4.1. Were there deviations from the intended intervention beyond what would be expected in usual practice?	1. Yes 2. Probably yes 3. Probably No 4. No
4.2. If Y/PY to 4.1: Were these deviations from intended intervention unbalanced between groups and likely to have affected the outcome?	1. Yes 2. Probably yes 3. Probably No 4. No
If your aim for this study is to assess the effect of starting and adhering to intervention, answer questions 4.3 to 4.6	
4.3. Were important co-interventions balanced across intervention groups?	1. Yes 2. Probably yes 3. Probably No 4. No
4.4. Was the intervention implemented successfully for most participants?	1. Yes 2. Probably yes 3. Probably No 4. No
4.5. Did study participants adhere to the assigned intervention regimen?	1. Yes 2. Probably yes 3. Probably No 4. No
4.6. If N/PN to 4.3, 4.4 or 4.5: Was an appropriate analysis used to estimate the effect of starting and adhering to the intervention?	1. Yes 2. Probably yes 3. Probably No 4. No

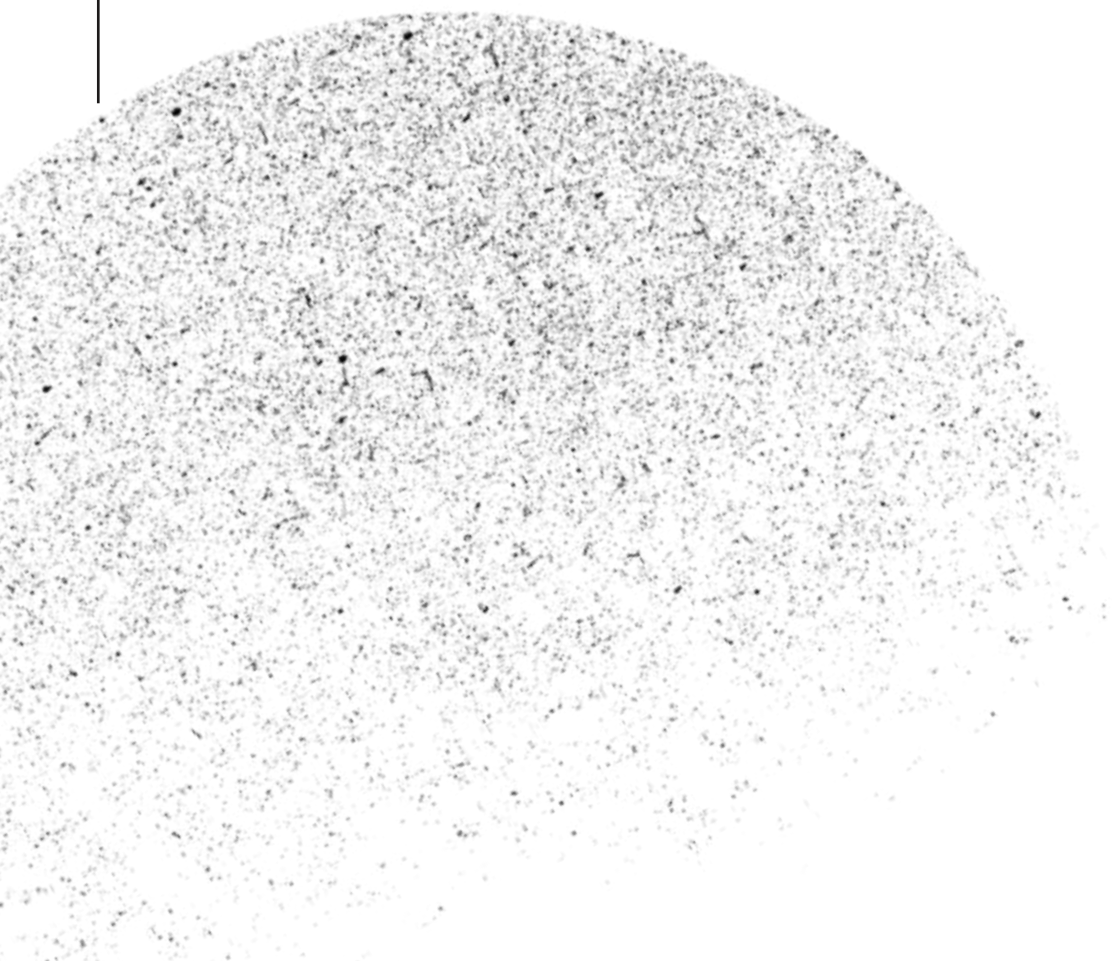
Bias due to deviations from intended interventions	
Risk-of-bias judgement	1. Low 2. Moderate 3. Serious 4. Critical
Optional: What is the predicted direction of bias due to deviations from the intended interventions?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable

Bias due to deviations from intended interventions	
5.1 Were outcome data available for all, or nearly all, participants?	1. Yes 2. Probably yes 3. Probably No 4. No
5.2 Were participants excluded due to missing data on intervention status?	1. Yes 2. Probably yes 3. Probably No 4. No
5.3 Were participants excluded due to missing data on other variables needed for the analysis?	1. Yes 2. Probably yes 3. Probably No 4. No
5.4 If PN/N to 5.1, or Y/PY to 5.2 or 5.3: Are the proportion of participants and reasons for missing data similar across interventions?	1. Yes 2. Probably yes 3. Probably No 4. No
5.5 If PN/N to 5.1, or Y/PY to 5.2 or 5.3: Is there evidence that results were robust to the presence of missing data?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk of bias judgement	1. Low 2. Moderate 3. Serious 4. Critical
5.5 If PN/N to 5.1, or Y/PY to 5.2 or 5.3: Is there evidence that results were robust to the presence of missing data?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable

Bias in measurement of outcomes	
6.1 Could the outcome measure have been influenced by knowledge of the intervention received?	1. Yes 2. Probably yes 3. Probably No 4. No
6.2 Were outcome assessors aware of the intervention received by study participants?	1. Yes 2. Probably yes 3. Probably No 4. No
6.3 Were the methods of outcome assessment comparable across intervention groups?	1. Yes 2. Probably yes 3. Probably No 4. No
6.4 Were any systematic errors in measurement of the outcome related to intervention received?	1. Yes 2. Probably yes 3. Probably No 4. No
Risk of bias judgement	1. Low 2. Moderate 3. Serious 4. Critical
Optional: What is the predicted direction of bias due to measurement of outcomes?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable
Bias in selection of the reported result	
Is the reported effect estimate likely to be selected, on the basis of the results, from...	
7.1. ...Multiple outcome measurements within the outcome domain?	1. Yes 2. Probably yes 3. Probably No 4. No
7.2 ...Multiple analyses of the intervention-outcome relationship?	1. Yes 2. Probably yes 3. Probably No 4. No
7.3 ...Different subgroups?	1. Yes 2. Probably yes 3. Probably No 4. No

Bias in selection of the reported result (Continued)	
Risk of bias judgement	1. Low 2. Moderate 3. Serious 4. Critical
Optional: What is the predicted direction of bias due to selection of the reported result?	1. Favours experimental 2. Favours comparator 3. Towards null 4. Away from null 5. Unpredictable

Bias in selection of the reported result (Continued)	
Risk of bias judgement	1. Low 2. Moderate 3. Serious 4. Critical



8 Assessing quality in Qualitative studies (White and Keenan tool)	
Are the evaluation questions clearly stated?	1. Yes 2. No
Is the qualitative methodology described?	1. Yes 2. No
Is the qualitative methodology appropriate to address the evaluation questions?	1. Yes 2. No 3. Insufficient detail
Is the recruitment or sampling strategy described?	1. Yes 2. No
Is the recruitment or sampling strategy appropriate to address the evaluation questions?	1. Yes 2. No 3. Insufficient detail
Are the researcher's own position, assumptions and possible biases outlined?	1. Yes 2. No
Have ethical considerations been sufficiently considered?	1. Yes 2. No 3. Insufficient detail
Is the data analysis approach adequately described?	1. Yes 2. No
Is the data analysis sufficiently rigorous?	1. Yes 2. No
Is there a clear statement of findings?	1. Yes 2. No
Are the research findings useful?	1. Yes 2. No

	Group descriptors				
Study dentifier	Intervention name	Control group name	Subgroup name	Data provider	How?

Outcome data - choose only one type per outcome					
Binary 2x2 table			Continuous data		
Treated group			Control group		
Sample size	Number of events	Prop w/ event	Sample size	Number of events	Prop w/ event

Outcome descriptors						Analysis info	
Outcome name	Outcome definition	Outcome in map	Time point measured	Upper/ lower scales	Scale range	Type of analysis	Data adjusted?

Continuous data		Log odds ratio		Relative risk (RR)		
Mean difference		Log odds ratio		Relative risk (RR)		
Mean	Variance of Mean	Log OR	Variance of Log OR	RR	Log RR	Variance of Log RR

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