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Caseworker-assigned discharge plans to prevent hospital readmission for acute exacerbations in children with chronic respiratory illness

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Published in:
Cochrane Database of Systematic Reviews

DOI:
[10.1002/14651858.CD012315.pub2](https://doi.org/10.1002/14651858.CD012315.pub2)

Published: 02/11/2018

Document Version
Publisher's PDF, also known as Version of record

[Link to publication](#)

Citation for published version (APA):

Hall, K. K., Petsky, H. L., Chang, A. B., & O'grady, K. F. (2018). Caseworker-assigned discharge plans to prevent hospital readmission for acute exacerbations in children with chronic respiratory illness. *Cochrane Database of Systematic Reviews*, 2018(11), 1-40. [CD012315].
<https://doi.org/10.1002/14651858.CD012315.pub2>

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Cochrane Database of Systematic Reviews 2018, Issue 11. Art. No.: CD012315.

DOI: 10.1002/14651858.CD012315.pub2.

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[Intervention Review]

Caseworker-assigned discharge plans to prevent hospital readmission for acute exacerbations in children with chronic respiratory illness

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Editorial group: Cochrane Airways Group.

Publication status and date: New, published in Issue 11, 2018.

Citation: Hall KK, Petsky HL, Chang AB, O'Grady KF. Caseworker-assigned discharge plans to prevent hospital readmission for acute exacerbations in children with chronic respiratory illness. *Cochrane Database of Systematic Reviews* 2018, Issue 11. Art. No.: CD012315. DOI: 10.1002/14651858.CD012315.pub2.

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ABSTRACT

Background

Chronic respiratory conditions are major causes of mortality and morbidity. Children with chronic health conditions have increased morbidity associated with their physical, emotional, and general well-being. Acute respiratory exacerbations (AREs) are common in children with chronic respiratory disease, often requiring admission to hospital. Reducing the frequency of AREs and recurrent hospitalisations is therefore an important goal in the individual and public health management of chronic respiratory illnesses in children. Discharge planning is used to decide what a person needs for transition from one level of care to another and is usually considered in the context of discharge from hospital to the home. Discharge planning from hospital for ongoing management of an illness has historically been referral to a general practitioner or allied health professional or self management by the individual and their family with limited communication between the hospital and patient once discharged. Effective discharge planning can decrease the risk of recurrent AREs requiring medical care. An individual caseworker-assigned discharge plan may further decrease exacerbations.

Objectives

To evaluate the efficacy of individual caseworker-assigned discharge plans, as compared to non-caseworker-assigned plans, in preventing hospitalisation for AREs in children with chronic lung diseases such as asthma and bronchiectasis.

Search methods

We searched the Cochrane Airways Group Specialised Register of Trials, the Cochrane Central Register of Controlled Trials (CENTRAL), MEDLINE, Embase, trials registries, and reference lists of articles. The latest searches were undertaken in November 2017.

Selection criteria

All randomised controlled trials comparing individual caseworker-assigned discharge planning compared to traditional discharge-planning approaches (including self management), and their effectiveness in reducing the subsequent need for emergency care for AREs (hospital admissions, emergency department visits, and/or unscheduled general practitioner visits) in children hospitalised with an acute exacerbation of chronic respiratory disease. We excluded studies that included children with cystic fibrosis.

Caseworker-assigned discharge plans to prevent hospital readmission for acute exacerbations in children with chronic respiratory illness (Review)

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Data collection and analysis

We used standard Cochrane Review methodological approaches. Relevant studies were independently selected in duplicate. Two review authors independently assessed trial quality and extracted data. We contacted the authors of one study for further information.

Main results

We included four studies involving a total of 773 randomised participants aged between 14 months and 16 years. All four studies involved children with asthma, with the case-planning undertaken by a trained nurse educator. However, the discharge planning/education differed among the studies. We could include data from only two studies (361 children) in the meta-analysis. Two further studies enrolled children in both inpatient and outpatient settings, and one of these studies also included children with acute wheezing illness (no previous asthma diagnosis); the data specific to this review could not be obtained. For the primary outcome of exacerbations requiring hospitalisation, those in the intervention group were significantly less likely to be rehospitalised (odds ratio (OR) 0.29, 95% confidence interval (CI) 0.16 to 0.50) compared to controls. This equates to 189 (95% CI 124 to 236) fewer admissions per 1000 children. No adverse events were reported in any study. In the context of substantial statistical heterogeneity between the two studies, there were no statistically significant effects on emergency department (OR 0.37, 95% CI 0.04 to 3.05) or general practitioner (OR 0.87, 95% CI 0.22 to 3.44) presentations. There were no data on cost-effectiveness, length of stay of subsequent hospitalisations, or adherence to medications. One study reported quality of life, with no significant differences observed between the intervention and control groups.

We considered three of the studies to have an unclear risk of bias, primarily due to inadequate description of the blinding of participants and investigators. The fourth study was assessed as at high risk of bias as a single unblinded investigator was used. Using the GRADE system, we assessed the quality of the evidence as moderate for the outcome of hospitalisation and low for the outcomes of emergency department visits and general practitioner consultations.

Authors' conclusions

Current evidence suggests that individual caseworker-assigned discharge plans, as compared to non-caseworker-assigned plans, may be beneficial in preventing hospital readmissions for acute exacerbations in children with asthma. There was no clear indication that the intervention reduces emergency department and general practitioner attendances for asthma, and there is an absence of data for children with other chronic respiratory conditions. Given the potential benefit and cost savings to the healthcare sector and families if hospitalisations and outpatient attendances can be reduced, there is a need for further randomised controlled trials encompassing different chronic respiratory illnesses, ethnicity, socio-economic settings, and cost-effectiveness, as well as defining the essential components of a complex intervention.

PLAIN LANGUAGE SUMMARY

Can individual caseworker-assigned discharge plans reduce readmissions for acute exacerbations in children with chronic respiratory disease?

Background

Acute exacerbations (flare-ups) of long-term breathing diseases in children leads to high use of health resources and poor quality of life for children and their families. Providing extra support and education to a child and his/her family during a hospitalisation for an acute flare-up may improve their quality of life and reduce future healthcare visits. A caseworker assigned to each child during a hospital admission may help provide individual education and discharge planning and ongoing support once the child has been discharged. We reviewed whether individualised case management support services during and following hospitalisation for acute flare-ups of long-term breathing diseases in children is beneficial. Specifically, we wanted to know if caseworkers can help prevent further hospital admissions and reduce the number of visits to other health services such as emergency departments and general practitioners.

Review question

Do individualised, caseworker-assigned discharge management plans prevent hospital readmissions for acute flare-ups in children with long-term breathing diseases?

Study characteristics

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We included all randomised controlled trials (a type of study in which participants are assigned to a treatment group using a random method) that assessed whether those who received individualised caseworker discharge planned management (the intervention group) had better outcomes compared to those who received usual care (the control group). We considered the number of hospital readmissions, emergency department visits, and/or unscheduled general practitioner visits following discharge.

The evidence is current to 15 November 2017.

We found four studies that included 773 children aged 14 months to 16 years. All the studies involved children with asthma. The programme used for the discharge plan differed among the studies, but all were delivered by a trained asthma educator (lay health worker or nurse specifically trained on educating patients with asthma). The studies followed the children for 2 to 14 months after discharge. We could only include data from two studies in a combined analysis (i.e. the meta-analysis), as the other two studies also enrolled children who were not hospitalised, and we could not obtain data specific to the children who were hospitalised and one of those studies included children with acute wheezing illness(no previous asthma diagnosis); the data specific to this review could not be obtained.

Key results

In this review involving children hospitalised with asthma flare-ups, trained asthma educator-led and structured discharge plans that included follow-up support (compared to the control group) reduced the number of hospital readmissions for acute asthma. No clear benefit was seen on future emergency department or general practitioner visits for acute asthma. Data on cost-effectiveness, length of stay of future hospitalisations, and adherence to discharge medications were not available. One study reported quality of life and found no differences between the intervention and control group. There were no studies relating to other long-term breathing diseases.

Conclusions

Individual caseworker-assigned discharge planned management, as compared to non-caseworker-assigned management, may prevent readmissions to hospital for asthma flare-ups in children. However, the current evidence is limited to only two studies in children with asthma. Further studies are needed in a broad range of long-term breathing diseases in childhood.

Quality of the evidence

We considered the quality of the evidence to be moderate for the outcome of hospital readmissions and low for the outcomes of future emergency department visits and general practitioner consultations for asthma flare-ups.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON *[Explanation]*

Caseworker-assigned discharge plans compared with non-caseworker-assigned plans in preventing rehospitalisation for acute respiratory exacerbations in children with asthma

Patient or population: children aged 2 to 16 years with asthma

Settings: tertiary paediatric hospitals

Intervention: individual caseworker-assigned discharge plans

Comparison: non-caseworker assigned plans

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No. of participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	Non-caseworker-assigned plans	Individual caseworker-assigned discharge plans				
Hospitalisation follow-up: range 6 to 60.7 weeks	303 per 1000 children	114 (71 to 170) per 1000 children	OR 0.29 (0.16 to 0.50)	361 (2 studies)	⊕⊕⊕ Moderate ¹	Lack of information on blinding in both studies and no allocation concealment in 1 study
Emergency department visit follow-up: range 6 to 60.7 weeks	205 per 1000 children	74 (40 to 123) per 1000 children	OR 0.37 (0.04 to 3.05)	361 (2 studies)	⊕⊕ Low ²	Lack of information on blinding in both studies and no allocation concealment in 1 study. Provision of oral steroids to children in the intervention group was not accounted for in the analysis in 1 study. Substantial statistical heterogeneity was present

General practitioner visit follow-up: 3 to 6 weeks	162 per 1000 children	134 (87 to 194) per 1000 children	OR 0.87 (0.22 to 3.44)	351 (2 studies)	⊕⊕ Low ³	Lack of information on blinding in both studies and no allocation concealment in 1 study. Substantial statistical heterogeneity was present
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*We were unable to meta-analyse results for **cost-effectiveness** and **quality of life** as each outcome was reported by only 1 included study.*
*None of the included studies reported on **adverse events**, **duration of stay at subsequent hospitalisations**, **adherence to discharge medications**, or **mortality**.*

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).

CI: confidence interval; OR: odds ratio

GRADE Working Group grades of evidence

High quality: Further research is very unlikely to change our confidence in the estimate of effect.

Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.

Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.

Very low quality: We are very uncertain about the estimate.

¹Allocation was unclear in two studies ([Madge 1997](#); [Wesseldine 1999](#)). Also, risk of bias for this outcome was high in one study and unclear for the other study.

²We downgraded two points due to considerable heterogeneity between the two studies and lack of blinding ([Madge 1997](#); [Wesseldine 1999](#)).

³We downgraded two points due to considerable heterogeneity between the two studies and lack of blinding ([Madge 1997](#); [Wesseldine 1999](#)), and because the follow-up period was short in comparison to other outcomes.)

BACKGROUND

Description of the condition

Chronic respiratory conditions are major causes of mortality and morbidity in low-, middle-, and high-income countries. Children with chronic health conditions have increased morbidity associated with their physical, emotional, and general well-being. Their illness and these stressors can have a great impact on parents and caregivers and their ability to cope with ongoing care, especially when recurrent hospitalisations are required (Peterson-Carmichael 2012; Plant 2013). Disability from chronic diseases is increasing as the burden from acute infectious diseases is being controlled through treatment and prevention (Ait-Khaled 2001; Asher 2014; Beran 2015; Chang 2013). Chronic respiratory diseases that are important in childhood include recurrent protracted bacterial bronchitis, asthma, chronic suppurative lung disease, and bronchiectasis (Chang 2014). These diseases present challenges to public health in both low- middle- and high income countries alike due to their high prevalence, the burden they place on the individual, and the high economic cost to health systems globally (Abramson 2015; Asher 2014; Chang 2013; Kapur 2010; Zar 2014). Furthermore, repeated exacerbations of chronic lung diseases in childhood (e.g. bronchiectasis) are known precursors to further lung function decline (Kapur 2010).

Acute respiratory exacerbations (AREs) are common in children with chronic respiratory disease, often requiring admission to hospital (Redding 2014). A retrospective study in Brisbane, Australia, of children with non-cystic fibrosis bronchiectasis reported that 35% of AREs required hospitalisation (Kapur 2009). A multicentre randomised controlled trial (RCT) of Indigenous children in Australia and Alaska found that 15% of children with chronic respiratory disease were hospitalised for an ARE during the study period, and that 52% of children at enrolment to the RCT had been hospitalised for an ARE in the previous 12 months before enrolment (Redding 2014). A retrospective, cross-sectional study from the USA reported hospital admission rates of 18% in children with asthma exacerbations (Sanders 2007). Reducing the frequency of AREs and recurrent hospitalisations is therefore an important goal in the individual and public health management of chronic respiratory illnesses in children. Discharge planning is a process used to decide what a person needs for transition from one level of care to another and is usually considered in the context of discharge from hospital to the home. Discharge planning from hospital for ongoing management of an illness has historically been referral to a general practitioner (GP) or allied health professional or self management by the individual and their family with limited communication between the hospital and patient once discharged. Effective discharge planning can decrease the risk of recurrent acute exacerbations of chronic disease with or without readmission to hospital (Lorig 2003).

In this review we focused on children hospitalised with AREs with or without deterioration of the following chronic respiratory conditions: asthma, recurrent protracted bacterial bronchitis, chronic suppurative lung disease, and bronchiectasis. The aim of the review was to determine the effectiveness of individual, caseworker-assigned discharge planning and follow-up, during and after hospitalisation, in reducing subsequent hospital admissions in children with chronic respiratory diseases as compared to traditional discharge-planning approaches (including self management).

Description of the intervention

Traditional hospital discharge plans tend to focus on disease-specific information and skills patients need to manage the disease, for example blood glucose testing in diabetes. The information may be written, verbal, or both and is usually delivered to the patient by a ward nurse. A discharge summary of the hospital stay is usually sent to the patient's GP and can include diagnostic findings and postdischarge follow-up requirements (Kripalani 2007). Discharge plans may vary in health facilities and across cultures (Holland 2007). Traditional discharge plans are often based on the medical management of the condition and may not consider other aspects of living that are impacted by chronic illness such as the emotional well-being of the individual, their support network, and the general tasks of daily living (Lorig 2003).

Self management plans are formalised care plans developed while the person is in hospital that aim to teach the person and their family/career the required skills to manage and control their health condition. Self management plans teach strategies on how to control disease, promote health, and live with a chronic health condition. This generally includes monitoring and managing symptoms and signs of illness, managing the impacts of illness, and adhering to treatment regimens that allow the person to identify changes in their condition and to implement appropriate changes as their condition or disease changes (Auduly 2013; Lorig 2003; Regan-Smith 2006). Self management discharge plans do not generally incorporate follow-up post-hospital discharge by the discharge planner to determine plan outcomes. Self management can be effective if the patient and his/her family have the motivation and confidence to self manage the patient's illness. Self management plans can fail if considerations have not been given to the patient's perception of healthcare practices and cultural considerations. For self management plans to be effective, the individual, career, and primary healthcare provider need to continually engage and be proactive in the healthcare plan, which can be difficult for parents of chronically ill children (Lorig 2003).

Individual caseworker-assigned plans involve the patient being assigned a specific caseworker on admission, or shortly after, and aim to address the gaps in the simple or self management discharge plans described above. The caseworker's role is to liaise with hospital staff during the person's stay in hospital and develop a plan for discharge that provides individual, ongoing support to patients

and their families to improve health outcomes. This may be done by support and motivation to attend appointments and behaviour risk modification through education programmes (Plant 2013). A Cochrane Review that included generic discharge planning from hospital found that tailored discharge planning led to a “small reduction in hospital length of stay and reduced the risk of readmission to hospital at three months follow-up for older people with a medical condition” but did not reduce costs to the health service (Gonçalves-Bradley 2016). This review did not specifically examine individualised caseworker-assigned plans that incorporate one-on-one care postdischarge until resolution of symptoms or care is no longer required (Gonçalves-Bradley 2016). Individualised caseworker plans are likely to be particularly important in settings where a person with low health literacy or from a different culture requires assistance to navigate the health system. A Cochrane Review on Indigenous health worker involvement (compared to routine care) for Indigenous people with asthma described improvements in knowledge scores but no significant difference in exacerbation rates (Chang 2010).

How the intervention might work

Caseworkers may reduce rehospitalisation of people with chronic conditions through a variety of ways. These can include:

1. improving communication between service providers and patients, hence leading to better self management plans;
2. understanding the types of available inpatient and outpatient hospital services so as to maximise outpatient care;
3. co-ordinating care from hospital to home and beyond; and
4. improved health education and promotion such as reducing tobacco smoking exposure, which will reduce acute exacerbations of chronic diseases (Jordan 2007; Plant 2013).

A holistic approach to chronic disease management may provide opportunities to identify early barriers to full recovery, and early symptom management could prevent deterioration in the person's condition that, if not addressed, would be likely to result in readmission to hospital.

Why it is important to do this review

The morbidity (including hospitalisations) related to chronic respiratory disease is an important issue for patients and health systems globally. Preventing possible triggers (e.g. infections, poor adherence to medications, etc.) that could cause exacerbations leading to hospitalisation have important individual and public health impacts (Ait-Khaled 2001; Chang 2013; Plant 2013). The appropriate management and follow-up of children postdischarge through individualised case management is one possible strategy that may reduce the overall burden of disease and long-term sequelae of chronic respiratory disease such as bronchiectasis. However, this strategy would likely add a cost to the health system.

A systematic review of individual caseworker-assigned discharge care plans (versus discharge planning without caseworker involvement) to determine the effectiveness of this strategy in reducing the hospitalisations and the burden of disease in children would inform clinical care and health policy.

OBJECTIVES

To evaluate the efficacy of individual caseworker-assigned discharge plans, as compared to non-caseworker-assigned plans, in preventing hospitalisation for AREs in children with chronic lung diseases such as asthma and bronchiectasis.

METHODS

Criteria for considering studies for this review

Types of studies

We included RCTs comparing individual caseworker-assigned discharge plans, as compared to non-caseworker-assigned plans, in preventing hospitalisation for AREs in children with chronic lung diseases. We included studies reported as full text, those published as abstract only, and unpublished data. We did not include cross-over trials.

Types of participants

We included children aged less than 18 years admitted to hospital with an ARE or deterioration of an underlying chronic respiratory illness. We excluded children with a diagnosis of cystic fibrosis.

Types of interventions

We included RCTs comparing individual caseworker-assigned plans that involved the caseworker facilitating communication on admission to the hospital setting between the child, family, and attending physician as well as the caseworker involving the child and family in developing and implementing the plan of care during and following hospitalisation. Caseworker activities could include, but were not limited to:

1. facilitating the discharge plan and obtaining needed consultations from other allied health services as required;
2. collaborating with home health agencies; and
3. providing educational information and emotional support to the child and family.

We included studies comparing caseworker-assigned discharge plans versus discharge plans that do not involve caseworker support. We included studies with a follow-up period of at least eight weeks to determine study outcomes.

Types of outcome measures

Primary outcomes

1. Proportion of children requiring emergency department (ED) visit
2. hospitalisation or rate/frequency
3. subsequent ED visits and readmissions
4. Adverse events (all causes)

Secondary outcomes

1. Proportion of children or rate of unscheduled healthcare visits to a GP
2. Quality of life (measured on a validated scale) at months one and six postdischarge
3. Cost-effectiveness
4. Duration of stay at subsequent hospitalisations
5. Adherence to discharge medications
6. Mortality rate postdischarge for respiratory-related illness

Search methods for identification of studies

Electronic searches

We identified trials from the following databases:

1. Cochrane Airways Register of Trials, searched through the Cochrane Register of Studies (CRS), on 15 November 2017;
2. Cochrane Central Register of Controlled Trials (CENTRAL), searched through the Cochrane Register of Studies Online (crso.cochrane.org) on 15 November 2017;
3. MEDLINE Ovid SP (1946 to November 2017), searched on 17 November 2017;
4. Embase Ovid SP (1974 to November 2017), searched on 17 November 2017.

The full search strategies for each database are provided in [Appendix 1](#).

We also conducted a search of ClinicalTrials.gov (www.clinicaltrials.gov) and the World Health Organization International Clinical Trials Registry Platform (WHO ICTRP) (www.who.int/ictpr/en/) on 22 November 2017 using the search strategy in [Appendix 2](#). We searched all databases from their inception and we imposed no restriction on language of publication.

Searching other resources

We checked reference lists of all primary studies and reviewed articles for additional references. We attempted to contact the primary author of two studies to obtain group-specific data from their studies ([Karnick 2007](#); [Stevens 2002](#)), but we could not contact the authors of [Stevens 2002](#), and we were unable to obtain

additional data for [Karnick 2007](#), a paper that was published a decade ago. We searched for errata or retractions from included studies published in full text on PubMed (www.ncbi.nlm.nih.gov/pubmed), searched 13 June 2018.

Data collection and analysis

Selection of studies

Two review authors (KH and HP) independently screened titles and abstracts of all the studies identified by the search, coding them as 'retrieve' (eligible or potentially eligible/unclear) or 'do not retrieve'. We retrieved the full-text study reports/publications, and two review authors (KH and HP) independently screened the full text and identified studies for inclusion and recorded reasons for exclusion of the ineligible studies. We planned to resolve any disagreements through discussion or by consulting a third review author (KO) if required. We identified and excluded duplicates and collated multiple reports of the same study so that each study, rather than each report, was the unit of interest in the review. We recorded the selection process in sufficient detail to complete a PRISMA flow diagram and 'Characteristics of excluded studies' table ([Moher 2009](#)). We addressed these findings in the [Discussion](#) to provide an overview of the evidence to support/refute the intervention for reducing recurrent AREs in children with chronic respiratory diseases.

Data extraction and management

We used a data collection form for study characteristics and outcome data that had been piloted on at least one study in the review. Two review authors (KH and HP) extracted study characteristics from included studies as follows.

1. Methods: study design, total duration of study, details of any 'run-in' period, number of study centres and location, study setting, withdrawals, and date of study.
2. Participants: N, mean age, age range, gender, indigenous status, socio-economic status, diagnosis, severity of condition, diagnostic criteria, inclusion criteria, and exclusion criteria.
3. Interventions: intervention, comparison, concomitant medications, and excluded medications.
4. Outcomes: primary and secondary outcomes specified and collected, and time points reported.
5. Notes: funding for trial, and notable conflicts of interest of trial authors.

Two review authors (KH and HP) independently extracted outcome data from the included studies. These data were checked by Dr Cates, editor of the Cochrane Airways Group. We noted in the 'Characteristics of included studies' table if outcome data were not reported in a usable way. Any disagreements were resolved by consensus or by involving a third review author (KO). One

review author (KH) transferred data into the Review Manager 5 file ([RevMan 2014](#)). We double-checked that data were entered correctly by comparing the data presented in the systematic review with the study reports. A second review author (HP) spot-checked study characteristics for accuracy against the trial report.

Assessment of risk of bias in included studies

Two review authors (KH and HP) independently assessed risk of bias for each study using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* ([Higgins 2011](#)). Any disagreements were resolved by discussion or by involving another review author (AC). We assessed risk of bias according to the following domains.

1. Random sequence generation.
2. Allocation concealment.
3. Blinding of participants and personnel.
4. Blinding of outcome assessment.
5. Incomplete outcome data.
6. Selective outcome reporting.
7. Other bias.

We graded each potential source of bias as high, low, or unclear and provided a quote from the study report together with a justification for our judgement in the 'Risk of bias' table. We summarised the 'Risk of bias' judgements across different studies for each of the domains listed. We considered blinding separately for different key outcomes where necessary (e.g. for unblinded outcome assessment, risk of bias for all-cause mortality may be very different than for a patient-reported pain scale). Where information on risk of bias related to unpublished data or correspondence with a trialist, we noted this in the 'Risk of bias' table. When considering treatment effects, we took into account the risk of bias for the studies that contribute to that outcome.

Assessment of bias in conducting the systematic review

We conducted the review according to the published protocol and reported any deviations from it in the [Differences between protocol and review](#) of the systematic review.

Measures of treatment effect

We analysed dichotomous data as odds ratios (OR) in the meta-analyses. We undertook meta-analysis only where this was meaningful, that is if the treatments, participants, and the underlying clinical question were similar enough for pooling to make sense. Where multiple trial arms were reported in a single trial, we included only the relevant arms. If two comparisons (e.g. intervention A versus control and intervention B versus control) were combined in the same meta-analysis, we halved the control group to avoid double-counting. Had we encountered continuous outcomes, we would have analysed these data using mean differences

(MDs), or standardised mean differences (SMDs) if the scales differed. We planned to narratively describe skewed data reported as medians and interquartile ranges.

Unit of analysis issues

For dichotomous data, we reported the proportion of participants contributing to each outcome in comparison with the total number randomised. For rate ratios of common events whereby one participant may have more than one event, we planned to use generic inverse variance. We planned to take the rate ratios from the published papers and the standard error of the log rate ratios calculated from confidence intervals or P values published in the papers. However, we did not encounter rates or frequencies of events per child time of observation, hence we could only analyse data by the proportions of children in the intervention and control groups who had a least one outcome event in each study and in the meta-analysis. One study reported the proportions who had more than one episode, and we have presented these descriptively ([Wesseldine 1999](#)).

Dealing with missing data

We contacted investigators or study sponsors in order to verify key study characteristics and to obtain missing numerical outcome data where possible (e.g. when a study was identified as abstract only). Where this was not possible, and the missing data were thought to introduce serious bias, we explored the impact of including such studies in the overall assessment of results by a sensitivity analysis. Where we could not obtain further data from the authors, we described the study's results qualitatively.

Assessment of heterogeneity

We described and tested any heterogeneity between study results to determine if it reached statistical significance using a Chi² test. We considered heterogeneity as significant when the P value was less than 0.10 ([Higgins 2011](#)). We used the I² statistic to measure heterogeneity among the studies in the meta-analysis. We planned that if we identified substantial heterogeneity, we would report and explore possible causes for it by prespecified subgroup analysis, when possible.

Assessment of reporting biases

As we could include data from only two studies in the meta-analyses, we did not create and examine a funnel plot to explore possible small-study and publication biases. We planned to undertake this if there were data from 10 or more studies.

Data synthesis

We included the results from studies that met the inclusion criteria and reported any outcomes of interest in the subsequent

meta-analyses. We calculated the summary OR and 95% confidence interval (CI) (fixed-effect model) (RevMan 2014). We used a random-effects model when there were concerns about statistical heterogeneity. We presented the findings of our primary outcomes and other important outcomes in a 'Summary of findings' table according to recommendations in the *Cochrane Handbook for Systematic Reviews of Interventions*, which we generated using GRADEpro GDT software [Gradepro GDT Software](#).

'Summary of findings' table

As we were unable to analyse rates and frequency of episodes of hospitalisations and ED presentations with the available data, we created a 'Summary of findings' table using the following outcomes.

1. Proportion of children requiring hospitalisation during the follow-up period
2. Proportion of children requiring ED visits during the follow-up period
3. Proportion of children presenting to a GP for acute asthma in the three to six weeks following randomisation

We used the five GRADE considerations (study limitations, consistency of effect, imprecision, indirectness, and publication bias) to assess the quality of a body of evidence as it related to the studies that contributed data to the meta-analyses for the prespecified outcomes. We used methods and recommendations described in Section 8.5 and Chapter 12 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011), employing GRADEpro GDT software [Gradepro GDT Software](#). We justified all decisions to down- or upgrade the quality of studies using footnotes and made comments to aid the reader's understanding of the review where necessary.

Subgroup analysis and investigation of heterogeneity

We planned the following subgroup analyses.

1. Disease types: chronic suppurative lung disease, bronchiectasis, asthma, others
2. Two or more comorbidities; these comorbidities could include but were not limited to the following: atopy, dysphagia, bronchopulmonary dysplasia, juvenile diabetes, congenital/neurological disorders, obstructive sleep apnoea

3. Age groups of children: preschool (aged < 6 years) versus older children (aged 6 or more years)
4. Control group (e.g. usual discharge letter, action plans)
5. Setting: low-income economies, low-middle-income economies, upper-middle-income economies, high-income economies, high-income Organisation for Economic Co-operation and Development members
6. Indigenous versus non-Indigenous
7. Hospital type, e.g. tertiary paediatric referral centre or local/district hospital

We planned to use the formal test for subgroup interactions in Review Manager 5 if data were available (RevMan 2014).

Sensitivity analysis

We planned to carry out the following sensitivity analyses.

1. Excluding studies with a high risk of bias based on the 'Risk of bias' assessment. Studies that did not have adequate allocation concealment and sequence generation were removed.
2. Unpublished data obtained from study authors or from conference abstracts.
3. Analysis that used a fixed-effect model as opposed to a random-effects model.

RESULTS

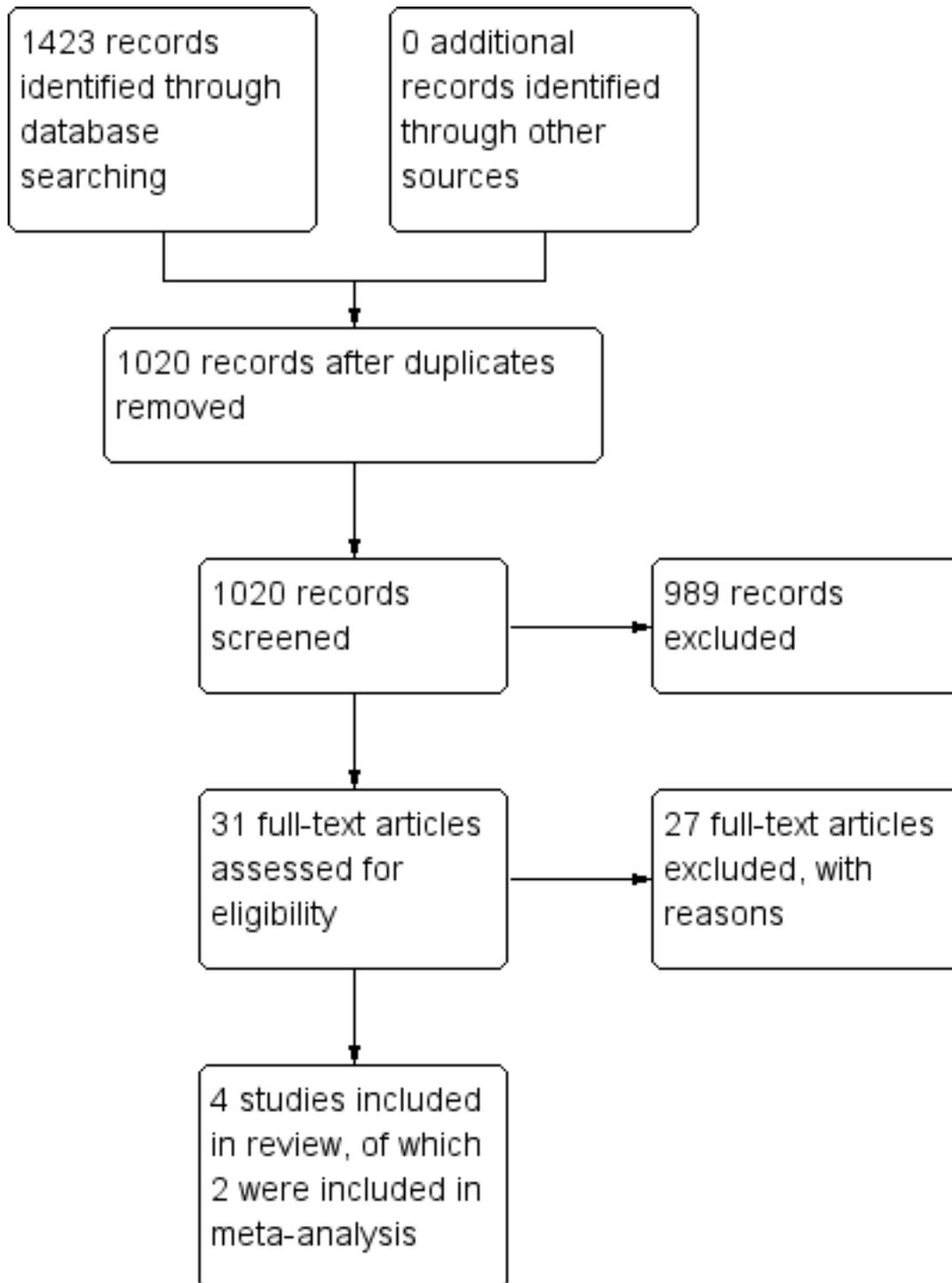
Description of studies

See [Characteristics of included studies](#); [Characteristics of excluded studies](#).

Results of the search

The database searches identified 1423 potentially relevant titles from the 2016 and 2017 searches (Figure 1). We did not identify any additional titles through searches of ClinicalTrials.gov (www.clinicaltrials.gov) or the WHO ICTRP (www.who.int/ictrp/en/). After assessing the abstracts, we obtained 31 papers for consideration after removal of duplicates. We excluded 27 papers due to the intervention being an educational programme or non-randomisation.

Figure 1. Study flow diagram.



Included studies

See [Characteristics of included studies](#) table.

We included four studies involving 773 randomised children (Karnick 2007; Madge 1997; Stevens 2002; Wesseldine 1999). The children in these studies were aged between 14 months and 16 years. All four studies involved children with asthma. Two studies enrolled children in both inpatient and outpatient settings (Karnick 2007; Stevens 2002), and as the data for only the hospitalised children (an inclusion criterion of this review) were not presented or available from the authors, data from those studies could not be included in the meta-analysis. In one study (Stevens 2002), children with acute wheezing were also enrolled, and data on only children with asthma were not available. We instead described the study outcomes for the two studies excluded from the meta-analysis in the [Effects of interventions](#) section. In the remaining two studies (Madge 1997; Wesseldine 1999), 361 children were enrolled and included in the meta-analysis.

Study design

Three studies were parallel, single-centre RCTs (Karnick 2007; Madge 1997; Wesseldine 1999). One study was conducted in two centres (Stevens 2002). Two studies enrolled children on hospital wards during admission for acute asthma (Madge 1997; Wesseldine 1999), and two studies enrolled children from inpatient settings, the ED, outpatients department, and through paediatric pulmonologist referrals (Karnick 2007; Stevens 2002). Follow-up periods ranged from 2 to 14 months postenrolment. For the two studies included in the meta-analysis (Madge 1997; Wesseldine 1999), different time periods were reported for GP presentations; both studies reported events in the three to six weeks following randomisation, and one also reported events in the six months following randomisation (Wesseldine 1999).

Control group

The control group in Madge 1997 was “usual care” as determined by the attending paediatrician, but the elements of “usual care” were not described. In Wesseldine 1999, the control group was described as children who received standard discharge care from ward staff. Standard discharge care was described as “variable” and dependent on factors such as availability and experience of parents and staff and time constraints. Children assigned to the control group in Stevens 2002 received usual care, which was described as a range of medical and nursing approaches. Karnick 2007 used historical clinical data reported by parents/caregivers at baseline as the comparator within each of the three groups in the study; there was no prospective concurrent control group (i.e. no inter-

vention undertaken), although it could be argued that the “reinforced asthma education” group could be considered the control group. In Karnick 2007, all the children were reviewed at the time of enrolment by a paediatric pulmonologist who amended the patients’ existing management plans to meet the national treatment guideline.

Caseworker-assigned discharge plan group

In Madge 1997, the intervention group received a structured asthma education and home management training programme delivered by a single person who was a trained specialist asthma nurse. This consisted of discussion sessions, written information and advice, an appointment with the nurse two to three weeks after discharge, and access to telephone advice from the nurse throughout the entire follow-up period. Families were also provided with a course of oral steroids and guidance on when to start them if needed.

In Stevens 2002, children assigned to the intervention group received a general education booklet about asthma, a written guided self management plan, and two 20-minute structured education sessions provided one-to-one by a specialist respiratory nurse. One session was provided one month after discharge.

In Wesseldine 1999, the caseworker-assigned discharge package consisted of an interview on the day of discharge with the study’s chief investigator, during which education and information were provided on asthma and the self management of asthma post-discharge. Participants were provided with an individual written home management plan, and parents were provided a short educational booklet entitled “At home with asthma” that included contact information for an asthma help-line and support groups. No additional caseworker support was provided following discharge from hospital.

There were three study groups in Karnick 2007. Group 1 received basic asthma education delivered by a lay asthma health educator in a single session lasting 20 to 30 minutes, and the other two groups received additional support beyond basic asthma education. Group 2 (“reinforced asthma education”) received basic asthma education information that was reinforced at each monthly data collection point throughout the study, and participants were encouraged to contact the asthma educator if they needed further assistance. Group 3 was the “case-management and reinforced education” group, which received the same education as the reinforced asthma education group and had a nurse practitioner/caseworker assigned to them. The caseworker completed an initial case evaluation, worked with the family to develop an asthma management action plan, and then supported the family in implementing the management plan during the follow-up period.

Participants

The inclusion criteria for the two studies where data could be included in the meta-analysis were children hospitalised with acute asthma (Madge 1997; Wesseldine 1999). Madge 1997 included children aged 2 to less than 14 years and who were identified and families contacted within 24 hours of admission. Wesseldine 1999 included children aged 2 to 16 years who became eligible on the day of discharge and were approached if the investigator was available. How and by whom the diagnosis of acute asthma was assigned was not reported. Stevens 2002 recruited children who were aged 14 months to 5 years at the time of admission with a primary diagnosis of acute severe asthma or wheezing. No exclusion criteria were reported. Karnick 2007 included children aged 1 to 16 years with asthma; children with other chronic conditions were excluded.

Outcomes

The primary outcome of all four studies was the proportion of children readmitted to hospital for acute asthma within the follow-up time frame (Karnick 2007; Madge 1997; Stevens 2002; Wesseldine 1999). Stevens 2002 also included as primary outcomes the proportion of children with ED attendances and primary care provider visits for asthma. With respect to secondary outcomes, three studies included the proportion of children with ED and GP attendances for asthma (Karnick 2007; Madge 1997; Wesseldine 1999). Wesseldine 1999 also included the proportion of children with primary care provider visits for any acute respiratory illness, days of school lost for any medical illness, and a parent-completed questionnaire capturing nocturnal symptoms, ac-

tivity restrictions, and frequency of specific infections at six weeks postdischarge as secondary outcomes. Madge 1997 also included asthma symptoms at four weeks postdischarge as a secondary outcome. The secondary outcomes for Stevens 2002 included Usherwood's index of symptoms and level of disability, Paediatric Asthma Caregiver's Quality of Life Questionnaire (PACQLQ), 4 weeks of symptom diaries, and a 42-item caregiver's knowledge of asthma questionnaire. Karnick 2007 included hospital days and cost-benefit as secondary outcomes.

Excluded studies

We excluded 27 studies, and have presented the reasons for exclusion in the Characteristics of excluded studies table. The most common reasons for exclusion were: participants were not admitted to hospital or were treated in ED then discharged (n = 7); the intervention did not include a caseworker (n = 8); the study was not a randomised controlled trial (n = 5); the study was community based or conducted in GP clinic (n = 4); and participants were adults (n = 3).

Risk of bias in included studies

Full details of 'Risk of bias' judgements are presented in the Characteristics of included studies table and summarised in Figure 2. Overall, the methodological quality of the studies was low given the lack of information on randomisation and blinding procedures. We did not consider it to be very low given that the major outcomes of hospitalisations, ED presentations, and GP visits were objective measures.

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

	Random sequence generation (selection bias)	Allocation concealment (selection bias)	Blinding of participants and personnel (performance bias)	Blinding of outcome assessment (detection bias)	Incomplete outcome data (attrition bias)	Selective reporting (reporting bias)	Other bias
Karnick 2007	?	?	?	?	+	?	?
Madge 1997	-	-	?	?	?	+	?
Stevens 2002	+	+	?	?	+	+	-
Wesseldine 1999	+	+	-	-	+	+	?

Allocation

Two studies reported allocation concealment and were thus judged as at low risk of bias (Stevens 2002; Wesseldine 1999). As generation of the randomisation sequence was not present in Madge 1997, we judged this study as at high risk of selection bias. Karnick 2007 did not describe the randomisation process and allocation concealment, hence we judged the risk of bias to be unclear.

Blinding

We considered risk of detection and performance bias due to inadequate blinding of outcome assessors as high in one study given that outcomes were collected by a single investigator (Wesseldine 1999), and unclear in three studies as blinding was not described (Karnick 2007; Madge 1997; Stevens 2002). However, Stevens 2002 did report blinding of the assessors who scrutinised parent-completed diary cards (secondary outcome). Karnick 2007, Stevens 2002, and Wesseldine 1999 did not describe blinding of participants. In the third study (Madge 1997), the authors only reported that “parents within the control group were not aware that other children were receiving an intervention nor that subsequent admissions were being tracked”, thus we judged the risk of performance bias as unclear.

Incomplete outcome data

We deemed three studies to be at low risk of attrition bias as they had acceptable attrition rates (i.e. less than 25%) (Karnick 2007; Stevens 2002; Wesseldine 1999). Madge 1997 did not report attrition, however as all study follow-up was ceased 2 months after the last child was randomised, the study had variable periods of follow-up completed per participant (range 2 to 14 months), thus the completeness of outcome data was unclear.

Selective reporting

We considered three studies that reported all outcomes as having a low risk of reporting bias (Madge 1997; Stevens 2002; Wesseldine 1999). We judged one study as having an unclear risk of reporting bias (Karnick 2007).

Other potential sources of bias

Interpretation of the findings by Karnick and colleagues is complicated by the lack of a prospective control group, the use of within-group historical data as the unit of comparison, and the review of all children by a paediatric pulmonologist at baseline (Karnick 2007). Given that the study used historical data as the comparator, delineating the effects of the pulmonologist review from the effects of the intervention on study outcomes is problematic, and

hence the risk of other bias is unclear. In Stevens 2002, children with acute wheezing illnesses were included, and only 69% of all enrolled children had been given a diagnosis of asthma. Hence it is likely that not all children had a chronic illness, therefore we deemed this study as at high risk of other bias. Furthermore, we were unable to analyse the Karnick and Stevens data for hospitalised children only, and it is unclear what proportions of children in the intervention and control groups were enrolled from inpatient and outpatient settings. For the two studies included in the meta-analysis (Madge 1997; Wesseldine 1999), the periods of follow-up for children enrolled varied within and between studies, hence we could not determine whether the rates of admissions and ED presentations differed between study groups, and these studies were deemed as at unclear risk of other bias. Moreover, in the Madge 1997 study, children in the intervention group were discharged home with a course of steroids and instructions for parents on when to use them if needed. As steroid use postdischarge in both the intervention and control groups was not reported in the study, it is possible there was a considerable differential bias that could have overestimated the effect of the intervention in this study, therefore we deemed the risk of other bias in this study to be unclear.

Effects of interventions

See: [Summary of findings for the main comparison Caseworker-assigned discharge plans compared with non-caseworker-assigned plans in preventing rehospitalisation for acute respiratory exacerbations in children with asthma](#)

See [Summary of findings for the main comparison](#) for the main comparison 'Individual caseworker-assigned discharge plans compared to non-caseworker assigned plans'.

Primary outcomes

All studies reported on the proportion of children requiring hospitalisations and ED presentations for acute asthma as their primary outcome; we included data from two studies in the meta-analysis (Madge 1997; Wesseldine 1999), although follow-up times differed between the 2 studies (2 to 14 months for Madge 1997 and 6 months for Wesseldine 1999). Data for only those children hospitalised with asthma at time of enrolment were not available for Karnick 2007 and Stevens 2002. No studies reported adverse events. No studies reported rates or frequencies of subsequent hospitalisations, and hence only proportions of children could be analysed.

Exacerbation requiring hospitalisation

[Analysis 1.1](#)

In the meta-analysis of two studies (Madge 1997; Wesseldine 1999), the proportion of children who were rehospitalised for acute asthma was significantly lower ($P < 0.001$) in the intervention group compared to controls (odds ratio (OR) 0.29, 95% confidence interval (CI) 0.16 to 0.50; participants = 361; studies = 2; $I^2 = 0\%$). Based on these figures, the number needed to treat for an additional beneficial outcome over the study period (6 to 14 months) was 6 (95% CI 4 to 9).

Wesseldine 1999 also reported that the overall number of readmissions over a six-month period was 18/80 children in the intervention group and 69/80 children in the control group, equating to approximately 22.5 admissions per 100 children and 87 admissions per 100 children, respectively. The study authors reported that the excess number in the control group was due to multiple admissions for several children.

In Karnick 2007, which included children enrolled in outpatient settings, there was a significant reduction in hospitalisations for asthma in each of the three study groups (asthma education only group = 76% reduction; reinforced education group = 81% reduction; and case management and reinforced education group = 86% reduction; $P < 0.001$ in each group), however there were no statistically significant differences between the three groups (P value = 0.77).

In Stevens 2002, the proportions of children requiring inpatient admissions in the 12 months following the intervention were not statistically different between the intervention and control groups (P value = 0.29).

Exacerbation requiring emergency department presentation

Analysis 1.2

We included two studies in the meta-analysis of ED presentations for acute asthma (Madge 1997; Wesseldine 1999). The odds of ED presentations for acute asthma in a random-effects model were lower in the intervention group compared to the control group (OR 0.37, 95% CI 0.04 to 3.05; participants = 361; studies = 2; $I^2 = 88\%$), however this difference was not statistically significant. When analysed using a fixed-effect model, the difference between groups changed substantially (OR 0.30, 95% CI 0.16 to 0.58; $P < 0.001$), and the I^2 for heterogeneity remained unchanged (88%). Subgroup analyses to explore the potential reasons for this heterogeneity were not possible with the available data, however it may be due to the different periods of follow-up used in the studies.

In Karnick 2007, which included children enrolled in outpatient settings, there was a significant reduction in ED visits for asthma in each of the three study groups compared to their previous baselines (asthma education only group = 55% reduction, P value < 0.05 ; reinforced education group = 66% reduction, P value < 0.005 ; and case management and reinforced education group = 86% reduction, $P < 0.001$). Although the largest reduction was found in the case management and reinforced education group, there was

no statistically significant difference between the three groups (P value 0.14).

In Stevens 2002, the proportions of children presenting to EDs and outpatient clinics in the 12 months following the intervention were not statistically different (P value 0.88).

Adverse events

No studies reported adverse events.

Secondary outcomes

Unscheduled healthcare visits to a GP for asthma

Analysis 1.3

We included two studies in the analysis of GP visits for asthma in the three to six weeks following randomisation (Madge 1997; Wesseldine 1999). The odds of GP presentations in a random-effects model did not differ significantly between groups, and substantial heterogeneity was present (OR 0.87, 95% CI 0.22 to 3.44; participants = 351; studies = 2; $I^2 = 79\%$). The estimates remained largely unchanged in a fixed-effect model (OR 0.77, 95% CI 0.43 to 1.40).

However, Wesseldine 1999 also reported GP consultations for asthma over a six-month follow-up period. They reported that 31/78 (39%) of children in the intervention group and 72/77 (90%) of children in the control group had presented during the follow-up period ($P < 0.001$).

There was no difference (P value 0.63) between groups for GP consultations/subject/year in Stevens 2002; the intervention group had mean 3.87 (standard deviation (SD) 3.93) versus mean 4.13 (SD 3.68) in the control group.

Karnick 2007 reported significant within-group reductions in clinic visits for all three groups (asthma education only group = 45% reduction; reinforced education group = 49% reduction; and case management and reinforced education group = 79% reduction, $P < 0.001$ for all three groups). For this outcome, the reduction in unscheduled GP visits was significantly better in the case management and reinforced education group compared with either the asthma education only group or the reinforced education group ($P = 0.001$).

Quality of life

Only one study included quality of life scores (Stevens 2002), however there was no difference between groups at 3 months (intervention group mean 5.41 (SD 1.3) versus control group mean 5.38 (SD 1.39), P value 0.9) or 12 months (intervention group mean 5.45 (SD 1.49) versus control group mean 5.73 (SD 1.28), P value 0.19) postintervention. Stevens 2002 also reported Usherwood's index of symptoms and level of disability and symptom

scores at 3 and 12 months postintervention, finding no significant differences between groups. One of the studies completed an asthma morbidity questionnaire at three to four weeks postdischarge (Madge 1997), reporting significantly lower day scores (P value < 0.001) and night scores (P value < 0.001) in the intervention group compared to the control group. No differences were noted in the disability scores between the intervention and control group. Wesseldine 1999 reported no statistically significant difference in median school days missed in the six months postdischarge between the intervention and control group (P value 0.07).

Cost analysis

No studies examined cost-effectiveness. One study reported on cost (Karnick 2007). The authors undertook a pre- and poststudy cost-benefit analysis of the programmes delivered to each of the three study groups, Group 1 (asthma education only), Group 2 (reinforced education), and Group 3 (case management and reinforced education), by comparing cost of care in the 12 months prior to enrolment to the cost of care in the study follow-up period. Costing data were based on the average 1998 Medicaid expenditures for asthma. Given that the majority of participants were insured by the US Medicaid programme, the analysis focused on the expected cost savings of the programme to the local health department. Pre-study costs per child per year were significantly higher in Group 3 compared to Groups 1 and 2 (P < 0.001). Programme cost savings per person were USD 4020, USD 4140, and USD 4503 for Groups 1, 2, and 3 respectively. Cost savings per dollar spent were highest in the asthma education only group (USD 43.64) followed by the reinforced education group (USD 27.66) and finally the case management and reinforced education group (USD 7.79).

Duration of subsequent admissions

None of the included studies reported duration of stay at subsequent admissions.

Adherence to discharge medications

None of the included studies reported adherence to discharge medications.

Mortality postdischarge for respiratory-related illness

None of the included studies reported mortality postdischarge for a respiratory-related illness.

Subgroup analysis and investigation of heterogeneity

1. Disease types: chronic suppurative lung disease, bronchiectasis, asthma, others

All studies were conducted on children who had asthma, therefore no subgroup analysis was relevant.

2. Two or more comorbidities; these comorbidities may include but are not limited to the following: atopy, dysphagia, bronchopulmonary dysplasia, juvenile diabetes, congenital/neurological disorders, obstructive sleep apnoea

None of the included studies reported comorbidities.

3. Age groups of children: preschool (aged < 6 years) versus older children (aged 6 or more years)

We did not perform subgroup analyses as data by age group were not available. Wesseldine 1999 reported no significant differences in the effectiveness of the intervention in reducing hospital readmissions by children aged less than 5 years compared to older children (P value < 0.25). Madge 1997 reported an imbalance in age distribution between the intervention and control groups, with more younger children in the intervention group. After stratifying for age in Cox proportional hazards models for the outcome of hospital readmissions, they reported that the differences between the intervention and control groups remained significant, favouring the intervention group.

4. Control group (e.g. usual discharge letter, action plans)

The control group in all four studies varied, therefore it was not possible to explore the control group in subgroup analysis.

5. Setting: low-income economies, low-middle-income economies, upper-middle-income economies, high-income economies, high-income Organisation for Economic Co-operation and Development members

None of the included studies reported income levels in their analyses.

6. Indigenous versus non-Indigenous

None of the included studies reported outcomes by Indigenous status. Karnick 2007 was conducted in a disadvantaged community of non-Hispanic black and Hispanic children but did not report subgroup analyses by these groups. Stevens 2002 included children from Asian, West Indian, Caucasian (understood to be white), and "other" ethnic origins, however data were not presented by these subgroups.

7. Hospital type, e.g. tertiary paediatric referral centre or local/district hospital

All studies were conducted in tertiary paediatric hospitals.

Sensitivity analysis

1. Excluding studies with a high risk of bias based on the 'Risk of bias' assessment

As risk of bias was unclear for the two studies included in the meta-analysis, we did not perform sensitivity analyses.

2. Unpublished data

No included studies were found to have unpublished data.

DISCUSSION

Summary of main results

We found 4 RCTs involving 773 participants that could be included in this review. Three of the four studies included only children with asthma, and one, [Stevens 2002](#), included both children with asthma and acute wheezing illnesses. No studies addressed other chronic respiratory diseases in children. The duration of follow-up ranged from 2 to 14 months. All studies reported our review's primary outcomes (exacerbations requiring hospitalisation or ED visit). We could combine data from only two studies for the meta-analysis of exacerbations requiring hospitalisations, ED visits, and unscheduled GP visits ([Madge 1997](#); [Wesseldine 1999](#)). Meta-analyses of data from two studies showed significant reductions in the proportion of children hospitalised for acute asthma favouring the caseworker-assigned group compared to the control group ([Madge 1997](#); [Wesseldine 1999](#)). There was no evidence of statistical heterogeneity between the two studies, and the number needed to treat for an additional beneficial outcome was 6, 95% CI 4 to 9. The absolute reductions in numbers of hospitalisations could not be analysed, however the analysis identified a reduction of 189 per 1000 children requiring hospitalisation during the follow-up periods. Nevertheless, in [Madge 1997](#), children in the intervention group were discharged home with a course of steroids and guidance on when to use them, and hence differentiating the effect of the caseworker intervention from the appropriate use of steroids is difficult. A significant reduction in hospitalisations compared to baseline historical data was also observed in [Karnick 2007](#), which used historical controls and enrolled hospitalised and non-hospitalised children. However, there were no significant differences between specific intervention groups during prospective follow-up ([Karnick 2007](#)). In the final study ([Stevens 2002](#)), which also enrolled hospitalised and non-hospitalised children, as well as children without a specific diagnosis of asthma, no difference in the proportions of children requiring hospitalisation during follow-up was observed.

There was significant heterogeneity between the studies for the outcomes of ED visits and GP visits, and the per-protocol random-effects models employed to analyse the data found no significant differences between the intervention and control groups. Of particular importance is that the estimates of effect with respect to ED presentations varied substantially when analysed by either a fixed-effect or random-effects model. This may be due to differences in baseline risk and periods of follow-up between the studies, however there were insufficient data available in both papers to evaluate these potential differences. Individually, however, [Madge 1997](#) reported no effect on these outcomes, whereas significant differences favouring the intervention group were observed by [Wesseldine 1999](#). Furthermore, the age distribution of children in the two studies differed: 62.5% of children in the Wesseldine study were aged 2 to 5 years compared to 48.7% in the Madge study, and hence younger children may be more likely to present to EDs and GPs for acute asthma due to either an increased burden of disease or parental concern. There were no adverse events reported in any of the included studies.

One study reported on quality of life and found no differences between groups ([Stevens 2002](#)). Two studies included measures of disability, [Stevens 2002](#), or morbidity, [Madge 1997](#). [Stevens 2002](#) reported no differences between the groups identified, [Madge 1997](#) significantly lower day scores (P value 0.0005) and night scores (P value 0.0002) in the intervention group compared to the control group. There were no data available to assess the overall effectiveness of the intervention on the other secondary outcomes of cost-effectiveness and adherence to discharge medications. We were unable to perform subgroup analyses.

Overall completeness and applicability of evidence

The review included four studies, and as only children with asthma were included, the findings are likely to be applicable only to children with asthma, and the results only reflect the potential benefits of the intervention on hospitalisations. There may be associated reductions with respect to ED presentations, however the heterogeneity of the studies limit our confidence in that conclusion, although one of the studies did find significant reductions over six months postdischarge ([Wesseldine 1999](#)). Other than differing periods of follow-up, the reasons for the differences between the two studies with respect to ED and GP presentations are not clear. We could not include the [Karnick 2007](#) and [Stevens 2002](#) trial data in meta-analyses because the study populations included children who were not hospitalised at the time of enrolment. Furthermore, the Stevens study included children who may not have had asthma, and although this potential measurement error is likely to be non-differential given randomisation, it is possible their inclusion has biased the estimates of effect towards the null. There is no evidence to support the intervention in children with chronic respiratory diseases other than asthma or in subgroups of children

with asthma, although one study reported that their findings were not influenced by the effect of age (Wesseldine 1999). Of note is that the two studies included in the meta-analysis were conducted approximately 20 years ago, and standard or usual care was likely to differ to current practice. Guidelines recommending that self management action plans be incorporated into asthma management did not appear until 2003 (British Thoracic Society 2003). As the intervention groups in the studies included in the meta-analysis included both self management plans and caseworker-assigned discharge planning, it is not possible to determine which component of the intervention was responsible for the positive outcomes observed, nor whether it is the two combined that is critical. This also applies to the impact that the use of steroids may have had on the outcomes, particularly if that use differed between groups in each of the studies, as is likely to have occurred in Madge 1997, in which children in the intervention group were discharged home with a course of steroids and guidelines on when to use them if needed. In Wesseldine 1999, oral steroids were provided to children in the intervention group if previously used and acceptable to parents, however the number of children provided with steroids and who used them was not reported; baseline use between the intervention and control groups did not differ. As data on steroid use postdischarge were not available, adjustment for this potential confounder could not be performed.

Quality of the evidence

Overall, we judged the quality of the studies to be low given the lack of clarity around randomisation processes and blinding. Furthermore, two of the included studies did not account for other aspects of the intervention that could have independently impacted on study outcomes (Karnick 2007; Madge 1997). In Madge 1997, participants in the intervention group were provided with a course of oral steroids and instructions on when to use them, but the investigators did not collect data on the use of steroids postdischarge, and hence unmeasured confounding is a possible explanation for the differences observed between the intervention and control groups. Karnick 2007 used historical clinical data as the control group, but arguably the control group could be considered to be the asthma education only group which is currently the standard practice. Stevens 2002 included children presenting with acute wheezing illness, of whom 69% had not had a previous diagnosis of asthma, which may have introduced a non-differential measurement error, hence biasing the study findings.

Potential biases in the review process

This review was based on a published protocol (Hall 2016). We followed standard procedures as described in the *Cochrane Handbook for Systematic Reviews of Interventions* in order to minimise

bias in the review process (Higgins 2011). With regard to the search process, the Cochrane Airways Group Information Specialist designed and conducted the main electronic search; two review authors independently sifted the search results; and two review authors (one with expert clinical knowledge) reviewed the full-text results. Consistent with Cochrane methods, we excluded no trials on the basis of language, publication status, or outcomes reported, so we are confident that we identified all potentially relevant evidence from RCTs. Two review authors (KH and HP) independently assessed the risk of bias. KO and the review editor (Christopher Cates) independently checked the data extraction.

Agreements and disagreements with other studies or reviews

We were unable to identify previous reviews that had conducted meta-analysis of known data.

AUTHORS' CONCLUSIONS

Implications for practice

This review has demonstrated that structured discharge planning and case-managed discharge planning with education and individualised written information reduces subsequent hospitalisations in children with asthma. However, our review does not distinguish between which aspects of the intervention have the greatest impact, particularly given that the intervention differed in type, delivery, and duration between the studies, with one study providing steroids to family for use postdischarge if needed. The evidence applies to interventions that provide trained asthma educators to deliver asthma education programmes in hospital, however it does not reflect the effect of postdischarge support, as this was limited in all studies. Hence there is limited research addressing a broader role of caseworker support beyond brief contact with the families once the child has gone home. There is currently no evidence to support or refute the benefits of caseworker-assigned discharge planning for other chronic paediatric respiratory illnesses, nor for other outcomes such as adverse events, medication use and adherence, quality of life, and healthcare resource use beyond hospitalisations.

Implications for research

Due to a lack of trials meeting the review's selection criteria, many outcomes could not be addressed in this review. There is therefore scope for future research to investigate the effects of caseworker-assigned discharge plans on adverse events, emergency department and general practitioner use, quality of life, cost-effectiveness, duration of hospital admission, adherence to discharge medications, and mortality rates for respiratory illnesses. In addition, there are

other factors that could impact the effectiveness of discharge planning that were not considered in this review but are worthy of further studies such as disease type, comorbidities, the age of the child (e.g. aged < 6 versus older children), the socio-economic status of families, and ethnicity and cultural considerations. Furthermore, we identified only two studies that incorporated postdischarge support by the caseworker, and one could not be included in the meta-analyses. Hence comparability between the interventions included in the review was limited. Further research on caseworker-managed discharge planning and care needs to include postdischarge support as a core component of supporting families to manage their child's illness. Similarly, future research would better support translation into practice by investigating, if possible, the relative importance of each component of the intervention to outcomes. Finally, current health care delivery for people with chronic diseases is becoming increasingly technology dependent with a large array of resources such as websites, mobile applications, and clinician- and patient-oriented management algorithms available or under investigation for several diseases. These often include multidisciplinary teams and patient support groups, and hence it is likely that caseworker-assigned discharge management would be only one aspect of strategies aimed at reducing morbidity in those

with chronic respiratory diseases. Further research aimed at evaluating caseworker management in chronic diseases will need to account for these additional support mechanisms.

ACKNOWLEDGEMENTS

We thank Dr Chris Cates for his expertise and guidance on the data extraction and analysis. We also thank Elizabeth Stovold for undertaking the relevant searches. We are grateful to the staff for the general support of the Cochrane Airways Group including that of Sean Beggs who was the Contact Editor for this protocol and commented critically on the document.

The Background and Methods sections of this review are based on a standard template used by Cochrane Airways.

This project was supported by the National Institute for Health Research (NIHR), via Cochrane Infrastructure funding to the Cochrane Airways Group. The views and opinions expressed therein are those of the authors and do not necessarily reflect those of the Systematic Reviews Programme, NIHR, National Health Service (NHS), or the Department of Health.

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* Indicates the major publication for the study

CHARACTERISTICS OF STUDIES

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

Karnick 2007

Methods	A randomised, 3-arm, parallel-group clinical trial of discharge planning that included reinforced asthma education, with or without case management, in children with asthma. The diagnosis was confirmed by a paediatric pulmonologist at enrolment. Children with other chronic conditions (not further specified) were excluded. The study was conducted at a single hospital in Chicago, USA between July 2000 and May 2001. Children were recruited through the emergency department, inpatient wards, and from referrals to the institution's paediatric pulmonologist. Data were retrospectively collected for 1 year before study start and monthly for 9 months following enrolment. The authors undertook within- and between-group analyses
Participants	212 children aged 1 to 16 years with asthma: Group 1: 74 children, mean age 5.5 years, 55% male Group 2: 68 children, mean age 5.1 years, 66% male Group 3: 70 children, mean age 5.7 years, 59% male All but 1 child in the study identified as non-Hispanic black or Hispanic, and the majority (> 85%) were Medicaid recipients
Interventions	Group 1 (Asthma Education Group): a basic asthma education session lasting 20 to 30 minutes delivered by an asthma "lay health" educator with the child and his/her caregivers. If children required any further support, they were referred to their primary care provider Group 2 (Reinforced Education Group): Participants and caregivers received the same education session as Group 1, and this education was reinforced as needed postdischarge with a minimum interval being reinforcement at the study's monthly follow-up telephone calls. Participants were encouraged to call the asthma educator if needed Group 3 (Case Management and Reinforced Education Group). Participants received the same education package as Group 2 and had a case manager assigned to them. The case manager completed an initial case evaluation and developed a case management plan with the family. Active support was provided to the family in implementation of the management plan Controls: Group 2 above
Outcomes	<ol style="list-style-type: none"> 1. Hospitalisations for asthma 2. Hospital days for asthma 3. Emergency department presentations for asthma 4. Clinic visits for asthma (the type of clinic was not specified) 5. Number of school days missed because of asthma 6. Cost-benefit of the intervention
Notes	At enrolment, participants' existing treatment plans were amended by a paediatric pulmonologist to comply with the National Health, Lung and Blood Institute treatment guidelines. The study was funded by Michael Reese Health Trust and the Crown Foundation

<i>Risk of bias</i>		
Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	The random sequence generation was not described in the paper
Allocation concealment (selection bias)	Unclear risk	Allocation concealment was not described in the paper.
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	Blinding of participants and personnel were not described in the paper
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Blinding of outcome assessment was not described in the paper
Incomplete outcome data (attrition bias) All outcomes	Low risk	165 children (77.8%) completed the 9-month follow-up. An average of 8.7 months (6 to 9) were completed per participant. There were no significant differences in follow-up rates by study group
Selective reporting (reporting bias)	Unclear risk	Unable to assess reporting bias
Other bias	Unclear risk	There was no concurrent control group where no intervention was provided. However, current standard practice (asthma education only group) could have been considered the control group. Authors' main analysis was comparison of outcomes with historical clinical data

Madge 1997

Methods	<p>A prospective randomised controlled trial of a discharge plan involving an asthma home management training programme. The study was conducted between January 1994 and January 1995 in 4 medical wards of the Royal Hospital for Sick Children, Glasgow, Scotland</p> <p>All children 2 years to < 14 years of age admitted with acute asthma were eligible for the study</p> <p>Detailed written consent was not obtained from participants or caregivers or both</p> <p>Randomisation was performed before study commencement by drawing cards and allocating each sequential future admission to either the intervention or control group. The duration of the entire study from first randomisation was 14 months, hence individual children completed follow-up periods of between 2 and 14 months</p>
Participants	<p>201 children with acute asthma were enrolled and randomised.</p> <p>Control group: N = 105; male 62, female 43; median age 4.23 (2.0 to 15.3)</p> <p>Intervention group: N = 96; male 62, female 34; median age 6.0 (2.0 to 13.1)</p>
Interventions	<p>Control group: "Usual care"</p> <p>Authors commented: "For both the groups all clinical care, including decisions about drug management and medical follow up were determined by their attending paediatrician following standard practice"</p> <p>Intervention group:</p> <p>A structured asthma education and home management training programme was developed</p> <p>A nurse-led teaching programme using the current attack as a model for management of future attacks</p> <ol style="list-style-type: none"> 1. 2 discussion/teaching sessions lasting 45 minutes 2. Advice "going home with Asthma" booklet given 3. Written summary of agreed management plan information 4. Course of oral steroids and guidance of when to use 5. Follow-up appointment in nurse-run asthma clinic 2 to 3 weeks postdischarge 6. Telephone advice from nurse available throughout study
Outcomes	<p>Primary outcome: subsequent admissions to hospital</p> <p>Secondary outcomes: subsequent attendance at emergency departments; presentation to a general practitioner for acute asthma within 3 to 4 weeks of discharge from hospital; an asthma morbidity questionnaire completed by carers 4 weeks postdischarge</p>
Notes	No funding source was stated.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Randomisation was performed before the study commenced by drawing cards (not further described) and allocating sequential future admissions to intervention or control group. No details provided on who performed the randomisation process

Madge 1997 (Continued)

Allocation concealment (selection bias)	High risk	No allocation concealment
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	Parents in the control group were blinded to which group they were allocated; blinding of participants in intervention group not described. Blinding of personnel not described
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Blinding of outcome assessments not described, but the manuscript mentioned that “Decisions to admit were made by the clinical staff in the emergency room who had no information on whether the child had been in the intervention or control group”
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Insufficient information to permit judgement of low risk or high risk
Selective reporting (reporting bias)	Low risk	All study outcomes were reported.
Other bias	Unclear risk	Unable to assess reporting bias. Use of steroids between intervention and control groups was not described

Stevens 2002

Methods	A randomised controlled trial of children aged 14 months to 5 years who were admitted to or who attended the emergency department (A&E) or children’s emergency assessment unit (CAU) at Children’s Hospital, Leicester Royal Infirmary and Booth Hall Children’s Hospital, Manchester with a primary diagnosis of asthma or wheeze over a 13-month period in 1998-1999. Randomisation took place at the time of discharge for the children admitted to hospital. The children who attended A&E or CAU but who were not admitted were contacted by telephone on the first working day after attendance Written informed consent was obtained from parents. Randomisation was by blocks of 10, codes were held in individual sealed envelopes. Separate lists were generated for each patient group and for each study centre
Participants	200 children aged 14 months to 5 years with asthma 101 children were randomised to the control group, 69 male, 32 female, median age 32 months (18 to 61) 99 children were randomised to the intervention group, 65 male, 34 female, median age 32 months (14 to 61)
Interventions	Children assigned to the control group received standard care (a range of medical and nursing approaches according to the skills of health professionals) Children assigned to the intervention group received: <ol style="list-style-type: none"> 1. a general education booklet; 2. a self guided written management plan;

	3. 2, 20-minute structured educational sessions, given on a 1-to-1 basis by a specialist respiratory nurse.
Outcomes	Outcomes were measured at 3, 6, and 12 months. Primary outcomes: 1. GP consultation rates 2. Hospital readmissions 3. Attendances to A&E or CAU Secondary outcomes: 1. Child's asthma symptoms and perceived disability as perceived by parents, using the Index of Perceived Symptoms in Asthmatic Children (IPSAC) 2. Parents' quality of life, using the Paediatric Asthma Caregiver's Quality of Life Questionnaire (PACQLQ)
Notes	The study was funded by the NHS Executive Mother and Child Health Programme Approval was given by the Leicestershire Health Ethics Committee At Booth Hall Children's Hospital, recruitment and data collection were completed by 1 researcher

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Randomisation generated numerical codes in random permuted blocks of 10
Allocation concealment (selection bias)	Low risk	Codes were held in individual sealed envelopes that were opened after consent was obtained
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	Single blinding for 1 secondary outcome
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Symptom diaries analysed by blinded research team member.
Incomplete outcome data (attrition bias) All outcomes	Low risk	10% to 12% did not complete the 12 months.
Selective reporting (reporting bias)	Low risk	All outcome data were presented.
Other bias	High risk	The study population included children presenting with an acute wheezing illness that may not have been asthma. 69% of children did not have a previous diagnosis of asthma

Methods	A randomised controlled trial of children aged 2 to 16 years who were admitted to the Children's Hospital Leicester with a diagnosis of acute asthma over a 12-month period in 1996. Randomisation took place at the time of discharge, at which time the child was allocated to the intervention group or the control group, which was standard care from ward staff. The study period was for 6 months post-index admission
Participants	No data were provided on the number of children screened and reasons for ineligibility or refusal 160 participants were enrolled and 160 randomised. Control group (N = 80): median age 5.9 years (2 to 16 years); 30 male, 30 female Intervention group (N = 80): median age 5.6 years (2 to 15 years); 48 male, 32 female
Interventions	Control group: "Standard care from ward staff". Standard discharge care was described as "variable" and dependent on factors such as availability and experience of parents and staff and time constraints Intervention group: The structured nurse-led discharge package consisted of an interview during which information was provided on the nature of asthma, the recognition of risk factors and how to avoid them, and on drugs and devices 1. The educational component emphasised guided self management, and an individual written home management plan was devised for each child that allowed doses of preventers and relievers to be adjusted according to symptoms and peak flow (for children over 7 to 8 years of age). 2. A short booklet for parents and children entitled "At home with Asthma" was provided to reinforce verbal information. This booklet also provided relevant local and national contact numbers if the family needed further information or support or both. 3. Children were also provided with a course of oral steroids at the time of discharge, with the family given instructions on when to use them postdischarge, however the use of those steroids postdischarge was not monitored. 4. Any care children may have received in the community postdischarge was not reported. 5. No postdischarge follow-up support or other postdischarge interventions were provided.
Outcomes	Primary outcome: readmission in the 6 months after discharge Secondary outcomes: 1. Re-attendance without admission at an emergency department or children's admission unit in the 6 months postdischarge 2. Unplanned visits to GP in the 6 months postdischarge 3. GP visits for any acute respiratory illness in the 6 months postdischarge 4. Days off school for any medical illness (for schoolchildren) in the 6 months since discharge
Notes	The intervention package was delivered by a single specialist nurse who was also the chief investigator Many eligible children were missed due to time constraints or because the investigator was unavailable for recruitment on the day of discharge Approval was given by the Leicestershire Health Ethics Committee The study was conducted with the support of Glaxo Wellcome UK

<i>Risk of bias</i>		
Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Computer-generated numerical codes in blocks of 10. No details were provided on who undertook the randomisation procedures
Allocation concealment (selection bias)	Low risk	Codes were held in sealed envelopes that were opened after consent had been obtained
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not blinded, single investigator
Blinding of outcome assessment (detection bias) All outcomes	High risk	Not blinded, single investigator
Incomplete outcome data (attrition bias) All outcomes	Low risk	Low dropout rate
Selective reporting (reporting bias)	Low risk	All outcomes reported on.
Other bias	Unclear risk	No other bias detected.

GP: general practitioner

Characteristics of excluded studies [ordered by study ID]

Study	Reason for exclusion
Bloch 2013	Children were not admitted to hospital.
Charlton 1994	Education programme, no caseworker included
Choy 1999	Not an RCT
Cowie 2002	Included adults
Ekim 2016	Not an RCT. A quasi-experimental study to determine efficacy of a nurse-led discharge planning programme for childhood asthma management

(Continued)

Farber 2004	Education programme, no caseworker included
Garrett 1994	Community-based education programme
Gorelick 2006	Study participants were not admitted to hospital, treated in emergency department only
Greineder 1995	Not an RCT
Greineder 1999	Participants were selected from urban health centres, were recruited by reviewing hospitalisations lists or by referral from paediatrician
Griffiths 2004	Education programme, no caseworker included
Harish 2001	Education programme, no caseworker included
Hughes 1991	Children were not recruited from hospital.
Kamps 2004	Study was conducted in general practice.
Khan 2004	RCT of parents of children discharged from emergency department, children were not hospitalised
Krieger 2009	Study was conducted in homes.
Marks 1999	Study did not fulfil inclusion criteria as the intervention consisted of study investigators ringing GPs at or before discharge and informing the GPs of the child's admission and planned follow-up. There was no specific individual caseworker-assigned planning
McCowan 1997	Conducted in general practice
Mitchell 1986	Enrolled to the study after discharge
Morice 2001	Study did not include children.
Ng 2006	Education programme, no caseworker included
Nickerson 2009	Not an RCT. Retrospective random review of medical charts monitoring discharge planning
Olmedo 2001	Education programme, no caseworker included
Popatia 2017	Not an RCT
Rice 2015	Education programme, no caseworker included
Shanovich 2009	Participants recruited from 6 health insurance organisations, and took part in an intake interview with asthma education prior to randomisation

(Continued)

Sin 2004	Participants were not admitted to hospital, study included adults
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RCT: randomised controlled trial

DATA AND ANALYSES

Comparison 1. Individual caseworker-assigned discharge plans compared to non-caseworker-assigned plans

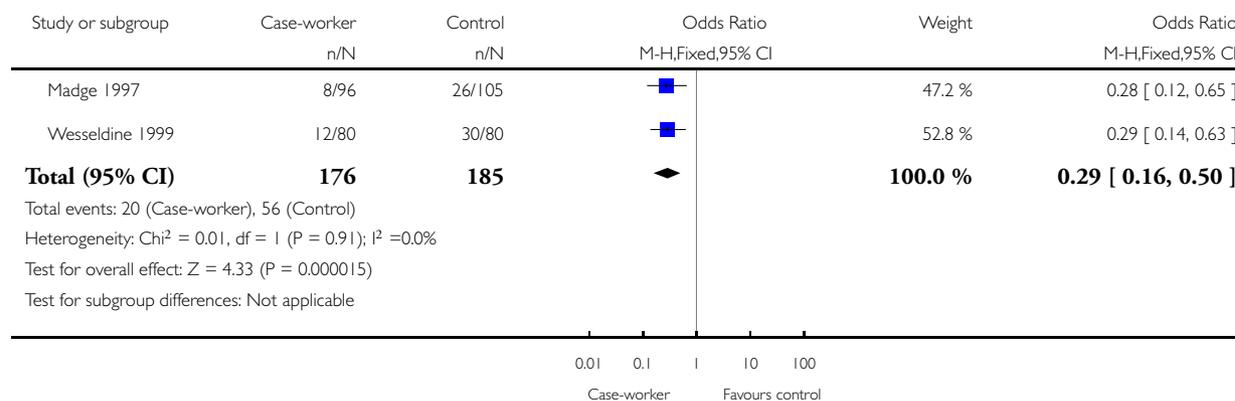
Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Hospitalisation	2	361	Odds Ratio (M-H, Fixed, 95% CI)	0.29 [0.16, 0.50]
2 Emergency department visits	2	361	Odds Ratio (M-H, Random, 95% CI)	0.37 [0.04, 3.05]
3 General practitioner visits	2	351	Odds Ratio (M-H, Random, 95% CI)	0.87 [0.22, 3.44]

Analysis 1.1. Comparison 1 Individual caseworker-assigned discharge plans compared to non-caseworker-assigned plans, Outcome 1 Hospitalisation.

Review: Caseworker-assigned discharge plans to prevent hospital readmission for acute exacerbations in children with chronic respiratory illness

Comparison: 1 Individual caseworker-assigned discharge plans compared to non-caseworker-assigned plans

Outcome: 1 Hospitalisation

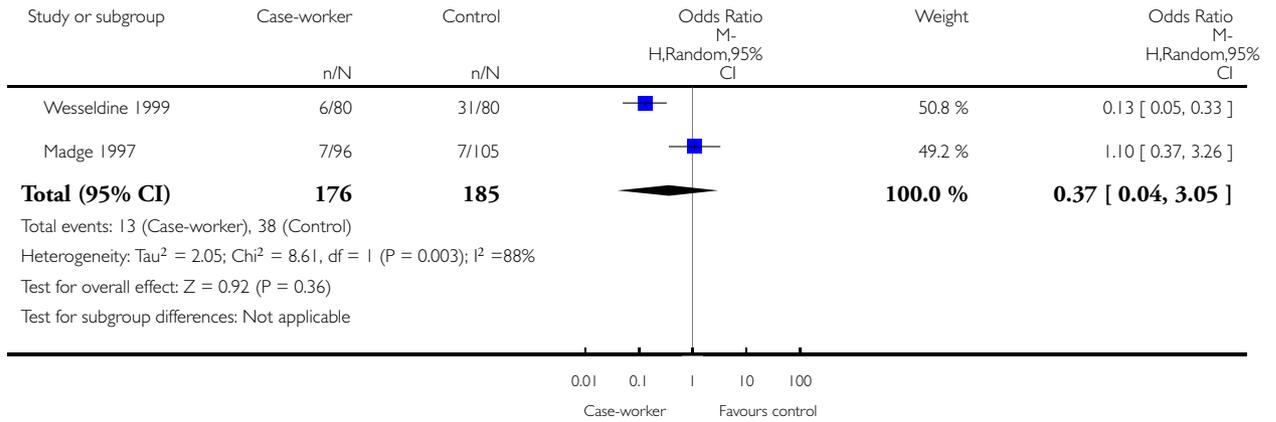


Analysis 1.2. Comparison 1 Individual caseworker-assigned discharge plans compared to non-caseworker-assigned plans, Outcome 2 Emergency department visits.

Review: Caseworker-assigned discharge plans to prevent hospital readmission for acute exacerbations in children with chronic respiratory illness

Comparison: 1 Individual caseworker-assigned discharge plans compared to non-caseworker-assigned plans

Outcome: 2 Emergency department visits

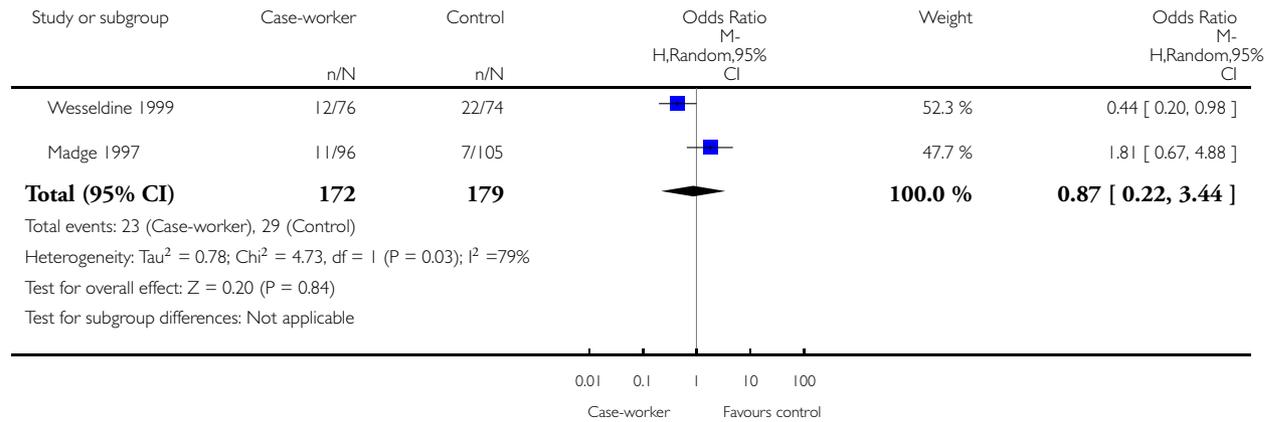


Analysis 1.3. Comparison 1 Individual caseworker-assigned discharge plans compared to non-caseworker-assigned plans, Outcome 3 General practitioner visits.

Review: Caseworker-assigned discharge plans to prevent hospital readmission for acute exacerbations in children with chronic respiratory illness

Comparison: 1 Individual caseworker-assigned discharge plans compared to non-caseworker-assigned plans

Outcome: 3 General practitioner visits



APPENDICES

Appendix 1. Search strategies

Cochrane Airways Trials Register (Cochrane Register of Studies)

- #1 MESH DESCRIPTOR Asthma EXPLODE ALL TREES
- #2 asthma*
- #3 MESH DESCRIPTOR Respiratory Sounds EXPLODE ALL TREES
- #4 wheez*
- #5 MESH DESCRIPTOR Bronchiectasis EXPLODE ALL TREES
- #6 bronchiectasis*:TI,AB,KY
- #7 MESH DESCRIPTOR Lung Diseases
- #8 MESH DESCRIPTOR Suppuration EXPLODE ALL TREES
- #9 ((chronic* NEAR3 suppur* NEAR3 lung*):TI,AB,KY
- #10 MESH DESCRIPTOR Pneumonia EXPLODE ALL TREES
- #11 (recur* NEAR2 pneumoni*):TI,AB,KY
- #12 MESH DESCRIPTOR Bronchitis EXPLODE ALL TREES
- #13 (((bacterial* or recur*) NEAR2 bronchitis*)):TI,AB,KY
- #14 (((chronic* or recurrent*) NEAR2 (respiratory* or lung*) NEAR2 (illness or disease*)):TI,AB,KY
- #15 #1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7 OR #8 OR #9 OR #10 OR #11 OR #12 OR #13 OR #14

#16 MESH DESCRIPTOR Child EXPLODE ALL TREES
 #17 MESH DESCRIPTOR infant EXPLODE ALL TREES
 #18 MESH DESCRIPTOR adolescent EXPLODE ALL TREES
 #19 MESH DESCRIPTOR Pediatrics EXPLODE ALL TREES
 #20 (paediatric* or paediatric* or child* or adolescen* or infant* or young* or preschool* or pre-school* or newborn* or new-born* or neonat* or neo-nat*):TI,AB,KY
 #21 #16 OR #17 OR #18 OR #19 OR #20
 #22 #15 AND #21
 #23 MESH DESCRIPTOR Patient Discharge EXPLODE ALL TREES
 #24 MESH DESCRIPTOR Patient Care Planning EXPLODE ALL TREES
 #25 MESH DESCRIPTOR Case Management EXPLODE ALL TREES
 #26 ((discharge* NEAR3 (plan* or program* or intervention* or service* or procedure*)):TI,AB,KY
 #27 (((patient* or hospital*) NEAR2 discharge*)):TI,AB,KY
 #28 #23 OR #24 OR #25 OR #26 OR #27
 #29 #22 AND #28

CENTRAL (Cochrane Register of Studies Online)

#1 MESH DESCRIPTOR Asthma EXPLODE ALL TREES
 #2 asthma*
 #3 MESH DESCRIPTOR Respiratory Sounds EXPLODE ALL TREES
 #4 wheez*
 #5 MESH DESCRIPTOR Bronchiectasis EXPLODE ALL TREES
 #6 bronchiectasis*:TI,AB,KY
 #7 MESH DESCRIPTOR Lung Diseases
 #8 MESH DESCRIPTOR Suppuration EXPLODE ALL TREES
 #9 ((chronic* NEAR3 suppur* NEAR3 lung*)):TI,AB,KY
 #10 MESH DESCRIPTOR Pneumonia EXPLODE ALL TREES
 #11 (recur* NEAR2 pneumoni*):TI,AB,KY
 #12 MESH DESCRIPTOR Bronchitis EXPLODE ALL TREES
 #13 (((bacterial* or recur*) NEAR2 bronchitis*)):TI,AB,KY
 #14 (((chronic* or recurrent*) NEAR2 (respiratory* or lung*) NEAR2 (illness or disease*)):TI,AB,KY
 #15 #1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7 OR #8 OR #9 OR #10 OR #11 OR #12 OR #13 OR #14
 #16 MESH DESCRIPTOR Child EXPLODE ALL TREES
 #17 MESH DESCRIPTOR infant EXPLODE ALL TREES
 #18 MESH DESCRIPTOR adolescent EXPLODE ALL TREES
 #19 MESH DESCRIPTOR Pediatrics EXPLODE ALL TREES
 #20 (paediatric* or paediatric* or child* or adolescen* or infant* or young* or preschool* or pre-school* or newborn* or new-born* or neonat* or neo-nat*):TI,AB,KY
 #21 #16 OR #17 OR #18 OR #19 OR #20
 #22 #15 AND #21
 #23 MESH DESCRIPTOR Patient Discharge EXPLODE ALL TREES
 #24 MESH DESCRIPTOR Patient Care Planning EXPLODE ALL TREES
 #25 MESH DESCRIPTOR Case Management EXPLODE ALL TREES
 #26 ((discharge* NEAR3 (plan* or program* or intervention* or service* or procedure*)):TI,AB,KY
 #27 (((patient* or hospital*) NEAR2 discharge*)):TI,AB,KY
 #28 #23 OR #24 OR #25 OR #26 OR #27
 #29 #22 AND #28

MEDLINE (Ovid)

1. exp Asthma/
2. asthma\$.tw.

3. Respiratory Sounds/
4. wheez\$.tw.
5. exp Bronchiectasis/
6. bronchiectasis\$.tw.
7. Lung Diseases/
8. exp Suppuration/
9. (chronic\$ adj3 suppur\$ adj3 lung\$).tw.
10. exp Pneumonia/
11. (recur\$ adj2 pneumonia\$).tw.
12. exp Bronchitis/
13. ((bacterial\$ or recur\$) adj2 bronchitis\$).tw.
14. ((chronic\$ or recurrent\$) adj2 (respiratory\$ or lung\$) adj2 (illness or disease\$)).tw.
15. or/1-14
16. exp Child/
17. exp Pediatrics/
18. exp infant/
19. exp adolescent/
20. (paediatric\$ or paediatric\$ or child\$ or adolescen\$ or infant\$ or young\$ or preschool\$ or pre-school\$ or newborn\$ or new-born\$ or neonat\$ or neo-nat\$).tw.
21. or/16-20
22. 15 and 21
23. Patient Discharge/
24. Patient Care Planning/
25. Case Management/
26. (discharge\$ adj3 (plan\$ or program\$ or intervention\$ or service\$ or procedure\$)).tw.
27. ((patient\$ or hospital\$) adj2 discharge\$).tw.
28. or/23-27
29. 22 and 28
30. (controlled clinical trial or randomized controlled trial).pt.
31. (randomized or randomised).ab,ti.
32. placebo.ab,ti.
33. dt.fs.
34. randomly.ab,ti.
35. trial.ab,ti.
36. groups.ab,ti.
37. or/30-36
38. Animals/
39. Humans/
40. 38 not (38 and 39)
41. 37 not 40
42. 29 and 41

Embase (Ovid)

1. exp Asthma/
2. asthma\$.tw.
3. wheezing/
4. wheez\$.tw.
5. exp bronchiectasis/
6. bronchiectasis\$.tw.
7. chronic lung disease/
8. suppuration/
9. (chronic\$ adj3 suppur\$ adj3 lung\$).tw.

10. exp Pneumonia/
11. (recur\$ adj2 pneumonia\$).tw.
12. exp Bronchitis/
13. ((bacterial\$ or recur\$) adj2 bronchitis\$).tw.
14. ((chronic\$ or recurrent\$) adj2 (respiratory\$ or lung\$) adj2 (illness or disease\$)).tw.
15. or/1-14
16. child/
17. exp pediatrics/
18. adolescent/
19. (paediatric\$ or paediatric\$ or child\$ or adolescen\$ or infant\$ or young\$ or preschool\$ or pre-school\$ or newborn\$ or new-born\$ or neonat\$ or neo-nat\$).tw.
20. or/16-19
21. 15 and 20
22. hospital discharge/
23. patient care planning/
24. case management/
25. (discharge\$ adj3 (plan\$ or program\$ or intervention\$ or service\$ or procedure\$)).tw.
26. ((patient\$ or hospital\$) adj2 discharge\$).tw.
27. or/22-26
28. 21 and 27
29. Randomized Controlled Trial/
30. randomization/
31. controlled clinical trial/
32. Double Blind Procedure/
33. Single Blind Procedure/
34. Crossover Procedure/
35. (clinica\$ adj3 trial\$).tw.
36. ((singl\$ or doubl\$ or trebl\$ or tripl\$) adj3 (mask\$ or blind\$ or method\$)).tw.
37. exp Placebo/
38. placebo\$.ti,ab.
39. random\$.ti,ab.
40. ((control\$ or prospectiv\$) adj3 (trial\$ or method\$ or stud\$)).tw.
41. (crossover\$ or cross-over\$).ti,ab.
42. or/29-41
43. exp animals/ or exp invertebrate/ or animal experiment/ or animal model/ or animal tissue/ or animal cell/ or nonhuman/
44. human/ or normal human/ or human cell/
45. 43 and 44
46. 43 not 45
47. 42 not 46
48. 28 and 47

Appendix 2. Search strategy to identify relevant trials from ClinicalTrials.gov and WHO ICTRP

“discharge planning AND acute respiratory exacerbations AND clinical trials”

CONTRIBUTIONS OF AUTHORS

KH wrote the first draft of the protocol, undertook data extraction and analyses, and was responsible for the initially drafting the review. KO contributed to the protocol and the final versions of the review. KH, KO, and AC contributed to the body of methodology. KH and HP independently extracted outcome data from the included studies for the review. HP contributed to data extraction and checked transferred data for accuracy. AC provided clinical expertise and guidance with protocol development and edited the protocol and review. All authors approved the final draft before submission.

DECLARATIONS OF INTEREST

KH: no interests to declare.

HP: no interests to declare.

AC: no interests to declare.

KO: no interests to declare.

SOURCES OF SUPPORT

Internal sources

- The authors declare that no such funding was received for this systematic review, Other.

External sources

- National Health and Medical Research Council (NHMRC) Centre, Australia.
A top-scholarship provided to K Hall by the NHMRC Centre for Research Excellence in Lung Health for Aboriginal and Torres Strait Islander Children
- Australian Government, Australia.
Australian Post-Graduate Award provided to K Hall
- National Health and Medical Research Council, Australia.
Career Development Fellowship awarded to K O'Grady
- Queensland Government, Australia.
Smart Futures Fellowship provided to K O'Grady
- National Health and Medical Research Council, Australia.
Practitioner Fellowship awarded to AB Chang
- Asthma Australia, Australia.
Early Career Fellowship provided to H Petsky

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

1. For the outcomes, we used proportion of children rather than the rate or frequency of the outcome of interest (hospitalisations, emergency department visits, and general practitioner visits).
2. As no data were available, subgroup analyses were not undertaken.
3. We added Helen Petsky as an author.
4. We deleted the following sentence from the Methods: “While not part of the quantitative analyses, we also plan a qualitative synthesis of findings from non-RCTs addressing the role of case-worker assigned discharge management strategies”.
5. We added the Chi² test as a measure of heterogeneity and added the threshold.