Primary care professionals providing non-urgent care in hospital emergency departments (Review)

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[Intervention Review]

Primary care professionals providing non-urgent care in hospital emergency departments

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ABSTRACT

Background

In many countries emergency departments (EDs) are facing an increase in demand for services, long-waits and severe crowding. One response to mitigate overcrowding has been to provide primary care services alongside or within hospital EDs for patients with non-urgent problems. It is not known, however, how this impacts the quality of patient care, the utilisation of hospital resources, or if it is cost-effective.

Objectives

To assess the effects of locating primary care professionals in the hospital ED to provide care for patients with non-urgent health problems, compared with care provided by regular Emergency Physicians (EPs),

Search methods

We searched the Cochrane Effective Practice and Organisation of Care (EPOC) Group Specialized register; Cochrane Central Register of Controlled Trials (The Cochrane library, 2011, Issue 4), MEDLINE (1950 to March 21 2012); EMBASE (1980 to April 28 2011); CINAHL (1980 to April 28 2011); PsychINFO (1967 to April 28 2011); Sociological Abstracts (1952 to April 28 2011); ASSIA (1987 to April 28 2011); SSSCI (1945 to April 28 2011); HMIC (1979 to April 28 2011), sources of unpublished literature, reference lists of included papers and relevant systematic reviews. We contacted experts in the field for any published or unpublished studies, and hand searched ED conference abstracts from the last three years.

Selection criteria

Randomised controlled trials, non-randomised studies, controlled before and after studies and interrupted time series studies that evaluated the effectiveness of introducing primary care professionals to hospital EDs to attend to non-urgent patients, as compared to the care provided by regular EPs.

Data collection and analysis

Two reviewers independently extracted data and assessed the risk of bias for each included study. We contacted authors of included studies to obtain additional data. Dichotomous outcomes are presented as risk ratios (RR) with 95% confidence intervals (CIs) and continuous outcomes are presented as mean differences (MD) with 95% CIs. Pooling was not possible due to heterogeneity.

Main results

Three non randomised controlled studies involving a total of 11 203 patients, 16 General Practioners (GPs), and 52 EPs, were included. These studies evaluated the effects of introducing GPs to provide care to patients with non-urgent problems in the ED, as compared to EPs for outcomes such as resource use. The quality of evidence for all outcomes in this review was low, primarily due to the non-randomised design of included studies.

The outcomes investigated were similar across studies; however there was high heterogeneity (I^2>86%). Differences across studies included the triage system used, the level of expertise and experience of the medical practitioners and type of hospital (urban teaching, suburban community hospital).

Two of the included studies report that GPs used significantly fewer healthcare resources than EPs, with fewer blood tests (RR 0.22; 95%CI: 0.14 to 0.33; N=4641; RR 0.35; 95%CI 0.29 to 0.42; N=4684), x-rays (RR 0.47; 95% CI 0.41 to 0.54; N=4641; RR 0.77 95% CI 0.72 to 0.83; N=4684), admissions to hospital (RR 0.33; 95% CI 0.19 to 0.58; N=4641; RR 0.45; 95% CI 0.36 to 0.56; N=4684) and referrals to specialists (RR 0.50; 95% CI 0.39 to 0.63; N=4641; RR 0.66; 95% CI 0.60 to 0.73; N=4684). One of the two studies reported no statistically significant difference in the number of prescriptions made by GPs compared with EPs, (RR 0.95 95% CI 0.88 to 1.03; N=4641), while the other showed that GPs prescribed significantly more medications than EPs (RR 1.45 95% CI 1.35 to 1.56; N=4684). The results from these two studies showed marginal cost savings from introducing GPs in hospital EDs.

The third study (N=1878) failed to identify a significant difference in the number of blood tests ordered (RR 0.96; 95% CI 0.76 to 1.2), x-rays (RR 1.07; 95%CI 0.99 to 1.15), or admissions to hospital (RR 1.11; 95% CI 0.70 to 1.76), but reported a significantly greater number of referrals to specialists (RR 1.21; 95% CI 1.09 to 1.33) and prescriptions (RR 1.12; 95% CI 1.01 to 1.23) made by GPs as compared with EPs.

No data were reported on patient wait-times, length of hospital stay, or patient outcomes, including adverse effects or mortality.

Authors' conclusions

Overall, the evidence from the three included studies is weak, as results are disparate and neither safety nor patient outcomes have been examined. There is insufficient evidence upon which to draw conclusions for practice or policy regarding the effectiveness and safety of care provided to non-urgent patients by GPs versus EPs in the ED to mitigate problems of overcrowding, wait-times and patient flow.

PLAIN LANGUAGE SUMMARY

Does employing general practitioners to provide care for patients with non-urgent problems in emergency departments decrease resource use and costs?

An important portion of patients who attend hospital emergency departments (EDs) present with health problems that are classified as non-urgent. With many EDs experiencing long-waits and overcrowding, it has been suggested that providing primary care services in EDs for patients with non-urgent problems may be an efficient and cost-effective alternative to emergency care.

This review included three non-randomised studies, involving a total of 11 203 patients, 16 General Practitioners (GPs), and 52 Emergency Physicians (EPs), evaluating the effects of introducing GPs to provide care for patients with non-urgent problems in the ED, compared to EPs. The reported outcomes were similar across studies, however, pooling of the results was not feasible due to differences among the studies. Hence, we present the results as individual study risk ratios (RRs).

Two studies, involving 9325 patients and conducted at urban-teaching hospitals, demonstrated that GPs order less blood tests and x-rays and admit fewer patients to hospital. In addition, these studies demonstrated that EPs referred more patients and prescribed more medications than GPs. These two studies showed marginal cost savings of the intervention and provided limited evidence on patients' self-reported health outcomes.

A third study reported no differences between the two approaches with respect to blood tests, x-rays or hospitalizations. It did show that GPs referred more patients and prescribed more medications than EPs. This study involved fewer participants (1878), and used an unstructured triage system which may have led to misclassification of patients into urgent and non-urgent groups.

None of the included studies provided data on patient wait-times, length of hospital stay, adverse effects or mortality. Overall, the evidence is of very low quality, the safety has not been thoroughly examined and results are disparate. The evidence suggests that there is insufficient basis upon which to draw conclusions regarding the effectiveness and safety of care provided by GPs versus EPs for non-urgent patients in the ED.

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

Primary care professionals compared with ordinary emergency department physicians for patients with minor injuries and illnesses who attend hospital emergency departments

Patient or population: patients with minor injuries and illnesses
Settings: hospital emergency departments
Intervention: Primary care professionals
Comparison: ordinary emergency department physicians

Outcomes	Relative effect Risk Ratio range	No of Participants (studies)	Quality of the evidence (GRADE)	Comments
Blood investigations Percent of patients for whom any blood investigation was ordered Follow-up: 7-15 months	**RR** ranged from 0.35 to 0.96	11203 (3 studies[1])	⊕◯◯◯ **very low**[2,3,4]	
Any x-ray Percent of patients for whom any x-ray was ordered Follow-up: 7-15 months	**RR** ranged from 0.47 to 1.07	11203 (3 studies)	⊕◯◯◯ **very low**[2,4]	
Any prescription Precent of patients given medication or prescription Follow-up: 7-15 months	**RR** ranged from 0.95 to 1.45	11203 (3 studies)	⊕◯◯◯ **very low**[2,4]	
Admissions Percent of patients admitted to hospital from ED Follow-up: 7-15 months	**RR** ranged from 0.33 to 1.11	11203 (3 studies)	⊕◯◯◯ **very low**[2,4] ⊕⊕◯◯	
Referrals Percent of patients referred to consultants[4] Follow-up: 7-15 months	**RR** ranged from 0.5 to 1. 21	11203 (3 studies)	⊕◯◯◯ **very low**[2,4,5]	
Time from waiting room to clinical assessment - not reported				
Length of Stay - not reported				

[1] Non-randomised control studies due to cross-over of physicians in primary care sessions in Dale 1995 and predictable allocation of patients to either EPs or GPs in Murphy 1996 and Gibney
1999.
[2] Wide confidence intervals including null-effect and appreciable benefit or harm
[3] Murphy 1996 and Gibney 1999 reported ' ' any blood investigations". Dale 1995 reported data for haematology and biochemistry separately, but were summed, under the assumption of
independence given low event numbers, to provide an estimate of 'any blood investigations' which could be compared with Murphy 1996 and Gibney 1999
[4] Differing estimate of effect in Gibney 1999
[5] In Dale 1995, patients referred to on-call teams were excluded
GRADE Working Group grades of evidence
High quality: We are very confident that the true effect lies close to that of the estimate of the effect
Moderate quality: We are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low quality: Our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect
Very low quality: We have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

BACKGROUND

Description of the condition

Emergency departments (EDs) are designed to provide "rapid, high quality, continuously accessible, unscheduled care" (Ieraci 2000) for a wide range of acute illnesses and injuries. Many large-volume and urban hospitals in high income countries now face rising costs and a crisis in ED overcrowding, a state where the demand for services cannot be provided in a timely fashion. The cause of ED overcrowding is multi-factorial, and can be broken down into input, through-put and out-put factors (Asplin 2003). Input factors are those that affect the demand for ED services, through-put factors are involved within ED management and determine patients' length of stay and output factors involve the efficiency with which patients are discharged or transferred out of the ED for continuing care elsewhere (Asplin 2003).

One of the many possible explanations for overcrowding is the use of EDs for conditions triaged as non-urgent, an input factor which contributes to increased demand for ED services. Use of the ED for non-urgent problems that could be cared for in other settings has been described since the 1970s (Lees 1976) and is often labelled by health professionals as "inappropriate use" (Liggins 1993). The definition of "inappropriate use" is complicated by different definitions in the literature and by the fact that even patients with non-urgent triage can require advanced imaging, consultations and hospitalizations (Dong 2007).Inappropriate ED use may result in increased health-service costs, contribute to overcrowding and can compromise care for true emergencies (Derlet 2000; Jepson 2001; Siddiqui 2002). Inappropriate ED use may also lead to sub-optimal care of non-urgent cases which are managed hastily and without the benefit of comprehensive, continuous care that could be received in a primary care setting (Carret 2009). The introduction of General Practitioners (GPs) may provide more comprehensive and cost- and resource-effective care for patients with non-urgent problems in the ED. GPs may also reduce wait-times and patient's length of stay (by seeing non-urgent patients quickly and freeing Emergency Physicians (EPs) to see patients with more urgent problems), thus addressing some throughput and output factors that contribute to overcrowding.

It has been reported that between 6.7% and 89% of ED visits are for non-urgent problems that could have been looked after in less specialized settings (Carret 2009; Lowy 1994; Murphy 1998). This large variation can be explained by a number of factors. First, there is a lack of consistency in the definition of 'inappropriate use' (Murphy 1998). Studies may use one or some combination of the following criteria to define inappropriate ED use: number of hours' wait without risk of death; need for tests or treatment; need for hospitalisation; possibility of treatment at other levels of care; hours of observation required; or self-perceived urgency (Carret 2009). Second, different triage tools are used across the world, and definitions of non-urgent triage also vary.Other reasons for the large variation in reported inappropriate use include regional differences in health services and sample population demographics.

Inappropriate ED use has been shown to vary across age groups, time of day and day of week, type of disease, region, and socioeconomic status (Bezzina 2005; Carret 2009).

Description of the intervention

Research suggests that patients behave rationally, believing that emergency care is appropriate based on their perception of illness severity, health service availability, and ease of accessibility (Carret 2009; Parboosingh 1987; Rieffe 1999; Walsh 1995). Moreover, many patients attempt to obtain care in other settings only to end up in the ED after referral there, through advice from others, or lack of access to other timely health care. Thus, one response to inappropriate ED use has been to provide primary care and community services to which patients can be directed alongside or within hospital EDs. An unpublished report (Carson 2010) estimates that approximately half of UK hospitals have primary care staff operating within or alongside the ED. These interventions reflect a trend toward the provision of more comprehensive services in the hospital ED, and aim to provide appropriate services for patients with non-urgent problems.

How the intervention might work

There are different models by which primary care can be introduced to the ED, including primary care services (Carson 2010):

- **within the ED;** whereby patients enter the ED and are triaged into separate streams (broadly speaking urgent versus non-urgent); the non-urgent stream is staffed by primary care practitioners.
- **alongside the ED**; whereby primary care is available on-site, next to the ED, and patients either self-select or are re-directed from the ED towards the primary care service.
- **at the front of the ED screening or filtering patients;** whereby primary care practitioners are involved in the triage of patients presenting to the ED and may also use the see-and-treat model of care for non-urgent cases or re-direct non-urgent patients.
- **fully integrated** and providing care jointly with ED-staff on the full range of primary care and higher acuity emergency cases.

This review will focus on the first two models.
If GPs provide more efficient and less resource-intense care than their EP colleagues when managing non-urgent problems, ED time and resources might be more efficiently targeted towards urgent and potentially life-threatening cases.

Why it is important to do this review

Overcrowding in EDs occurs throughout the world. The introduction of primary care services within or alongside hospital EDs is one response to this problem; however, it is not known if this intervention results in better care for patients with non-urgent problems, if it frees hospital and ED resources to provide better care for more urgent medical problems, if it is a safe strategy, or if it is cost-effective.
A report commissioned by the UK Department of Health in 2009 (Carson 2010) examined the impact of introducing primary care services to the ED and concluded that "there is a paucity of evidence on which to base policy and local system design." This review strives to establish and identify gaps in the current evidence base for interventions which have introduced primary care professionals into the ED.

OBJECTIVES

To investigate the effects of locating primary or community healthcare professionals in hospital EDs to manage patients with non-urgent problems as compared with non-urgent care provided by emergency physicians. We addressed the following questions.

A. For patients with non-urgent problems managed by primary/community care professionals (intervention) and hospital-based emergency physicians (control), are there systematic differences in:

i) patient wait times, including:

a) time from arrival to assessment;

b) time to treatment initiation;

c) total length of ED stay (time from arrival to departure from ED discharge or admission);

ii) resources utilized (acute, primary and community):

a) diagnostic interventions;

b) admissions to hospital;

c) referrals to specialists;

iii) subsequent health care use:

a) re-attendance at hospital ED;

b) follow-ups with primary care physicians

iv) costs

v) health outcomes:

a) mortality;

b) adverse events (return visits to the ED or requiring hospitalizations);

B. Is the introduction of primary or community care professionals into the emergency department cost effective relative to the status quo?

METHODS

Criteria for considering studies for this review

Types of studies

We considered individual and cluster randomised controlled trials (RCTs), non-randomised controlled trials, controlled before and after studies (CBA), and interrupted time series (ITS), which met the quality criteria used by the Cochrane Effective Practice and Organisation of Care (EPOC) Group (EPOC 2009). CBA studies were eligible if (1) the pre- and post-intervention periods were the same and (2) if they included a minimum of two intervention and two control sites. We considered ITS studies that reported a clearly defined time point for the intervention and a minimum of three data points both before and after the intervention.

We decided to also include economic studies that were either conducted concurrently to, or based upon data from, effectiveness studies that met the eligibility criteria above.

Types of participants

(1) Patients who present to hospital EDs with illness or injury conditions suitable for primary care. Primary care suitable problems are those that are non-urgent, self-referred, and unlikely to require admission (Bezzina 2005). Furthermore, these problems do not require the specialized services of an ED, such as resuscitative facilities, urgent intervention, rapid and/or complex diagnostic work up (Bezzina 2005) and could equally be managed in an out-patient primary care setting. Given that what is 'primary care suitable' may vary by region, we used the definitions applied in individual studies. We excluded studies comparing triage nurse ordering (Rowe 2011), nurse practitioners for specific problems, or triage liaison physicians (Holroyd 2007; Rowe 2011b) to standard care for patients with non-urgent problems suitable for primary care.

(2) Primary care professionals working in hospital EDs. Primary care refers to the health services and health professionals that are the patient's first point of contact, thus defined it can include GPs, nurse practitioners, EPs, optometrists and dentists. In the context of this review, primary care professionals include any licensed member of an accredited health specialty who normally work in non specialized, outpatient settings to provide continuous "comprehensive care in the sense that only rare or unusual manifestations of ill health are referred elsewhere, and coordination of care such that all facets of care (wherever received) are integrated" (Starfield 1994; Starfield 2001).

(3) Hospital physicians, including residents, senior house officers (SHOs), hospital interns, registrars and consultants (attendings) - who work primarily in emergency medicine.

We excluded dentists, social workers, and optometrists.

Types of interventions

We included interventions in hospital EDs in which patients who presented with non-urgent problems were cared for by primary care professionals instead of regular EPs. The control group received standard ED care from assigned EPs.

We included all interventions for analysis independent of variations in the type of primary care professionals, time of day that they presented to the ED, or triage criteria used to determine 'non-urgent problems'.

A variant of the intervention is where primary-care services (for example out-of-hours GP services) have been established alongside, but not within, a hospital ED. We included these interventions if the newly introduced primary-care service and existing hospital ED worked cooperatively to provide care.

We excluded interventions:

- at non-hospital urgent-care centres;
- in EDs that employed primary care professionals prior to the intervention;
- which diverted patients into "Fast track" areas of the ED;
- where primary care professionals triage patients in the ED; and
- where primary care professionals care for both urgent and non-urgent patients alongside EPs.

Types of outcome measures

Primary outcomes

1. Time from arrival to clinical assessment and treatment for:
   i) patients with non-urgent problems;
   ii) patients with urgent problems.
2. Total length of ED stay (from time of triage/registration to time of admission or discharge).
3. Admission to hospital.

Secondary outcomes

1. Diagnostic tests (overall number, cost).
2. Treatments given (e.g., counselling, prescriptions, procedures)
3. Consultations or referrals to hospital-based specialists.
4. Arrangement of follow-up care.
5. Subsequent utilization of primary care/re-attendance to the ED.

6. Patient education for self-management or appropriate service use.
7. Cost comparison of:
   i) diagnostic tests/investigations;
   ii) treatment;
   iii) referrals.
8. Adverse outcomes:
   i) percentage of 72-hour ED re-presentations;
   ii) percentage of one week admission after discharge;
   iii) mortality.

Search methods for identification of studies

Electronic searches

We searched the following electronic databases up to April 2011:

- the Specialized register of the Cochrane EPOC group (Reference Manager);
- the Cochrane Central Register of Controlled Trials (CENTRAL) (The Cochrane Library, Issue 4);
- MEDLINE (OVID) (1950 - 2011);
- EMBASE (OVID) (1980 - 2011);
- CINAHL (EbscoHost) (1980 - 2011);
- PsychINFO (1967-2011);
- Sociological Abstracts (CSA Illumina)(1952-2011);
- Applied Social Sciences Index and Abstracts (CSA Illumina) (1987-2011);
- Social Science Science Citation Index (Web of Knowledge) (1945 - 2011);
- Health Management Information Consortium (OVID) (1979-2011)

In March 2012 we ran an updated search of Medline (OVID) (2011- March 2012).
In addition, we searched:

- OpenGrey (System for Information on Grey Literature in Europe) http://www.opengrey.eu/
- UK Economic and Social Research Council registry of grants and research http://www.esrc.ac.uk/impacts-and-findings/research-catalogue/index.aspx
- UK National Centre for Primary Care Research and Development database of research relating to the interface between primary and secondary care http://www.medicine.manchester.ac.uk/primarycare/npcrdc-archive/archive/
- UK Clinical Research Network (a registry of ongoing and completed research projects funded by the NHS in England,Northern Ireland, Scotland and Wales; (formerly known as the National Research Register Projects Database) http://www.crncc.nihr.ac.uk/
- NHS Economic Evaluation Database (NEED) http://www.crd.york.ac.uk/crdweb/
- Agency for Healthcare Research and Quality (AHRQ) http://www.ahrq.gov/

We incorporated the EPOC search strategy and methodological filter into the search strategies for primary studies. The MEDLINE search strategy was translated into other databases using the appropriate vocabulary, as applicable. Development of the final search strategy was done with the assistance of a HealthSciences Librarian and the EPOC Trial Search Coordinator. We included studies regardless of publication status or language of publication. Detailed search strategies are included in Appendix 1; Appendix 2; Appendix 3; Appendix 4; Appendix 5; Appendix 6; Appendix 7; Appendix 8; Appendix 9.

Searching other resources

We contacted experts in the field for advice on further potential studies for inclusion. The primary author searched the reference lists of included studies, relevant systematic reviews and the trial registries (e.g., http://www.clinicaltrials.gov/) for ongoing or planned trials. In addition we hand searched abstracts from the last three years of ED conferences (Society for Academic Emergency Medicine, [*Acad Emerg Med*], American College of Emergency Physicians [*Ann Emerg Med*], Canadian Association of Emergency Physicians [*Can J Emerg Med*]).

Data collection and analysis

Selection of studies

We downloaded all titles and abstracts retrieved by electronic searching to the EndNote reference management database. We removed duplicates and we excluded studies which clearly did not meet the inclusion criteria. Two of the review authors (JKK and GF) independently examined the remaining references and the full text of relevant references was obtained. Review authors independently assessed the eligibility of the full text studies. We resolved disagreements by full group discussion.

Data extraction and management

Two authors (JKK and GF) independently undertook data extraction using a modified version of the EPOC data extraction form (Appendix 10) and checklist (http://epoc.cochrane.org/epoc-author-resources). We resolved disagreements by discussion between reviewers.
We attempted to obtain individual patient data from investigators; however, the data were no longer available.

Assessment of risk of bias in included studies

Two authors (JKK and GF) assessed eligible studies for their risk of bias, in accordance with the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011) and the EPOC Risk of Bias Criteria (EPOC 2009) for non randomised studies.
These included:

1. sequence generation
2. concealment of allocation,
3. similar baseline outcome measurements,
4. similar baseline characteristics (for providers and patients),
5. incomplete outcome data,
6. blinding of participants, personnel and outcome assessors,
7. selective reporting of outcomes,
8. protection against contamination and
9. other sources of bias.

We classified individual studies by risk of bias for each of these criteria as follows:

- Low risk of bias
- Unclear risk of bias
- High risk of bias.

We resolved disagreements by full group discussion. Since few studies were identified for inclusion we did not assess whether variations in quality of the evidence could explain differences in study results.

Measures of treatment effect

We reported post-intervention risk ratios (RR) for intervention vs. control groups with associated 95% confidence intervals (CI). No pre-intervention data were reported in the included studies. We were not able to combine data due to high levels of statistical heterogeneity; data are presented in forest plots without a summary estimate, and as a narrative summary.

Unit of analysis issues

We noted that the unit of analysis across all three included studies was the patients. In one study (Dale 1995) the unit of analysis (patients) did not correspond with the unit of allocation (type of physician). A correct analysis for this study adjusting for the unit of allocation would have reduced the precision of the study estimate (larger 95% CI); in the context of a meta-analysis this would have reduced the weight given to this study. As no pooling was attempted due to the high heterogeneity observed, we decided not to attempt any further adjustment (which would have been based on assumptions of group correlation as no data on this were reported in the study).
No cluster randomised trials or ITS designs were identified.

Assessment of heterogeneity

We assessed statistical heterogeneity using I^2 and Chi-square tests. Given the limited number of studies included, we did not further explore quantitative assessment for potential sources of heterogeneity. We have provided a qualitative assessment of potential sources of heterogeneity in the discussion.
In future updates, if a sufficient number of homogenous studies are identified, we will attempt a quantitative assessment of heterogeneity among study populations (patient case mix, demographic characteristics), interventions (level of training amongst hospital doctors, differences in the organisation of care), outcomes and study design using forest plots and the Chi-square and I^2 statistics, which quantify the percentage of total variation across studies that is due to heterogeneity rather than chance (Higgins 2003).

Data synthesis

We present the main findings of this review as forest plots without summary estimates. We calculated and reported findings for each outcome as RRs. We could not, as planned, calculate the relative percent change as no pre-intervention data was available. We used Review Manager 5 (RevMan 2011) for all data synthesis.
In future updates, we will carry out a meta-analysis using a fixed-effect model if we identify enough studies that are sufficiently homogenous in terms of setting, design and intervention. Where there is evidence of heterogeneity, we will apply a random-effects model.

Subgroup analysis and investigation of heterogeneity

While we had planned the following subgroup analysis, they were not completed due to insufficient data:

- patients' socioeconomic status;
- level of primary care health professional training (years in practice or stage of training);
- health care systems and
- age (0-18, 18-65, >65).

In future up-dates, if more eligible studies are found and sufficient data are available, we will perform subgroup analyses to compare outcomes for these categories.

Sensitivity analysis

We planned to conduct sensitivity analyses, however, as only three studies with high heterogeneity were identified for inclusion, we did not pursue this.
In future up-dates, if more eligible studies are found and if a primary meta-analysis is possible, we will undertake a sensitivity analysis to investigate how the pooled intervention effect is affected by the inclusion of low-quality studies at a high risk of bias.

Summary of findings table

We provided results of key outcomes in this review in the Summary of findings table 1. The range of the RR for each outcome across included studies, along with their 95% CI are presented instead of summary estimates.

RESULTS

Description of studies

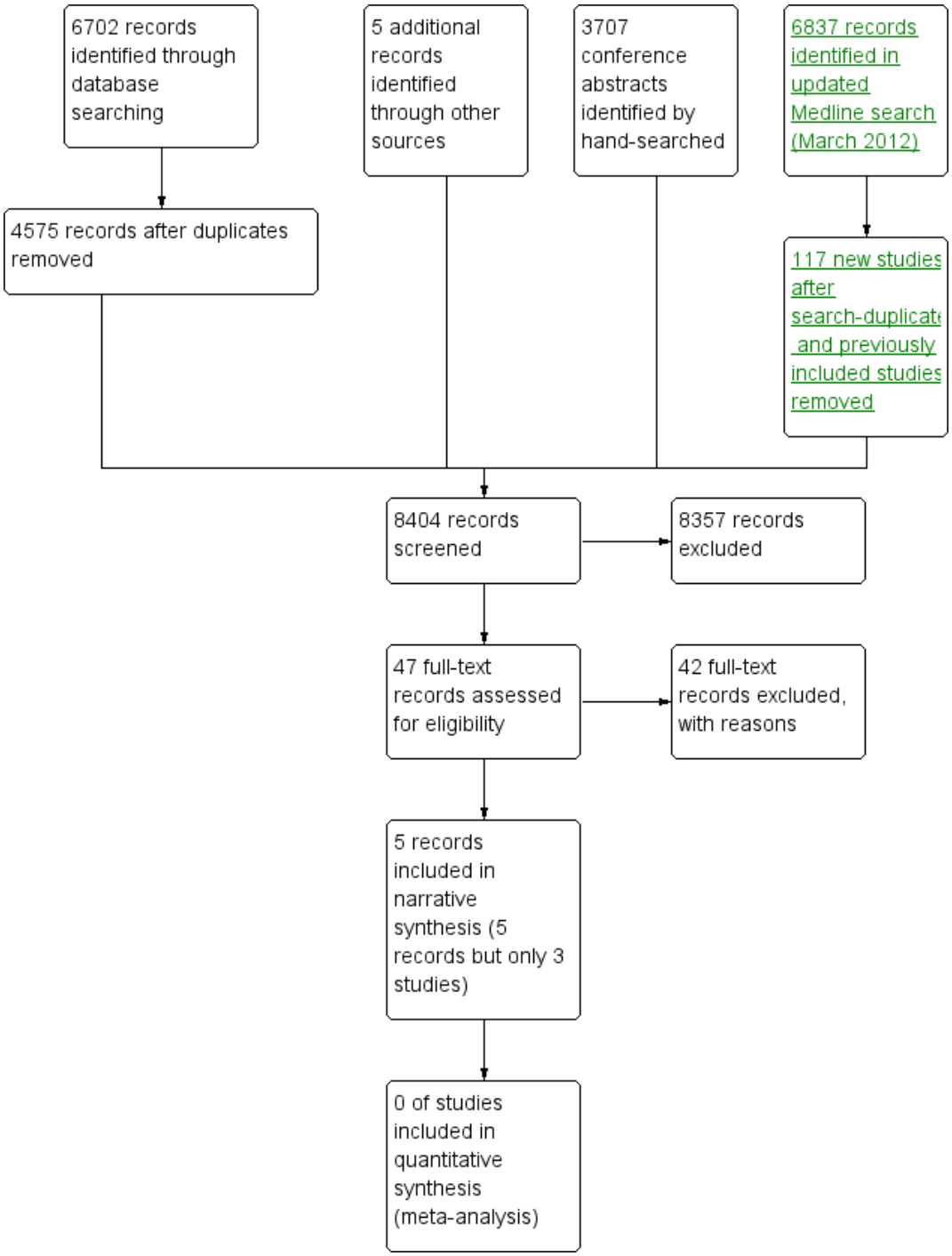
See: Characteristics of included studies; Characteristics of excluded studies.

See Characteristics of included studies table and Characteristics of excluded studiestable.

Results of the search

Figure 1 shows a PRISMA flow chart (Moher 2009) outlining the study selection process and the number of articles excluded at each stage of screening, Application of the electronic search strategy identified 4575 non-duplicate citations. These studies were screened by title or abstract; 4149 were excluded and 426 set aside and abstracts re-read. Of the remaining 426 papers, 46 potentially relevant articles were identified. Full text screening identified three studies meeting the inclusion criteria (Dale 1995; Gibney 1999; Murphy 1996). Dale 1995 presented the data and results in three publications, and Murphy 1996 presented data and results in two publications.

Figure 1. Study flow diagram.



In addition, 3707 conference abstracts were hand-searched and excluded after title or abstract screen. An update Medline search was conducted in March 2012, identifying 1479 studies; after removal of duplicates 117 studies remained. One potentially relevant article (Bosmans 2012) was identified for full text screen, and excluded on the basis of study design (uncontrolled before and after study).

Included studies

All three studies evaluated the effectiveness of introducing GPs into the ED to provide care for patients with "non-urgent" problems. In all studies, GPs working in the ED were supernumerary to the regular EPs.

Study design

The three included studies were prospective, non-randomised controlled studies conducted by two author groups, Dale et al (Dale 1995) and Murphy et al (Murphy 1996, Gibney 1999). None of the included studies were classified as RCTs because either (1) allocation of patients to GPs or EPs was predictable or (2) there was cross-over of physicians allocated to primary care sessions.

The Dale 1995 study took place from 1 June 1989 until 31 May 1990 (48 weeks total within 12 months, as bank holidays and the first two weeks of August and February when Senior House Officers change employment were excluded).

Primary care sessions were established within the ED from 10-1300 hr, 14-1700 hr and 18-2100 hr each day, except weekends when evening sessions were not available (see Figure 2). One physician - either a GP or an EP - was allocated to staff each primary care session according to a weekly rota. All patients triaged as *primary care suitable* during a particular session were seen by the same physician (a GP or EP). Medical staff knew patients' triage status but patients were unaware of their triage status or the type of physician (GP or EP) they were seeing. Both GPs and EPs were encouraged to use a designated consultation room for primary care sessions and were required to complete a consultation record form for each patient seen. Physicians were unaware how this data would be analysed.

Figure 2.

Each week, a random number table was used to select *2-3 daytime* and *1 evening weekday* sessions and *1 daytime weekend* session for inclusion in the study (see Figure 2). Hence 8-10 sessions, which included a mix of GP and EP assignments, were selected for inclusion each week; this was done for a total of 48 weeks. Physicians were unaware of which sessions were included in the study and what outcomes were being measured (Dale 1995). A total of 419 primary care sessions (215 GP and 204 EP staffed session) were selected by stratified random sampling for inclusion in the study. Primary care sessions staffed by an EP formed the control group.

The study authors noted that there was occasional crossover where the allocated physician did not treat primary care patients. This loss of randomisation occurred in both GP- and EP-staffed sessions when the primary care session workload was excessive (to prevent unacceptable wait-times) or when EPs were called away to manage urgent patients or to supervise junior physicians in the ED. The frequency and extent with which crossover occurred was not reported. To remedy this loss of randomisation, the study authors re-grouped patients according to the type of doctor seen and used log-linear modelling to adjust for confounding factors in their analysis.

The Murphy 1996 study took place between August 1993 and August 1994 (12 months). Three GPs were hired to work two four-hour shifts each week alongside EPs. During these primary care shifts, non-urgent patients were allocated to either the GP or EP according to registration time. The control group comprised non-

urgent patients seen by EPs when a GP was on-site. The allocation of patients was predictable but not necessarily consecutive, as the order in which patients were allocated depended on the length of consultations. In addition to temporal ordering, patients were also ordered by triage category - triage category 3 patients were seen prior to category 4.

The GPs and EPs in this study had access to all of the same ED facilities and patients were unaware what type of physician was treating them.

The Gibney 1999 study was conducted between March-September 1996 (7 months). This study was designed by the same author-group as Murphy 1996. Three GPs were hired by the hospital to work on a sessional basis. The frequency and duration of GP sessions in the ED was not reported. As in the Murphy 1996 study, non-urgent patients were allocated to either a GP or EP in alternating (but not random or consecutive) order according to time of registration. Triage status did not factor into the order in which patients were seen as only two triage categories were used - 'urgent' and 'non-urgent' (see 'Triage Methods' below). The control group, as in Murphy 1996, comprised non-urgent patients seen by EPs when a GP was on-site.

The unit of analysis across all three included studies was the patients. In one study (Dale 1995) the unit of analysis (patients) did not correspond with the unit of allocation (type of physician), see: Methods Unit of Analysis.

Classification of Patients - Triage methods and definition of non-urgent patients

The methods to identify non-urgent patients suitable for primary care differed across the included studies.

In Dale 1995, trained nurses (with at least 6 months experience) categorized new attendees as either *Primary Care* or *Accident and Emergency* during triage. Patients were classified by perceived need for care, rather than diagnosis or symptoms. *Primary Care* included self-referred, non-urgent problems that could be managed "in an average local general practice". Patients referred by their GP, those requiring immediate resuscitation or those likely to require hospital admission, were excluded. Application of these criteria resulted in approximately 41% of new attendees being classified as presenting with primary care problems (Dale 1995).

In Murphy 1996, patients were triaged by trained nurses according to the St. James triage criteria, which classifies patients as *(1) life-threatening, (2) urgent, (3) semi-urgent* and *(4) delay acceptable* based on physiological criteria. The proportion of patients in each of these categories was 2%, 16%, 61% and 21% respectively. Patients in triage categories 3 and 4 were eligible for the study; however those who were re-attendees or referred by a GP were excluded. Approximately 66% of all ED attendees met these eligibility criteria (Murphy 1996).

Gibney 1999, used an unstructured triage system executed by untrained receptionists who categorised patients as *urgent* or *non-urgent*. All ambulance patients were excluded from the *non-urgent* category, in spite of a pre-study audit showing that 53% of all ambulance patients were *non-urgent*. Unlike the other two studies (Dale 1995 ;Murphy 1996) in which patients referred by GPs were excluded, Gibney 1999 included patients who had been referred from GPs in the study population. Further details of the criteria used to classify patients were not reported.

Participants and settings

Two of the studies were conducted at major urban teaching hospitals; one in London (Dale 1995) and the other in Dublin (Murphy 1996). One study was conducted at a small district hospital catering to a mixed urban-rural population outside north Dublin (Gibney 1999).

The three included studies involved a total of 11 203 patients, 16 GPs and 52 EPs (42 senior house officers (SHOs), eight registrars, and two consultants). The participating providers constituted eight GPs and 13 EPs (27 SHOs, three registrars and one senior registrar) in Dale 1995; five GPs and 13 EPs (10 SHOs, two registrars and one consultant) in Murphy 1996; three GPs and eight EPs (five SHOs, two registrars and 1 consultant) in Gibney 1999.

GPs experience varied relative to EPs across studies. In Dale 1995 the time since registration was similar for GPs and EPs; in Murphy 1996, GPs had more experience than EPs (seven years vs six months since time of registration). The level and experience of practitioners in Gibney 1999 was not reported.

The studies by Dale 1995 and Murphy 1996 recruited more patients than Gibney 1999 (N=4641, N=4684, and N=1878, respectively). Study populations were similar with respect to age and gender (47.4% and 41.4% females respectively) in Dale 1995 and Murphy 1996 (not reported in Gibney 1999). The proportion of patients presenting with complaints lasting greater than 24 hours was notably higher in Dale 1995 (64.9%) than in Murphy 1996 (36.5%). The three most common complaints of non-urgent patients were: injury and poisoning (44.4%), musculoskeletal (13.7%), and non-specific complaints (7.0%) in Dale 1995; and musculoskeletal (32.3%), skin (11.8%), and neurological (8.8%) in Murphy 1996. The range of non-urgent complaints was broad across both studies.

The case-mix seen by GPs and EPs was not the same in either Dale 1995 or Murphy 1996. In Dale 1995, GPs saw statistically significantly more patients with: mental health disorders (49/1702 [2.9%] vs 34/2382 [1.4%] seen by SHOs and 10/557 [1.8%] seen by Registrars, x^2=64.7, df=14, p<0.001); or dermatologic complaints (126/1702 [7.6%] vs 128/2382 [5.4%] seen by SHOs and 32/557 [5.7%] seen by Registrars, x^2=8.5, df=2, p=0.014). EP's on the other hand saw significantly more injury or poisoning cases (748/1702 [43.9%] seen by GPs, 1028/2382 [43.2%] seen by SHOs and 285/557 [51.2%] seen by Registrars, x^2=12.0, df=2, p<0.002).

In Murphy 1996, GPs saw more Triage 4 patients (relative difference 9.6%, [calculated from data in Table 1 in Murphy 1996]), for which a delay in treatment was considered acceptable, and less semi-urgent (Triage 3) patients than EPs (relative difference -

18.3% [calculated from Table 1 data in Murphy 1996]). Overall, GPs and EPs saw similar numbers of patients (relative difference -1.7% [Table 1, Murphy 1996]).

Gibney 1999 reported no difference in "age, sex, socio-economic status, registration with a GP or type of presenting complaint" between patients seen by GPs or EPs, but data to this effect was not published as it was a brief report.

Outcomes

Data were not available for all of the review outcomes outlined in the protocol; (i.e. waiting times, length of ED stay, proportion of discharges from the ED, patient education for self-management or appropriate service use, and adverse events, including mortality).

Two of the included studies reported admission to hospital (Murphy 1996; Gibney 1999).

Outcomes reported in all three included studies were the number of patients: (a) undergoing investigations (laboratory, electrocardiographic and x-ray in Dale 1995; any blood or x-ray in Murphy 1996 and Gibney 1999), (b) receiving prescriptions, and (c) being referred (to consultants in Dale 1995; unspecified referral in the other two papers).

Blood investigations were broken down differently across studies: Murphy 1996 and Gibney 1999 reported "any blood investigations" while Dale 1995 provided separate data for haematology and chemical pathology. In the results reported here, data on haematology and chemical pathology were combined (assuming that these outcomes were independent) in Dale 1995 to provide a comparison with "any blood investigation" as reported in Murphy 1996 and Gibney 1999.

Two of the three included studies (Dale 1995, Murphy 1996) provided economic evaluations of the cost-effectiveness of introducing GPs to the ED, compared with the current standard of care/system with regular ED staff.

Other outcomes assessed in the included studies were patient's self-reported reasons for attending the ED (Dale 1995a), register data of subsequent health care use (Dale 1995), extent of recovery at 7 to 10 days (Dale 1995), consultation satisfaction scores (Dale 1995, Murphy 1996), re-attendance within 30 days (Murphy 1996) and health status at one month after index visit (Murphy 1996). Patients re-attendance rates within two years of index visit were reported in an extension of Murphy 1996.

Excluded studies

In total, 42 studies were excluded after full copies of papers were obtained and scrutinised. See Characteristics of excluded studies table.

The main reason for exclusion was ineligible study design (35 studies). Other studies were excluded due to ineligible intervention (4 studies), or a lack of eligible outcomes (8 studies) including one qualitative study.

Risk of bias in included studies

The risk of bias of included studies is described in the risk of bias table within the Characteristics of included studies table and summarized in Figure 3, Figure 4 and below. The main source of bias across studies related to non-randomised methods of allocation.

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

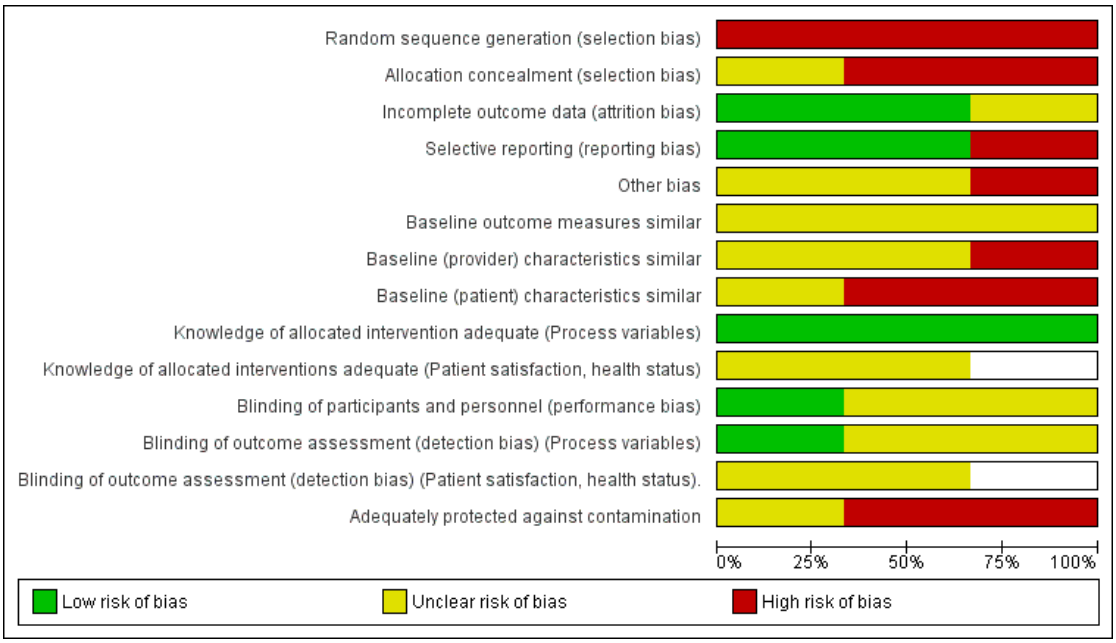


Figure 4. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.

Allocation

The method of sequence generation was not random in any of the included studies, hence all were judged to have high risk of selection bias. Allocation concealment was judged to be at high risk in two of the included studies (Dale 1995, Gibney 1999), since triage nurses were not blinded to the grade and speciality of the physician providing care for 'non-urgent ' patients at a particular session, which could have affected the triage and therefore also what type of patients the physician actually saw (i.e. more emergency type patients if an EP, and less so if a GP was providing the non-urgent care). Murphy 1996 did not describe the allocation concealment, and the risk of bias is therefore judged to be unclear.

Baseline outcome measures

None of the included studies reported any baseline measure of outcome, thus resulting in an unclear risk of bias.

Baseline provider characteristics

Dale 1995 and Gibney 1999 did not report any provider characteristics, and therefore the risk of bias was judged as unclear. In Murphy 1996 there were significant differences in age and work experience between GPs and EPs, with GPs being significantly older and more experienced, and thus a high risk of performance bias favoring GPs regarding the number of patients seen in a given time, or the types of investigations ordered.

Baseline patient characteristics

In Dale 1995 there were significant differences in age, in presenting complaints and injury related diagnosis with type of doctor seen; other variables (e.g., diagnosis of a mental disorder, nervous system or dermatological disease) also varied significantly between type of doctor seen, but had small effect sizes. Also, in Murphy 1996 there were significant differences between patients seen by GPs versus EPs for triage 3 (but not triage 4) patients. The median age of triage 3 patients with neurological complaints was 29 years for those seen by GPs and 38 for those seen by EPs (Kruskal-Wallist Test, P<0.05 in Murphy 1996), The average age of patients seen with "other" complaints by GPs was 32 years and by EPs was 42 years (Murphy 1996). General practitioners were also less likely to see female patients with "other" complaints (RR=0.56; 95% CI: 0.38 to 0.82). Hence, the risk of bias due to differences in patient characteristics is high for both these studies.

In Gibney 1999 the risk of bias for this item was deemed as unclear, as no data were provided to support the statement in the text that "there were no differences in age, sex, socio-economic status, registration with a GP, or type of presenting complaint" (p.43, Col 2, para. 6 in Gibney 1999) between groups.

None of the reported study outcomes adjusted for discrepancies in baseline characteristics.

Blinding

All studies used reliable, objective measures of outcome for investigating differences in processes of care (blood investigations, x-rays, prescriptions, and admissions) between physician groups; risk of detection bias was low for these outcomes.

However, detection bias for referrals was judged 'unclear' in Murphy 1996 and Gibney 1999 due to the lack of clarity around the definition of referrals and uncertainty as to whether physicians were aware of study outcomes. In Murphy 1996, only those referrals where "a second doctor was formally requested to review a patient and did so" were counted. Hence referral counts could have been lowered for either EPs or GPs by referring patients to non-physician health professionals outside of the hospital setting (i.e. out-patient or community clinics) instead of second doctors. Gibney 1999 did not specify how referrals were defined, and hence this was judged 'unclear'. These concerns did not apply to referral outcomes in Dale 1995, as physicians were unaware of study outcomes and referrals to outpatient clinics, community/general practice clinics, on-call specialists teams and scheduled return visits to the ED were all included (Dale 1997).

Two studies provided self-reported patient satisfaction and health status outcomes (Dale 1996, Murphy 1996); risk of detection bias was judged unclear for these outcomes. In Dale 1995, patient satisfaction and health status were assessed by a standardised telephone interview or postal questionnaire; however it was unclear if the assessors were blinded to the patient allocation. In Murphy 1996, a blinded interviewer assessed patient satisfaction immediately after the consultation, and health status one month later by telephone or postal questionnaire; however, both assessments used non-validated questionnaires. Gibney 1999 did not present any self-reported outcomes.

Performance bias was low in Dale 1995 as neither GPs, EPs or nurses were aware of study objectives or whether any particular primary care session was part of the study sample. In Murphy 1996 and Gibney 1999, it was unclear if personnel were blinded to the study objectives or to the outcomes being assessed. Knowledge of study outcomes (ex. number of prescriptions, investigations or referrals) could have impacted practice patterns.

Incomplete outcome data

Missing data (due to incomplete or missing records) was reported by Dale 1995 and Murphy 1996. Of the 4641 patients included in Dale 1995, records were incomplete for up to 35 (<1%) of patients records. Of the 4684 patients included in Murphy 1996,

83 (2%) of the patients' records could not be located; 33 had been seen by GPs and 50 by EPs. The number of missing records was small relative to the overall sample size. Hence the risk of bias due to incomplete outcome data in these two studies is assessed as low. The risk of bias due to incomplete outcome data was unclear in Gibney 1999 because of limited reporting of outcomes, and no mention of missing data.

Selective reporting

The risk of selective outcome reporting was judged to be low in two studies (Dale 1995, Murphy 1996), where results for all outcomes mentioned in the methods section were reported. Gibney 1999 was a brief report, and it was judged high risk for selective outcome reporting as it is possible that the outcome data reported in the publication did not include all the outcomes measured in the study.

Other potential sources of bias

A potential source of bias in Dale 1995 and Murphy 1996 was the difference in number of hours worked by GPs versus EPs. GPs had limited numbers of shifts per week (range 6 to 9 hours per week across studies) while there were no restrictions on the number of shifts or hours worked by ED staff. This difference in ED work hours and experience could have created a performance bias affecting the number of patients seen, physicians attitudes towards patients and their practice patterns when deciding on investigations, prescriptions, referrals, or admissions.
In Gibney 1999 the level of reporting was poor and the number of hours worked by GPs was also not reported, hence the risk of bias was judged as unclear.

Effects of interventions

See: **Summary of findings for the main comparison**
See Summary of findings and Table 1 *for main outcomes.*
Meta-analysis for process outcomes (diagnostic investigations, admissions and referrals) had very high statistical heterogeneity, with I^2 values greater than 85%, and was therefore discarded.

Comparison 1. Effects of GPs providing care to non-urgent patients in EDs as compared to care provided by EPs on patient wait times:

None of the included studies provided any data for this comparison.

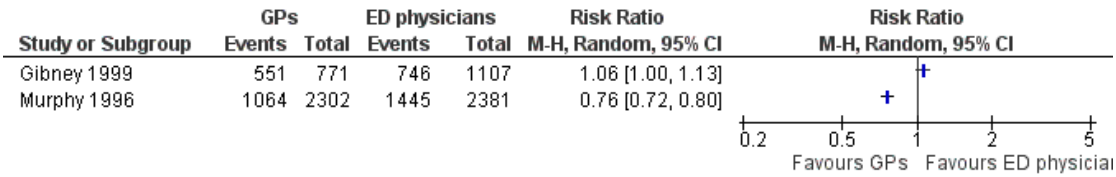
Comparison 2. Effects of GPs providing care to non-urgent patients in EDs as compared to care provided by EPs on resources utilised:

(i) Diagnostic interventions

Any investigations

The proportion of patients for whom "any investigation" was ordered is shown for two studies (Murphy 1996, Gibney 1999) in Analysis 1.1 (Figure 5). The direction of effect in the two studies differed, with results in one study (Murphy 1996) suggesting that GPs ordered less investigations than regular EPs (RR 0.76; 95% CI: 0.72 to 0.80), while the second study (Gibney 1999) did not report a statistically significant effect (RR 1.06; 95% CI: 1.00 to 1.13).

Figure 5. Forest plot of comparison: 1 Comparisons of GPs versus EPs, outcome: 1.1 All investigations.

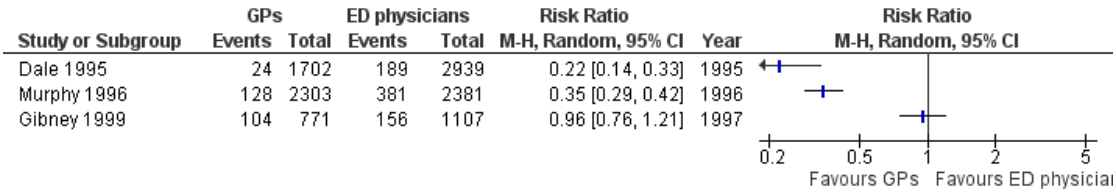


Blood investigations

The results for blood investigations ordered (see Analysis 1.2, Figure 6) suggests that sessional GPs order fewer blood tests than regular EPs, as the direction of effect across all studies was consistent. The size of the effect was similar and statistically significant in Dale 1995 (RR 0.22; 95%CI: 0.14 to 0.33) and Murphy 1996 (RR 0.35; 95% CI: 0.29 to 0.42). In Gibney 1999 the effect size

was smaller and confidence intervals crossed the line of no-effect (RR 0.96; 95% CI: 0.76 to 1.21).

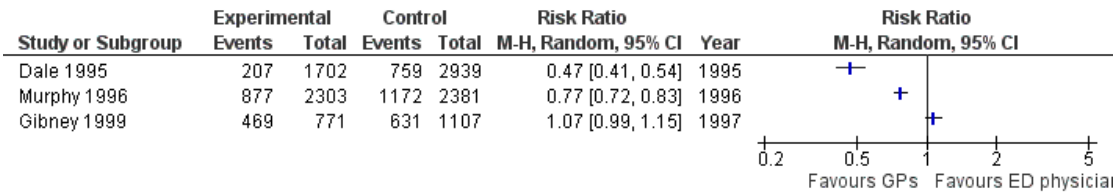
Figure 6. Forest plot of comparison: 1 Comparisons of GPs versus EPs, outcome: 1.2 Blood investigations.



Any x-ray

The results for 'any x-rays' ordered (see Analysis 1.3, Figure 7) showed that GPs ordered statistically significantly less x-rays than EPs in two studies (RR 0.47; 95% CI: 0.41 to 0.54 in Dale 1995, and RR 0.77; 95% CI: 0.72 to 0.83 in Murphy 1996), but data from Gibney 1999 did not support this with a RR of 1.07; 95% CI: 0.99 to 1.15.

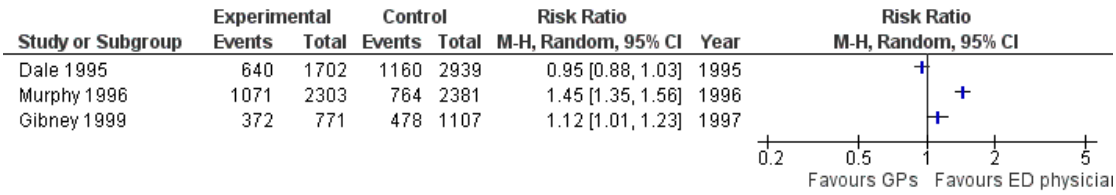
Figure 7. Forest plot of comparison: 1 Comparisons of GPs versus EPs, outcome: 1.3 Any x-ray.



Any prescription (treatments)

As illustrated in Analysis 1.4 (Figure 8), there was no statistically significant difference in prescribing behaviours between sessional GPs and regular EPs in one study: RR 0.95; 95% CI: 0.88 to 1.03 in Dale 1995; and a marginal difference RR 1.12; 95% CI: 1.01 to 1.23 in Gibney 1999. One study (Murphy 1996) showed that GP's prescribed significantly more than EPs (RR 1.45; 95% CI: 1.35 to 1.56).

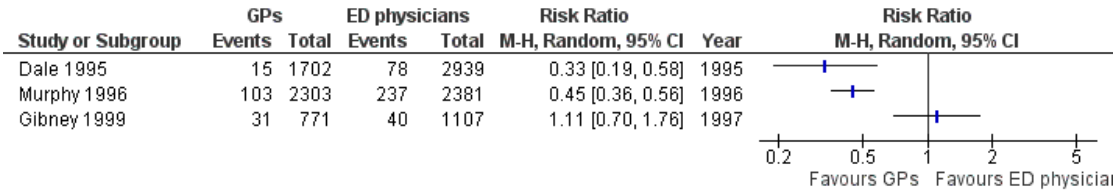
Figure 8. Forest plot of comparison: 1 Comparisons of GPs versus EPs, outcome: 1.4 Any prescription.



(ii) Admissions to hospital

GPs admitted significantly fewer non-urgent patients to hospital than EPs in two studies: RR 0.33; 95% CI: 0.19, 0.58 in Dale 1995; RR 0.45; 95% CI: 0.36 to 0.56 in Murphy 1996. In Gibney 1999, the proportion of admissions made by each type of physician was not statistically significantly different (RR 1.11; 95% CI: 0.70 to 1.76). See Analysis 1.5 (Figure 9).

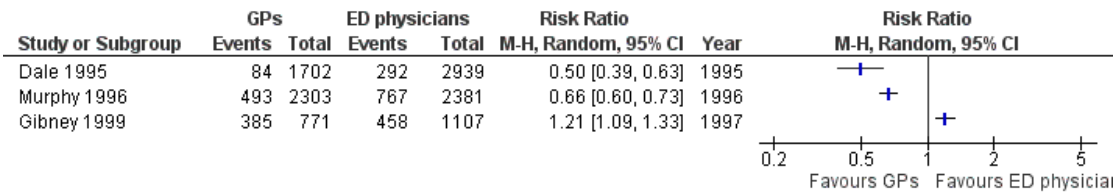
Figure 9. Forest plot of comparison: 1 Comparisons of GPs versus EPs, outcome: 1.5 Admissions.



(iii) Referrals

Two studies found that GPs made statistically significant fewer referrals to hospital specialists or consultants: RR 0.50; 95%CI: 0.39 to 0.63 in Dale 1995; RR 0.66; 95% CI: 0.60 to 0.73 in Murphy 1996. Gibney 1999 reported a statistically significant greater number of referrals made by GPs (RR 1.21; 95% CI 1.09 to 1.33) than EPs. See Analysis 1.6 (Figure 10).

Figure 10. Forest plot of comparison: 1 Comparisons of GPs versus EPs, outcome: 1.6 Referrals.



Comparison 3. Effects of GPs providing care to non-urgent patients in EDs as compared to care provided by EPs on subsequent health care use:

Murphy 1996, found no statistically significant difference in ED re-attendance rate by patients seen by GPs versus EPs: with 17% (95% CI: 15.7% to 18.8%) of patients seen by a GP and 18% (95% CI: 16.3% to 19.5%) of patients seen by an ED physician re-attending the ED for the same problem within 30 days of index visit.

Neither Dale 1995 nor Murphy 1996 reported differences in rates of general practice use across groups. In Murphy 1996, 25% (95% CI: 17.9% to 31.1%) of study patients seen by a GP and 22% (95% CI: 13.7% to 30.4%) seen by an EP attended a general practice for the same complaint within 30 days of their index ED visit. The Dale 1995 study looked at general practice use in the 7-10 days following patients index visit and reported that 20% (95% CI: 14.9% to 25.1%), 18% (95% CI: 13.3 to 22.5%) and 21% (95% CI: 10.5% to 31.7%) of patients seen by GPs, SHOs and registrars respectively consulted a GP or nurse practitioner in that time.

Comparison 4. Effects of GPs providing care to non-urgent patients in EDs as compared to care provided by EPs on costs

Dale 1995 reported that employing GPs to attend to primary care patients in the ED between 10am-9pm saved a total of 60 876 GBP at 1991 costs when admission costs were excluded, and ~150 000 GBP when the cost of admissions was included.

Murphy 1996 provided a limited cost comparison for process variables used by GPs versus regular EPs and estimated a total savings of 95 125 IP (Irish pounds) by employing GPs. It is unclear whether this included the cost of admissions.

Comparison 5. Effects of GPs providing care to non-urgent patients in EDs as compared to care provided by EPs on health outcomes

Only self reported outcome data (i.e. patient satisfaction and health status) were available in two of the included studies. No mortality data were reported.

A sub-sample of patients were administered questionnaires in Dale 1995 (N= 565) and Murphy 1996 (N=435 with 74% response rate). Dale 1995 reported high satisfaction ratings (>71%) amongst the 565 people sampled, with no significant difference across GPs, SHOs and registrars. Murphy 1996 also reported no significant difference in patient satisfaction between GPs or EPs.

The type of physician seen did not have a statistically significant effect on health status scores in Dale 1995 or Murphy 1996. In Dale 1995, self reported health status (n=563) one-week after attending the ED showed that the proportion of patients who were "recovered or improving" was 85.5% of GP patients versus 85.7% of EPs' patients. In Murphy 1996, 83.4% of patients seen by the GP in the ED were "cured" or "improved" compared to 87.4% of patients who saw ED staff one-month after attending the ED.

DISCUSSION

Summary of main results

This review included three non randomised controlled studies evaluating the effectiveness of employing sessional GPs in EDs to provide care for patients with non-urgent problems. No data were available on wait times or total length of stay; two of the primary outcomes for this review.

With regard to admissions and resource utilisation, two studies (Dale 1995; Murphy 1996) found that GPs made fewer admissions to hospital and ordered less blood or x-ray investigations than did regular EPs, while Gibney 1999 reported no significant difference across these same outcomes. There are a few possible explanations for this inconsistency.

That there were fewer x-rays ordered by GPs in both Dale 1995 and Murphy 1996 may reflect differences in case-mix. In Dale 1995, EPs saw a greater proportion of 'injury or poisoning complaints' than GPs, who saw a greater portion of patients presenting with mental disorders and dermatological problems. In Murphy 1996, EPs saw a higher portion of triage 3 ('semi-urgent') patients while GPs saw a higher portion of triage 4 ('delay-acceptable') patients. These results may suggest that particular types of presenting complaints can be directed to GPs working in the ED. The difference in x-ray findings might be explained by differences in provider characteristics (for which data was not available for Gibney 1999) between community and urban EDs which may account for the differences in x-ray ordering behaviour (Wadman 2005). Also, the study population in Gibney 1999 was much smaller (N=1878) than in the studies by Dale 1995 and Murphy 1996 (N >4000), hence greater sampling variation may have contributed to the inconsistency in results. Additional findings reported by Dale 1995 and Murphy 1996 (not available in Gibney 1999) suggest that the type of physician has no effect on re-attendance rates, patients' satisfaction or patients' self-reported health outcomes. With regard to cost, two studies (Dale 1995, Murphy 1996) reported marginal cost savings by employing sessional GPs to manage primary care patients in the EDs of urban-teaching hospitals. The economic evaluations should be interpreted with caution as their validity depends on the reliability of the clinical data (i.e. differences in resource use, admissions, etc., amongst GPs and EPs) gathered within each study. Evidence for safety outcomes (mortality, adverse events, return visits to the ED) was not available.

Overall completeness and applicability of evidence

No studies of primary care professionals other than GPs were identified. There was no evidence around the effects of GPs working in EDs on: waiting times, length of hospital stay or adverse outcomes such as 72-hour re-presentations, admissions within 1-week of discharge or mortality. Treatment data were limited to prescriptions given, and no data were available for medications provided in the ED, overall evidence-based care, procedures or education/counselling provided. Data on patient and provider characteristics were not available for one study (Gibney 1999).

All three included studies were conducted in the UK or Ireland between 1993 - 1999, which may limit the generalizability of results to other countries. Data on the proportion of non-urgent visits to the ED in these studies would be of interest, given the different financial structures in the UK and Ireland at the time the studies were conducted; these data are not available for comparison across all three studies. In the UK's national health system, GP and ED visits are available free of charge. The two Irish studies (Murphy 1996, Gibney 1999), were conducted at a time when the Irish health system was a mixed public (~85%) and private scheme, in which approximately 2/3 of patients paid a fee for GP and ED visits (Murphy 1996). Ireland has since adopted a publicly funded health system with the introduction of the Health Act of 2004. The results from this review may not be applicable in countries with different health care-structures, or even within the UK and Ireland given changes that have occurred in the finance, structure and delivery of primary and emergency care over the last decade.

Two major differences which make meaningful comparisons of EDs across studies and centres challenging are variations in: (1) the type of physicians who normally staff EDs and (2) the triage definitions of 'urgent' and 'non-urgent'. In major urban centres in many countries such as Canada and the United States, consultants in emergency medicine provide ED coverage every hour of every day. In contrast, the majority of the EPs in the included studies were SHOs and registrars, who in North America would be considered trainee doctors and may not necessarily be categorised as 'EPs'. Additionally, the lack of consensus on triage categories and definitions of "non-urgent" primary-care suitable problems, make meaningful comparisons across studies difficult since patients who may classify as "non-urgent" at one centre may be triaged as "urgent" at another.

The two largest included studies (Dale 1995, Murphy 1996) (each N > 4000) were conducted at major urban teaching centres. Their results may not be applicable in other health care settings (for example rural or community hospitals) which are often staffed by GPs. Patient case-mix may also vary between health care settings, which may help explain (in addition to the selection bias), why the results in Gibney 1999, which was conducted at a community hospital, differed consistently across outcomes of the two studies conducted at urban teaching hospitals (Dale 1995, Murphy 1996). Finally, the demographics of patients attending any ED are variable across centres, reflecting local socio-economic factors, health status and accessibility of primary care services. The type and number of non-urgent problems that present to a particular centre will vary, and the results from these studies may not be applicable at EDs which cater to a patient population with a different set of non-urgent problems.

Quality of the evidence

The quality of evidence of the included studies was assessed using the GRADE approach (Guyatt 2008). Few studies were identified, which limits the applicability of the study findings given the wide variation in ED structures and health care systems. The overall strength of evidence was weak, with 'very low' quality of evidence for all outcomes. This was primarily because none of the identified studies were RCTs. The study designs were classified as observational studies in the GRADE software. We recognize that RCTs are costly and difficult to conduct in the busy, unpredictable setting of an ED without encumbering ED staff or limiting patient flow. The studies included in this review were large (total N=11 203) and pragmatically designed to limit risk of bias, however due to the loss of randomisation arising from cross-over of physicians in Dale 1995 and the predictable allocation of patients to EPs or GPs in Murphy 1996 and Gibney 1999 they could not be classified as low risk. The quality of evidence was also downgraded for imprecise or inconsistent effects, as illustrated by the high heterogeneity across studies (I^2 values >86%). The high heterogeneity may have resulted from differences in study design (for example the method of allocation), triage criteria used, health care systems, medical practitioner experience or in outcome measurements (for example 'blood investigations' versus 'haematology' and 'biochemistry'). Finally, reporting bias, due to limited reporting, also lowered the quality of evidence of one study (Gibney 1999). Application of GRADE in this review was challenging as GRADE is designed to assess the quality of a body of evidence for each pooled-outcome in a review. In this review, pooled estimates or meta-analysis for each outcome were not possible because of high heterogeneity across studies; the critical strengths and limitations contributing to quality assessments for each outcome were weighed simultaneously for the three studies, each of which had different allocation designs.

Potential biases in the review process

Interest in integrating primary and emergency care services is high, as suggested by the relatively recent unpublished and secondary research papers on the topic (Cooke 2004, Winters 2009, Carson 2010, Fisher 2010), yet relevant high-quality research was difficult to identify. One potential source of bias in systematic reviews is publication bias. The search strategy was developed with experienced information technologists and was designed to maximize sensitivity (detection of relevant research) at the expense of specificity (excluding irrelevant research). We also hand-searched conference abstracts from emergency medicine conferences from the

last 3 years which should have reduced the likelihood of missing relevant studies. Previous research in this field (Ospina 2006) has demonstrated publication bias (positive results published more often than negative results) and the authors recognize that negative results likely exist. Another potential bias in systematic reviews is selection bias. Efforts were made to avoid selection bias through independent identification of studies for inclusion, data extraction and risk of bias assessment by two or more authors.

Agreements and disagreements with other studies or reviews

Previous reviews of this topic (Roberts 1998, Cooke 2004, Winters 2009, Fisher 2010, Carson 2010) included retrospective and observational study designs. These reviews also reported weak evidence suggesting cost-benefits of employing primary care physicians in the ED, and conflicting evidence on resource utilization with respect to investigations, prescriptions issued or referrals made. None of these reviews provided a formal risk of bias assessment of included studies.

AUTHORS' CONCLUSIONS

Implications for practice

There are few implications for practice based on the current available evidence.

There is very weak evidence in this review to suggest that GPs may use less resources to treat non-urgent patients in the ED than EPs, and thus that employing sessional GPs may provide cost-savings to EDs in this regard. However, it is unclear if less resource utilisation translates into improved outcomes for patients. Furthermore, cost-savings will vary in individual health care settings and may depend, for example, on the magnitude of the salary difference between primary care and ED practitioners and the relative productivity of each.

Non-urgent use of the ED has been hypothesized to contribute to long wait-times and overcrowding in the ED (Liggins 1993, Derlet 2000, Jepson 2001, Carret 2009). There is insufficient evidence in this review for decision makers to evaluate the full impact of employing GPs in the ED to care for non-urgent patients and the resulting effect on wait-times and overcrowding as current research has not addressed length of stay, health outcomes and safety, which are important considerations. Important safety outcomes for which there is no evidence include mortality and re-attendance. Provider satisfaction has not been examined and introducing GPs to the ED may not be viable if, the intervention is not welcome by EPs or GPs are not willing to work in ED settings. In Murphy 1996, three GPs left the study and had to be replaced; the reasons they left were not provided.

It may be noted that the benefit of providing primary care services within the ED may extend beyond cost- and resource- savings, and may be greatest in settings where access to primary care is limited or costly for patients, or a larger proportion of ED visits are for non-urgent problems. For example, additional benefits may arise when primary care and emergency staff work together through the exchange of ideas across disciplines (Chew-Graham 2004).

Implications for research

The three eligible studies identified in this review were conducted more than 15 years ago. There have been no RCTs to date, although the problem of inappropriate ED use and overcrowding appear frequently in the emergency literature. This likely reflects the difficulty of designing and carrying out randomised trials in the busy emergency setting. Factors to consider include an unpredictable work load, that randomisation must be designed so as not to prolong wait times and that health system wide changes may have an impact on the intervention (e.g., pay-for-performance, accountability, additional beds, time targets, etc).

Design

Further research is needed as evidence of resource and cost savings in themselves are insufficient for health authorities to decide whether to employ GPs in the ED. Future studies may wish to investigate whether providing primary care in EDs generates more demand and increases use of EDs for non-urgent problems. The effect on wait times, adverse effects, mortality and patient outcomes is extremely important and has not yet been studied. An additional outcome that is important to consider is use of evidence based care by practitioners.

Future studies should maximize the number of practitioners to reduce the effect of individual practitioners on outcomes. In addition, the methodological quality of the studies designed to evaluate the intervention could be improved by: triaging patients using a standard tool, using concealed allocation to randomise patients to see the EP or GP (for example using a black box from which the patients' chart were selected in the case of Murphy 1996 and Gibney 1999) or by randomising days of service prior to physician allocation (rather than selecting days of service post-hoc). That way, the length of stay, costs and adverse effects of the intervention can be compared. In order to facilitate comparisons across future studies, researchers need to reach a consensus on the definition of 'primary care-suitable problems' tailored to an ED.

Reporting

Adequate reporting of the implementation of the intervention is an additional area that requires attention to allow readers to evaluate the applicability of study findings to their own centres. In addition the lack of consensus on methods of triage across different health care systems means that future studies should provide detailed descriptions of the triage criteria and methods used.

Future studies should also aim to include descriptions of the:

- Proportion of ED attenders classified as *non-urgent* to allow comparisons across studies
- Patient characteristics for all groups
- Hospital characteristics (catchment size, type [teaching or community], location)
- Medical provider characteristics (age, experience, level of expertise)

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

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

Dale 1995

Methods	**Design:** non-randomised controlled study **Timeline:** 1 June 1989 - 31 May 1990 (not bank holidays or first 2 weeks of August, February); **Duration**:1 year **Triage:** Patients categorized by trained nurses based on **perceived need for care** as either "primary care" or "accident and emergency". **Data collection**: Data on process and outcome variables (doctor's use of radiology, haematology, chemical pathology and microbiology investigations, items prescribed), referral and discharge decisions were obtained from hospital records and consultation record forms Patient satisfaction and health status were assessed through a simple questionnaire (administered by phone or through post) to assess (1) self-reported recovery in 7-10 days subsequent to attending ED and (2) health seeking behaviour during this period, including re-attendance at ED or attendance at own GP surgery
Participants	**Intervention group:** N =8 GPs (11 GP's applied, 6 were appointed, 2 left during study and were replaced) **Control group:** N= 31 EPs (27 senior house officers, 3 registrars and 1 senior registrar) **Provider characteristics**: none reported **Patients:** New ED attendees with '"primary care" suitable problems **Total no of patients**: N=4641; Intervention group: n=1702 patients seen by GPs; Control group: n=2939 patients seen by EPs **Patient characteristics:** Sex: 47.4% female Age: 41.7% 17-30 years Duration of complaints: 62.2% problems >24 hrs; 20.8% had previously seen a GP Most common diagnoses: injury & poisoning (44.4%),musculoskeletal diseases (13.7%) , non-specific symptoms and signs (7.0%) Patient characteristics for control and intervention groups not available **Setting:** **Hospital**: one, King's College Hospital **Country:** United Kingdom **Hospital characteristics (1990 figures):** Beds: n/a Teaching hospital, inner city, 'multiethnic, socially deprived', Yearly attendance: 70 000 Yearly re-attendance: n/a
Interventions	**Intervention**: sessional GPs providing care for non-urgent patients in the ED **Control**: regular EPs providing care for non-urgent patients in the ED Patients referred by GPs were excluded.

Outcomes	1. Investigations: laboratory investigations - chemistry, haematology, microbiology; x-rays; ECGs 2. Prescriptions 3. Referrals to: community or GP; on-call specialist team; outpatient clinic 4. ED re-attendance 5. Patient satisfaction, recovery (i.e. health status in 7-10 days after attending the ED) (questionnaire/survey data); 6. Costs
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Quote: "General practitioners and accident and emergency medical staff were considered as two groups, and each group was allocated two or three weekday sessions running from 1000 to 1300 and 1400 to 1700, one weekday evening session from 1800 to 2100, and one weekend daytime session for each week during the study period.. ...**weekly rosters stipulated a named doctor with responsibility for primary care patients for every three hour session"** and "**a random sample of sessions stratified by time of day and day of week was determined by using a table of random numbers.** ...Hence, 8-10 sessions were sampled each week for a total of 48 weeks. The sample of sessions allocated to accident and emergency staff was the same as those described in the accompanying paper." See P.1, Col. 2, Para. 4. Comment: Primary care sessions selected for inclusion in study were randomly selected using a random number table, however allocation of physicians to selected sessions was not random, but depended on physician availability and scheduling. Also, since nurses performing triage knew if a GP or a EP was seeing the 'non-urgent' cases, this could affect what type of patients the physician in charge of providing care for the 'non-urgent' patient group actually saw (i.e. more emergency type patients if an EP, and less so if a GP)

Dale 1995 (*Continued*)

Allocation concealment (selection bias)	High risk	Quote: "Patients were unaware of their triage status or the grade and specialty of their doctor". See p.1, Col.5, Para.5 Comment: While patients were unaware of whether they were in the intervention (GP) or control (A&E staff) groups, this did not provide adequate allocation concealment in this study. The type of physician providing care for each primary care session was open and not concealed Importantly, triage nurses were not blinded to the grade and speciality of the physician in charge for providing care for 'non-urgent ' patients, which could have affected the triage and therefore also what type of patients the physician actually saw (i.e. more emergency type patients if an EP, and less so if a GP)
Incomplete outcome data (attrition bias) All outcomes	Low risk	Quote: "Not all records were complete" See p.2, Col.2, Para.2 Comment: Unclear whether missing data was predominantly from control or intervention group, or approximately equal across groups. Given binary outcomes and large samples, proportion of missing data probably less than effect size and low risk of bias
Selective reporting (reporting bias)	Low risk	All outcomes mentioned in methods section were reported.
Other bias	High risk	Quote: "General practitioners worked sessions of only three hours in accident and emergency, compared with senior house officers' and registrars' shifts of up to 11 hours. Duration of shift may affect attitudes to patient care and influence the threshold of initiating referral or investigation." See p.4, Col.2, Para.1 Comments: General practitioners and EPs did not work equal numbers of hours in ED; this imbalance in experience and numbers of patients seen between providers may bias results
Baseline outcome measures similar	Unclear risk	No baseline measure of outcome reported.

Baseline (provider) characteristics similar	Unclear risk	Quote: in recruiting GPs, "preference was given, firstly to those who had recently completed training (that is, general practitioners registered for similar numbers of years to the accident and emergency doctors) and, secondly, to those with flexible hours of availability". See p.1, Col.2, Para. 3 Comment: This does not tell us what the actual provider characteristics were, only what was aimed for in the recruitment process. Also, no data is presented
Baseline (patient) characteristics similar	High risk	Quote: "Two variables - age and an injury related diagnosis - were found to vary significantly with type of doctor seen. In addition, other variables (such as diagnosis of a mental disorder or a disease of the skin) varied significantly but had small effect sizes. " See p.3, Col.2, Para.4 and table VI
Knowledge of allocated intervention adequate (Process variables)	Low risk	Unclear if outcomes were assessed blindly but process variables (laboratory and x-ray investigations, prescriptions, referrals, admissions) were objective Referrals were defined in the primary author's PhD thesis as out-patient, on-call team and hospital admissions were all counted as referrals
Knowledge of allocated interventions adequate (Patient satisfaction, health status)	Unclear risk	Questionnaires were administered by standardized telephone interview or post within 7-10 days of patients' index visit: "We interviewed the patients again 7-10 days later by telephone (or sent them a postal questionnaire if they lacked a telephone) about their satisfaction with their assessment and treatment in the department, the extent of their recovery, and the health care they required after attending the department. Responses to questions of satisfaction were recorded on five point Lkert scales, ranging from very satisfied to very dissatisfied." See p.1, Col.2, Para.3 (Dale 1996) Comment: Self-reported data and un validated questionnaire (as per Dale thesis, no validated questionnaires were available at

		time of study). Unclear in interviewer was blinded
Blinding of participants and personnel (performance bias)	Low risk	Quote: (1) "Neither the general practitioners nor the accident and emergency doctors or nurses were informed about the study objectives or whether any particular session was part of the study sample." See p. 1, Col 2, Para. 4 (2) Patients were unaware of their triage status or the grade and speciality of their doctor." See p.427, Col 2, Para. 5 Comments: All personnel (GPs, EPs and nurses) were blinded to the study objectives and whether any particular session was part of the study sample, and the patients were unaware which type of doctor they were seen by
Blinding of outcome assessment (detection bias) (Process variables)	Low risk	Quote: "All doctors...were asked to complete a consultation record form for each patient seen...Doctors remained blind to how data from these forms would be analysed." See p.2, Col.1, Para.3 Comments: Outcomes were objective and physicians were unaware of what data was being collected for the study. It is unclear if researchers knew which physician saw patients
Blinding of outcome assessment (detection bias) (Patient satisfaction, health status)	Unclear risk	Unclear if outcome assessors for patient satisfaction and health status were blinded
Adequately protected against contamination	High risk	Quote: (1) "Although the intention was that all primary care patients would be treated by the allocated doctor, this did not always occur. Firstly, at times when the primary care workload was excessive, other doctors were directed by the nurse performing triage to treat primary care patients to prevent unacceptably long waiting periods from occurring; secondly, registrars in particular were often interrupted from completing primary care sessions by departmental circumstances (such as responding to patients with urgent or life threatening needs or providing advice or supervision to senior

		house officers). Hence patients were sometimes attended by a non-allocated doctor, both during sessions originally allocated to a general practitioner and during those allocated to another member of accident and emergency staff." See p.2, Col.1, Para.2 (2) "Since this breakdown of randomisation was not always clearly documented, data for all recorded primary care consultations occurring during the selected sessions were included in the sample, and data on patients were regrouped according to the type of doctor actually seen. The loss of randomisation was allowed for by including confounding factors in the analysis of the data." See p.2, Col.1, Para.2

Gibney 1999

Methods	**Design**: non-randomised controlled study **Time**: March 1996 - September1996; **Duration**:7 months **Triage**: Patients categorized by receptionists with no formal training into "urgent" and "non-urgent" **Data collection:** Process data were collected from a review of written patient records
Participants	**Intervention group:** N = 3 GPs **Control group: EPs**: N= 8 EPs (1 consultant, 2 registrars, 5 senior house officers) **Provider characteristics**: none reported **Patients:** All 'non-urgent' and non-ambulance patients attending the ED, ambulance patients were excluded **Total number of patients:** n=1878; Intervention group: n= 771 patients seen by GPs; Control group: n=1107 patients seen by EPs **Patient characteristics**: data no longer available. **Setting:** **Hospital:** one, James Connolly Memorial Hospital **Country**: Ireland **Hospital characteristics (1996 figures)**: Beds: 336, small district hospital, urban/rural mix, Yearly attendance: 25 047 Yearly re-attendance: 8213
Interventions	**Intervention**: sessional GPs providing care for non-urgent patients in the ED **Control:** regular EPs providing care for non-urgent patients in the ED (when GP present at the ED) Patients referred by GPs included.

Outcomes	1. Investigations: blood, x-ray, any 2. Referrals 3. Prescriptions 4. Admissions
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Quote: Allocation of patients "to either GP or A&E staff was the same as our previous study (Murphy 1996) and was performed according to time of registration." See p.1, Col.2, Para.5 Comment: Sequence generation was non-random; patients were seen in temporal order and allocation to provider was not necessarily consecutive, depending on the length of previous consultations
Allocation concealment (selection bias)	High risk	Quote: "An unstructured receptionist-based triage system divides all non-ambulance patients into two categories: 'urgent' and 'non-urgent'." See p.1, Col.2, Para.3 Comment: Patient allocation occurred as individuals entered the study (by attending the ED). It is unclear how physician allocation to primary care sessions was performed It is not specified whether nurses performing triage were blinded; nurses knowledge of whether a GP or a EP was working could have affected triage and the type of patients that physician working in primary care sessions saw (i.e. more emergency type of patients if an EP, and less so an EP)
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Not specified in the paper.
Selective reporting (reporting bias)	High risk	All outcomes mentioned in text reported in results, however the study was designed and carried out by same author-group as Murphy 1996 and fewer outcomes are reported without explanation.

Gibney 1999 (*Continued*)

Other bias	Unclear risk	It is probable that GPs and EPs did not work equal numbers of hours in ED; this imbalance in experience and numbers of patients seen between providers may bias results
Baseline outcome measures similar	Unclear risk	No baseline measure of outcome reported.
Baseline (provider) characteristics similar	Unclear risk	No provider characteristics reported.
Baseline (patient) characteristics similar	Unclear risk	Quote: "There were no differences in age, sex, socio-economic status, registration with a GP or type of presenting complaint between patients seen by a GP or usual A& E staff." See p.1, Col.2, Para.6 Comment: No data on patient characteristics were reported, hence we cannot corroborate that the patient groups seen by GPs or EPs were comparable in terms of duration of complaints, diagnoses etc
Knowledge of allocated intervention adequate (Process variables)	Low risk	The outcomes are objective.
Blinding of participants and personnel (performance bias)	Unclear risk	Not specified in the paper.
Blinding of outcome assessment (detection bias) (Process variables)	Unclear risk	Unclear if outcomes were assessed blindly but process variables (laboratory and x-ray investigations, prescriptions, admissions) were objective A definition of what constituted referrals in the study was not provided; if only some types of referrals (e.g., to on-call physicians) were counted, this would not objectively account for the total referrals made (e.g., to non-physician health professionals) by both intervention and control groups
Adequately protected against contamination	High risk	Quote: "Study enrolment only occurred when both GPs and usual A&E staff were on duty together." See p.1, Col.2, Para.5 Comments: GP's and EPs worked simultaneously in primary care sessions and overlap and contamination between groups was possible

Murphy 1996

Methods	**Design:** non-randomised study **Time:** August 1993 - October 1994; **Duration**:15 months **Triage:** Patients triaged by trained nurses based on physiological criteria as (1) life threatening, (2) urgent, (3) semi-urgent or (4) delay acceptable **Data collection:** Process information (investigations, referrals, prescriptions, etc) were collected from hospital records The numbers of patients re-attending the ED within one month of the index visit was determined using the hospital's mainframe computer Patient satisfaction was assessed immediately by a blinded interviewer using the consultation satisfaction questionnaire. Health status was determined one month after the initial consultation by means of a simple questionnaire (four questions) completed by telephone or letter Marginal (materials and disposables) and total (marginal plus all staff) costs were determined in conjunction with the hospital's finance department and x ray and laboratory staff. Costs were calculated for the following: full blood counts; measurements of blood urea and plasma electrolyte concentrations, plasma glucose concentration, and serum amylase activity; sequential multiple analysis with computer (SMAC); and chest, limb, skull, spine, and abdominal radiographs. Based on the hospital admission profile an estimate of the average cost per admission was also obtained
Participants	**Intervention group:** N=5 GPs Age (median): 32 years Years since registration (median): 7 years **Control group:** N= 13 EPs (1 consultant, 2 registrars, 10 senior house officers) Age (median): 26 years **Patients: n**ew ED attendees triaged as "semi-urgent" or "delay acceptable" **Total no of patients**: n=4684; Intervention group: n=2303 patients seen by GPs; Control group: n=2381 patients seen by EPs **Patient characteristics:** Sex: 41.4% female Age**:** median 28-34years Years since registration (median): 6 months Duration of complaints: 44% problems >24 hrs; 92.6% registered with GP's (unclear how many saw GP prior to attending) Most common diagnoses: musculoskeletal (50.9%), skin complaints (19.0%) and neurological (8.8%) **Setting** : **Hospital:** one, St James' Hospital **Country:** Ireland **Hospital characteristics (1992 figures):** Beds: 490, catchment 219 300 people Major teaching hospital Yearly attendance: 40 159 Yearly re-attendance: 7589
Interventions	**Intervention:** sessional GPs providing care for non-urgent patients at hospital ED **Control:** regular EPs providing care for non-urgent patients when GP present in department

Murphy 1996 (*Continued*)

	Patients referred by GPs (20%) were excluded.
Outcomes	1. Investigations: blood, x-ray, any 2. Referrals 3. Prescription 4. Disposal to: community; hospital; outpatient clinic; 5. admissions; 6. Re-attendance within 1 month; 2 years 7. Patient satisfaction 8. Health status
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Quote: "Randomisation of patients to the general practitioner or accident and emergency staff depended on time of registration. Once patients were registered their charts were divided according to triage category on to four separate shelves and then placed in line by strict temporal order. Doctors took the first chart on the triage 3 shelf and continued doing so until the shelf was empty. They then moved to the triage 4 shelf." See p.2, Col.1, Para.3 Comment: Sequence generation was non-random; patients were seen in temporal order and allocation to provider was not necessarily consecutive, depending on the length of previous consultations. Although a research nurse was employed to ensure adherence to the temporal order, this open allocation method could be problematic if the triage information recorded on chart influences physicians choice to accept or reject a patient (by waiting for the other physician to take the top chart). For example, GPs investigated fewer semi-urgent (triage 3) and more delay-acceptable (triage 4) patients than EPs (GPs saw 1516 vs . See p.3, Table 1: • GPs saw 1516 and EPs 1837 Triage 3 patients. • GPs saw 787 and EPs 544 Triage 4 patients

Allocation concealment (selection bias)	Unclear risk	Quote: "General practitioners... were dressed similarly to the usual staff and patients were unaware that they were being seen by a general practitioner" See p.2, Col. 1, Para.2-3 Comment: Patient allocation occurred as individuals entered the study (by attending the ED) and was carried out by a study researcher and enforced by the triage nursing team. It is unclear whether the same person conducted both steps of the randomisation process. Physicians were not blinded to the triage category of the patients being seen, however patients were probably unaware of the type of physician treating them It is unclear how physician allocation to primary care sessions was performed
Incomplete outcome data (attrition bias) All outcomes	Low risk	Quote: "The hospital's computer could not locate 83 (2%) of the 4684 patients enrolled in the study. Thirty three had been seen by the general practitioners and fifty by the usual accident and emergency staff. " See p.4, Col.2, Para.4 Comment: There were similar numbers of missing records across the two groups and a relatively small portion of data was missing, hence probably low risk of bias
Selective reporting (reporting bias)	Low risk	All outcomes mentioned in the text were reported in the results
Other bias	Unclear risk	Quote: Each GP "worked two four hour sessions a week, managing non-emergency patients". See p.2, Col.1, Para.2 GPs and EPs did not work equal numbers of hours in the ED; this imbalance in experience and numbers of patients seen between providers may bias results
Baseline outcome measures similar	Unclear risk	No baseline measure of outcome reported.
Baseline (provider) characteristics similar	High risk	The median age and time since registration of GPs and EPs was not equal. The median age of the 5 GPs employed during the project was 32 years compared with 26 years for EPs. Similarly, the median time

		since full registration was seven years for GPs and 6 months for EPs. See p.3, Col.2, Para.3 This difference in experience between the groups may bias the study outcomes
Baseline (patient) characteristics similar	High risk	Quote: (1) "There were significant differences (in presenting complaints)....between (triage 3) patients seen by the general practitioners and those seen by the usual accident and emergency staff". See p.4, Table 3 (2) "There were no differences between triage 4 patients seen by general practitioners and those seen by the usual accident and emergency staff". See p.3, Col.2, Para.5 Comment: High risk of bias because patient' diagnosis in control and intervention groups were not equal
Knowledge of allocated intervention adequate (Process variables)	Low risk	Unclear if outcomes were assessed blindly but process variables (laboratory and x-ray investigations, prescriptions, referrals, admissions) were objective (Referrals were "when a second doctor was formally requested to review a patient and did so" p.2, Col.2, para.2)
Knowledge of allocated interventions adequate (Patient satisfaction, health status)	Unclear risk	Quote: (1) "Patient satisfaction was assessed immediately by a blinded interviewer using the consultation satisfaction questionnaire. " See p.2, Col.2, Para.4 (2) "Health status was determined after one month by means of a simple questionnaire completed by telephone or letter" Patient satisfaction was assessed blindly. Unclear if health status was assessed blindly. See p.2, Col.2, Para.4 Comment: Self-reported data and unclear if questionnaires were validated or if health status was assessed blindly
Blinding of participants and personnel (performance bias)	Unclear risk	Quote: "General practitioners...had access to the same facilities as the usual medical staff. They were dressed similarly to the usual staff and patients were unaware that they were being seen by a general practitioner"

		Comments: Patients were unaware of which type of physician they were seeing It is unclear whether medical practitioners were aware of the study objectives. Knowledge of study objectives may have affected performance (ex. consciously choosing to order less investigations or make more referrals to the community rather than to a second doctor)
Blinding of outcome assessment (detection bias) (Process variables)	Unclear risk	It is unclear if outcomes were assessed blindly, but most process measures were objective items such as the number of investigations ordered, prescriptions given and admissions made Referrals were only counted in the study if "a second doctor was formally requested to review a patient and did so" (See p. 2, Col.2, Para.1). Hence any referrals to community or non-physician health care providers (ex. community nurses, social workers, mental health professionals) were excluded and detection bias could have been introduced if physicians were aware of the study definition or outcome; hence the risk of bias was judged unclear
Blinding of outcome assessment (detection bias) (Patient satisfaction, health status)	Unclear risk	Quotes: (1) "Patient satisfaction was assessed immediately by a blinded interviewer using the consultation satisfaction questionnaire. " See p.2, Col.2, Para.4 (2) "Health status was determined after one month by means of a simple questionnaire completed by telephone or letter" See p.2, Col.2, Para.4 Comment: Satisfaction assessment was blinded but it is unclear if health status assessments were blinded
Adequately protected against contamination	Unclear risk	Unclear. GP's and EPs worked simultaneously in primary care sessions and overlap and contamination between groups was possible. See p.2, Col.2, Para.2, 4-6

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

Study	Reason for exclusion
Bache 2001	Descriptive study.
Beales 1997	Description of an informal intervention.
Beales 1997a	Observational study.
Boeke 2010	Uncontrolled before and after study.
Bosmans 2012	Uncontrolled before and after study.
Byrne 2000	No effectiveness data - satisfaction is the only outcome.
Chew-Graham 2004	Qualitative study.
Cole 2006	Descriptive study.
Combs 2006	Description of an ineligible intervention - establishment of a fast-track unit staffed by emergency staff
Cooper 2002	Aims to develop methods and tools to measure quality of ENP. Ineligible intervention - emergency nurses not primary care professionals
Dale 1995a	Observational study.
Dale 1996b	Commentary.
Dealey 2002	Review of emergency nurse practitioner roles.
Dolan 1999	Descriptive study
Fry 2007	Descriptive study.
Fry 2011	Observational study.
Head 1988	Descriptive study.
Heltoft 2009	Descriptive study of a nurse led emergency clinic. No intervention or comparison
Henne 1988	Descriptive study.
Hughes 2010	Qualitative study of attitudes of practitioners toward nurse-physician collaboration
James 1989	Observational study.
Jennings 2008	Inappropriate study design.

(*Continued*)

Jennings 2009	Qualitative evaluation of patient satisfaction with either ED or NP care
Jimenez 2005	Non-randomised study comparing period with GP to period without GP (no pre-intervention data)
Kavanagh 2003	Qualitative study (questionnaire).
Mabrook 1998	Observational study.
Martin 2005	Uncontrolled before and after study.
Maurice 2001	Descriptive study.
McCaig 1998	Analysis of survey data.
Ong 2007	Qualitative study (questionnaire), no effectiveness data.
Pardee 1994	Descriptive study. Provides a qualitative commentary on practical aspects of implementing a collaborative care model
Pearce 1993	Descriptive study.
Read 1992	Qualitative (postal) survey.
Rhee 1995	No effectiveness data - satisfaction is the only outcome.
Sakr 1999	Ineligible intervention - nurses who already worked in ED, not primary care
Sakr 2003	Observational study
Steiner 2009	Addition of a 'broad-scope' nurse practitioner to the ED team but no comparison with care provided by a primary care professional
Tsai 2009	Uncontrolled before-after study.
Tye 2000	Case studies.
van der Linden 2010	Compares emergency nurses and physicians, no primary care professionals
Ward 1996	Qualitative questionnaire survey.
Wingert 1998	Descriptive study.

DATA AND ANALYSES

Comparison 1. Comparions of GPs versus EPs

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 All investigations	2		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
2 Blood investigations	3		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
3 Any x-ray	3		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
4 Any prescription	3		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
5 Admissions	3		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
6 Referrals	3		Risk Ratio (M-H, Random, 95% CI)	Totals not selected

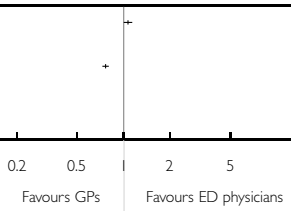
Analysis 1.1. Comparison 1 Comparions of GPs versus EPs, Outcome 1 All investigations.

Review: Primary care professionals providing non-urgent care in hospital emergency departments

Comparison: 1 Comparions of GPs versus EPs

Outcome: 1 All investigations

Study or subgroup	GPs n/N	ED physicians n/N	Risk Ratio M-H,Random,95% CI	Risk Ratio M-H,Random,95% CI
Gibney 1999	551/771	746/1107		1.06 [1.00, 1.13]
Murphy 1996	1064/2302	1445/2381		0.76 [0.72, 0.80]



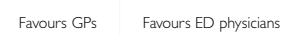
Analysis 1.2. Comparison 1 Comparions of GPs versus EPs, Outcome 2 Blood investigations.

Review: Primary care professionals providing non-urgent care in hospital emergency departments

Comparison: 1 Comparions of GPs versus EPs

Outcome: 2 Blood investigations

Study or subgroup	GPs n/N	ED physicians n/N	Risk Ratio M-H,Random,95% CI	Risk Ratio M-H,Random,95% CI
Dale 1995	24/1702	189/2939		0.22 [0.14, 0.33]
Murphy 1996	128/2303	381/2381		0.35 [0.29, 0.42]
Gibney 1999	104/771	156/1107		0.96 [0.76, 1.21]



Analysis 1.3. Comparison 1 Comparions of GPs versus EPs, Outcome 3 Any x-ray.

Review: Primary care professionals providing non-urgent care in hospital emergency departments

Comparison: 1 Comparions of GPs versus EPs

Outcome: 3 Any x-ray

Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Risk Ratio M-H,Random,95% CI
Dale 1995	207/1702	759/2939		0.47 [0.41, 0.54]
Murphy 1996	877/2303	1172/2381		0.77 [0.72, 0.83]
Gibney 1999	469/771	631/1107		1.07 [0.99, 1.15]

Analysis 1.4. Comparison 1 Comparions of GPs versus EPs, Outcome 4 Any prescription.

Review: Primary care professionals providing non-urgent care in hospital emergency departments

Comparison: 1 Comparions of GPs versus EPs

Outcome: 4 Any prescription

Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Random,95% CI	Risk Ratio M-H,Random,95% CI
Dale 1995	640/1702	1160/2939		0.95 [0.88, 1.03]
Murphy 1996	1071/2303	764/2381		1.45 [1.35, 1.56]
Gibney 1999	372/771	478/1107		1.12 [1.01, 1.23]

Analysis 1.5. Comparison 1 Comparions of GPs versus EPs, Outcome 5 Admissions.

Review: Primary care professionals providing non-urgent care in hospital emergency departments

Comparison: 1 Comparions of GPs versus EPs

Outcome: 5 Admissions

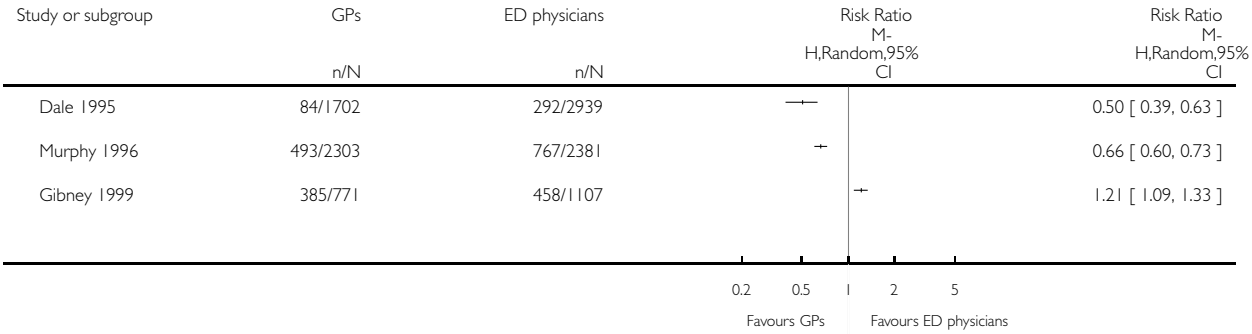
Study or subgroup	GPs n/N	ED physicians n/N	Risk Ratio M-H,Random,95% CI	Risk Ratio M-H,Random,95% CI
Dale 1995	15/1702	78/2939		0.33 [0.19, 0.58]
Murphy 1996	103/2303	237/2381		0.45 [0.36, 0.56]
Gibney 1999	31/771	40/1107		1.11 [0.70, 1.76]

Analysis 1.6. Comparison 1 Comparions of GPs versus EPs, Outcome 6 Referrals.

Review: Primary care professionals providing non-urgent care in hospital emergency departments

Comparison: 1 Comparions of GPs versus EPs

Outcome: 6 Referrals



ADDITIONAL TABLES

Table 1. Results Summary

	Dale 1995 (N=4641)	Murphy 1996 (N=4684)	Gibney 1999 (N=1878)
Blood investigations ordered	RR 0.22; 95%CI: 0.14 to 0.33	RR 0.35; 95%CI 0.29 to 0.42	RR 0.96 95%CI 0.76 to 1.2
Xrays ordered	RR 0.47; 95%CI 0.41 to 0.54	RR 0.77 95% CI 0.72 to 0.83	RR 1.07 95%CI 0.99 to 1.15
Admissions	RR 0.33; 95% CI 0.19 to 0.58	RR 0.45; 95%CI 0.36 to 0.56	RR 1.11; 95% CI 0.70 to 1.76
Referrals to specialists	RR 0.50; 95%CI 0.39 to 0.63	RR 0.66; 95%CI 0.60 to 0.73	RR 1.21 95%CI 1.09 to 1.33
Prescriptions	RR 0.95; 95%CI 0.88 to 1.03	RR 1.45; 95%CI 1.35 to 1.56	RR 1.12; 95% CI 1.01 to 1.23

APPENDICES

Appendix 1. MEDLINE (Ovid) Search Strategy

Medline (Ovid) (1950 -): Search run March 2012

1	Emergency Medical Services/	28640
2	Emergency Service, Hospital/ or Trauma centers/	41338
3	Triage/	6941
4	(emergency adj2 (care or healthcare or department? or unit or units or room? or treatment?)).ti,ab	58610
5	("accident and emergency" or "accident & emergency" or emergency service?).ti,ab	7116
6	(trauma adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab	8264
7	(triage adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab	313
8	(emergency adj2 visit?).ti,ab.	4349
9	(urgent adj2 (care or healthcare or health care)).ti,ab.	985
10	((semiurgent or semi-urgent or nonemergen$ or non-emergen$) adj2 (treatment? or care or visit?)).ti,ab	193
11	((emergency or non-emergency or nonemergency or urgent or non-urgent or nonurgent or semi-urgent or semiurgent) adj2 patient?).ti,ab	7007
12	or/1-11	114503
13	general practitioners/ or physicians, family/ or physicians, primary care/	15293
14	allied health personnel/ or community health aides/ or nurses' aides/ or psychiatric aides/ or pharmacists' aides/ or physician assistants/ or ophthalmic assistants/ or pediatric assistants/	19897
15	Nurse Practitioners/	14259
16	After-Hours Care/	744
17	((general or family) adj3 (practitioner? or physician? or doctor?)).ti,ab	52883

(*Continued*)

18	(nurse practitioner? or nurse specialist?).ti,ab.	9483
19	("out of hours" or after hours).ti,ab.	1693
20	or/13-19	99369
21	12 and 20	4492
22	((community or primary health$ or primary care) adj2 (nurse or nurses or nursing staff or nursing personnel$)).ti,ab. and (Emergency Service, Hospital/ or Trauma centers/)	23
23	Community.ti,hw. and (Emergency Service, Hospital/ or Trauma centers/)	1456
24	21 or 22 or 23 [Topic search]	5860
25	randomized controlled trial.pt.	322037
26	controlled clinical trial.pt.	83702
27	randomized.ab.	238038
28	placebo.ab.	133857
29	clinical trials as topic/	158353
30	randomly.ab.	175052
31	trial.ti.	101834
32	intervention*.ti.	64321
33	(intervention* adj6 (clinician* or collaborat* or community or complex or DESIGN* or doctor* or educational or family doctor* or family physician* or family practitioner* or financial or GP or general practice* or hospital* or impact* or improv* or individuali?e* or individuali?ing or interdisciplin* or multicomponent or multi-component or multidisciplin* or multi-disciplin* or multifacet* or multi-facet* or multimodal* or multi-modal* or personali?e* or personali?ing or pharmacies or pharmacist* or pharmacy or physician* or practitioner* or prescrib* or prescription* or primary care or professional* or provider* or regulatory or regulatory or tailor* or target* or team* or usual care)).ab	84561
34	(collaborativ* or collaboration* or tailored or personali?ed).ti,ab	84440

(*Continued*)

35	(exp hospitals/ or exp Hospitalization/ or exp Patients/ or exp Nurses/ or exp Nursing/) and (study.ti. or evaluation studies as topic/)	32901
36	demonstration project*.ti,ab.	1744
37	(pre-post or "pre test*" or pretest* or posttest* or "post test*" or (pre adj5 post)).ti,ab	51598
38	(pre-workshop or post-workshop or (before adj3 workshop) or (after adj3 workshop)).ti,ab	464
39	((study adj3 aim?) or "our study").ab.	387917
40	(before adj10 (after or during)).ti,ab.	311318
41	("quasi-experiment*" or quasiexperiment* or "quasi random*" or quasirandom* or "quasi control*" or quasicontrol* or ((quasi* or experimental) adj3 (method* or study or trial or design*))).ti,ab,hw	86214
42	("time series" adj2 interrupt*).ti,ab,hw.	676
43	(time points adj3 (over or multiple or three or four or five or six or seven or eight or nine or ten or eleven or twelve or month* or hour? or day? or "more than")).ab	6705
44	pilot.ti.	31477
45	Pilot projects/	69364
46	clinical trial.pt.	467394
47	multicenter study.pt.	139780
48	(multicentre or multicenter or multi-centre or multi-center).ti	23501
49	random*.ti,ab. or controlled.ti.	624169
50	(control adj3 (area or cohort? or compar? or condition or group? or intervention? or participant? or study)).ab	325304
51	25 or 26 or 27 or 28 or 29 or 30 or 31 or 32 or 33 or 34 or 35 or 36 or 37 or 38 or 39 or 40 or 41 or 42 or 43 or 44 or 45 or 46 or 47 or 48 or 49 or 50 [RCT Filter (lines 25-31) & EPOC Methodological Filter version 2.2 (lines 32-50)]	2230052

(*Continued*)

52	exp animals/ not humans/	3682001
53	”comment on“.cm. or systematic review.ti. or literature review.ti. or editorial.pt. or letter.pt. or meta-analysis.pt. or news.pt. or review.pt	2957303
54	52 or 53	6467134
55	51 not 54	1716712
56	24 and 55	1479

Medline (Ovid) (1950 -): Search run April 2011

1. Emergency Medical Services/
2. Emergency Service, Hospital/ or Trauma centers/
3. Triage/
4. (emergency adj2 (care or healthcare or department? or unit or units or room? or treatment?)).ti,ab.
5. (”accident and emergency“ or ”accident & emergency“ or emergency service?).ti,ab.
6. (trauma adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab.
7. (triage adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab.
8. (emergency adj2 visit?).ti,ab.
9. (urgent adj2 (care or healthcare or health care)).ti,ab.
10. ((semiurgent or semi-urgent or nonemergen$ or non-emergen$) adj2 (treatment? or care or visit?)).ti,ab.
11. ((emergency or non-emergency or nonemergency or urgent or non-urgent or nonurgent or semi-urgent or semiurgent) adj2 patient?).ti,ab.
12. or/1-11
13. general practitioners/ or physicians, family/ or physicians, primary care/
14. allied health personnel/ or community health aides/ or nurses’ aides/ or psychiatric aides/ or pharmacists’ aides/ or physician assistants/ or ophthalmic assistants/ or pediatric assistants/
15. Nurse Practitioners/
16. After-Hours Care/
17. ((general or family) adj3 (practitioner? or physician? or doctor?)).ti,ab.
18. (nurse practitioner? or nurse specialist?).ti,ab.
19. (”out of hours“ or after hours).ti,ab.
20. or/13-19
21. 12 and 20
22. ((community or primary health$ or primary care) adj2 (nurse or nurses or nursing staff or nursing personnel$)).ti,ab. and (Emergency Service, Hospital/ or Trauma centers/)
23. Community.ti,hw. and (Emergency Service, Hospital/ or Trauma centers/)
24. 21 or 22 or 23 [Topic Search]
25. randomized controlled trial.pt.
26. controlled clinical trial.pt.
27. randomized.ab.
28. placebo.ab.
29. clinical trials as topic/
30. randomly.ab.
31. trial.ti.

31a. or/25-31 [RCT Filter]
32. intervention*.ti.
33. (intervention* adj6 (clinician* or collaborat* or community or complex or DESIGN* or doctor* or educational or family doctor* or family physician* or family practitioner* or financial or GP or general practice* or hospital* or impact* or improv* or individuali?e* or individuali?ing or interdisciplin* or multicomponent or multi-component or multidisciplin* or multi-disciplin* or multifacet* or multi-facet* or multimodal* or multi-modal* or personali?e* or personali?ing or pharmacies or pharmacist* or pharmacy or physician* or practitioner* or prescrib* or prescription* or primary care or professional* or provider* or regulatory or regulatory or tailor* or target* or team* or usual care)).ab.
34. (collaborativ* or collaboration* or tailored or personali?ed).ti,ab.
35. (exp hospitals/ or exp Hospitalization/ or exp Patients/ or exp Nurses/ or exp Nursing/) and (study.ti. or evaluation studies as topic/)
36. demonstration project*.ti,ab.
37. (pre-post or "pre test*" or pretest* or posttest* or "post test*" or (pre adj5 post)).ti,ab.
38. (pre-workshop or post-workshop or (before adj3 workshop) or (after adj3 workshop)).ti,ab.
39. ((study adj3 aim?) or "our study").ab.
40. (before adj10 (after or during)).ti,ab.
41. ("quasi-experiment*" or quasiexperiment* or "quasi random*" or quasirandom* or "quasi control*" or quasicontrol* or ((quasi* or experimental) adj3 (method* or study or trial or design*))).ti,ab,hw.
42. ("time series" adj2 interrupt*).ti,ab,hw.
43. (time points adj3 (over or multiple or three or four or five or six or seven or eight or nine or ten or eleven or twelve or month* or hour? or day? or "more than")).ab.
44. pilot.ti.
45. Pilot projects/
46. clinical trial.pt.
47. multicenter study.pt.
48. (multicentre or multicenter or multi-centre or multi-center).ti.
49. random*.ti,ab. or controlled.ti.
50. (control adj3 (area or cohort? or compar? or condition or group? or intervention? or participant? or study)).ab.
51.or/32-50 [EPOC Methodolgical Filter version 2.2]
52. exp animals/ not humans/
53. "comment on".cm. or systematic review.ti. or literature review.ti. or editorial.pt. or letter.pt. or meta-analysis.pt. or news.pt. or review.pt.
54. 52 or 53
55. (or/31a,51)not 54
56. 24 and 55

Appendix 2. Embase (Ovid) Search Strategy:

Embase (Ovid) (1980-):
1. *emergency ward/
2. *emergency health service/
3. Triage/
4. (emergency adj2 (care or healthcare or department? or unit or units or room? or treatment?)).ti,ab.
5. ("accident and emergency" or "accident & emergency" or emergency service?).ti,ab.
6. (trauma adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab.
7. (triage adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab.
8. (emergency adj2 visit?).ti,ab.
9. (urgent adj2 (care or healthcare or health care)).ti,ab.
10. ((semiurgent or semi-urgent or nonemergen$ or non-emergen$) adj2 (treatment? or care or visit?)).ti,ab.
11. ((emergency or non-emergency or nonemergency or urgent or non-urgent or nonurgent or semi-urgent or semiurgent) adj2 patient?).ti,ab.
12. or/1-11
13. general practitioner/

14. health auxiliary/ or mental health care personnel/ or paramedical personnel/ or occupational therapist/ or occupational therapy assistant/ or ophthalmic technologist/ or pharmacist/ or pharmacy technician/ or physiotherapist/ or physiotherapist assistant/ or radiological technologist/
15. advanced practice nurse/ or clinical nurse specialist/
16. nurse practitioner/
17. ((general or family) adj3 (practitioner? or physician? or doctor?)).ti,ab.
18. (nurse practitioner? or nurse specialist?).ti,ab.
19. ("out of hours" or after hours).ti,ab.
20. or/13-19
21. 12 and 20
22. ((community or primary health$ or primary care) adj2 (nurse or nurses or nursing staff or nursing personnel$)).ti,ab. and (emergency health service/ or emergency ward/)
23. Community.ti,hw. and (emergency health service/ or emergency ward/)
24. emergency nurse practitioner/
25. 21 or 22 or 23 or 24
26. randomized controlled trial/
27. crossover-procedure/
28. double-blind procedure/
29. single-blind procedure/
30. (random$ or factorial$ or crossover$ or cross-over$ or placebo$ or (doubl$ adj blind$) or (singl$ adj blind$) or assign$ or allocat$ or volunteer$).ti,ab.
31. or/26-30
32. intervention*.ti.
33. (intervention* adj6 (clinician* or collaborat* or community or complex or DESIGN* or doctor* or educational or family doctor* or family physician* or family practitioner* or financial or GP or general practice* or hospital* or impact* or improv* or individuali?e* or individuali?ing or interdisciplin* or multicomponent or multi-component or multidisciplin* or multi-disciplin* or multifacet* or multi-facet* or multimodal* or multi-modal* or personali?e* or personali?ing or pharmacies or pharmacist* or pharmacy or physician* or practitioner* or prescrib* or prescription* or primary care or professional* or provider* or regulatory or regulatory or tailor* or target* or team* or usual care)).ab.
34. (collaborativ* or collaboration* or tailored or personali?ed).ti,ab.
35. demonstration project*.ti,ab.
36. (pre-post or "pre test*" or pretest* or posttest* or "post test*" or (pre adj5 post)).ti,ab.
37. (pre-workshop or post-workshop or (before adj3 workshop) or (after adj3 workshop)).ti,ab.
38. ((study adj3 aim?) or "our study").ab.
39. (before adj10 (after or during)).ti,ab.
40. ("quasi-experiment*" or quasiexperiment* or "quasi random*" or quasirandom* or "quasi control*" or quasicontrol* or ((quasi* or experimental) adj3 (method* or study or trial or design*))).ti,ab,hw.
41. ("time series" adj2 interrupt*).ti,ab.
42. (time points adj3 (over or multiple or three or four or five or six or seven or eight or nine or ten or eleven or twelve or month* or hour? or day? or "more than")).ab.
43. pilot.ti.
44. *experimental design/ or *pilot study/ or quasi experimental study/
45. (multicentre or multicenter or multi-centre or multi-center).ti.
46. random*.ti,ab. or controlled.ti.
47. (control adj3 (area or cohort? or compar? or condition or group? or intervention? or participant? or study)).ab.
48. or/32-47
49. 31 or 48
50. (animal model? or animal experiment? or animal study? or animal trial? or canine or feline or bovine or cow or cows or mice or dog? or cat or cats or rabbit? or rat or rats or veterinar$).ti. or (animal or veterinary).hw.
51. (editorial or letter or note or "review" or trade or survey).pt.
52. meta-analysis/ or systematic review/ or "literature review".ti. or "systematic review".ti. or (meta-analy$ or metaanalyt$).ti.
53. 50 or 51 or 52
54. 49 not 53

55. 25 and 54

Appendix 3. PsycINFO (Ovid) Search Strategy:

PsycINFO (Ovid) (1967 -):

1. *emergency services/
2. (emergency adj2 (care or healthcare or department? or unit or units or room? or treatment?)).ti,ab.
3. ("accident and emergency" or "accident & emergency" or emergency service?).ti,ab.
4. (trauma adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab.
5. (triage adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab.
6. (emergency adj2 visit?).ti,ab.
7. (urgent adj2 (care or healthcare or health care)).ti,ab.
8. ((semiurgent or semi-urgent or nonemergen$ or non-emergen$) adj2 (treatment? or care or visit?)).ti,ab.
9. ((emergency or non-emergency or nonemergency or urgent or non-urgent or nonurgent or semi-urgent or semiurgent) adj2 patient?).ti,ab.
10. or/1-9
11. family physicians/ or general practitioners/
12. exp mental health personnel/
13. ((general or family) adj3 (practitioner? or physician? or doctor?)).ti,ab.
14. (nurse practitioner? or nurse specialist? or nurse aide? or nurse auxilliar$).ti,ab.
15. ((community or primary health$ or primary care) adj2 (nurse or nurses or nursing staff or nursing personnel$)).ti,ab.
16. (((allied health or paramedical or auxilliary) adj2 (staff or personnel)) or (pharmacist$ or pharmacy technician$ or pharmacy aide$)).ti,ab.
17. (((mental health or psychiatric) adj2 (nurse$ or staff or personnel)) or psychiatrist$).ti,ab.
18. ("out of hours" or after hours).ti,ab.
19. or/11-18
20. 10 and 19
21. (abstract collection or bibliography or chapter or "column/opinion" or "comment/reply" or editorial or letter or obituary or publication information or reprint or review-book or review-media or review-software & other or reviews).dt.
22. ("literature review" or "systematic review" or (meta-analy$ or metaanalyt$)).ti.
23. 21 or 22
24. 20 not 23

Appendix 4. The Cochrane Central Register of Controlled Trials (Wiley) Search Strategy:

The Cochrane Central Register of Controlled Trials (Wiley):

#1 MeSH descriptor Emergency Medical Services, this term only
#2 MeSH descriptor Emergency Service, Hospital explode all trees
#3 MeSH descriptor Triage explode all trees
#4 (emergency near2 (care or healthcare or department* or unit or units or room* or treatment*)):ti,ab,kw
#5 ("accident and emergency" or "accident & emergency" or emergency service*):ti,ab,kw
#6 (trauma near2 (centre or centres or center or centers or department* or unit or units)):ti,ab,kw
#7 (triage near2 (centre or centres or center or centers or department* or unit or units)):ti,ab,kw
#8 (emergency near2 visit*):ti,ab,kw
#9 (urgent near2 (care or healthcare or health care)):ti,ab,kw
#10 ((semiurgent or semi-urgent or nonemergen* or non-emergen*) near2 (treatment* or care or visit*)):ti,ab,kw
#11 ((emergency or non-emergency or nonemergency or urgent or non-urgent or nonurgent or semi-urgent or semiurgent) near2 patient*):ti,ab,kw
#12 (#1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7 OR #8 OR #9 OR #10 OR #11) in Title, Abstract or Keywords
#13 MeSH descriptor Physicians, Family explode all trees
#14 MeSH descriptor Allied Health Personnel explode all trees
#15 MeSH descriptor Nurse Practitioners explode all trees

#16 ((general or family) near3 (practitioner* or physician* or doctor*)):ti,ab,kw #17 (nurse practitioner* or nurse specialist*):ti,ab,kw #18 ("out of hours"):ti,ab,kw
#19 MeSH descriptor After-Hours Care, this term only
#20 ((community or primary health* or primary care) near2 (nurse or nurses or nursing staff or nursing personnel*)):ti,ab,kw
#21 (#13 OR #14 OR #15 OR #16 OR #17 OR #18 OR #19 OR #20)
#22 (#12 AND #21) in Title, Abstract or Keywords

Appendix 5. Cinahl (Ebscohost) Search Strategy:

Cinahl (Ebscohost) (1980 -):
S54 S13 and S20
S53 S27 and S52
S52 S28 or S29 or S30 or S31 or S32 or S33 or S34 or S35 or S36 or S37 or S38 or S39 or S40 or S41 or S42 or S43 or S44 or S45 or S46 or S47 or S48 or S49 or S50 or S51
S51 TI ((time points n3 over) or (time points n3 multiple) or (time points n3 three) or (time points n3 four) or (time points n3 five) or (time points n3 six) or (time points n3 seven) or (time points n3 eight) or (time points n3 nine) or (time points n3 ten) or (time points n3 eleven) or (time points n3 twelve) or (time points n3 month*) or (time points n3 hour*) or (time points n3 day*) or (time points n3 "more than")) or AB ((time points n3 over) or (time points n3 multiple) or (time points n3 three) or (time points n3 four) or (time points n3 five) or (time points n3 six) or (time points n3 seven) or (time points n3 eight) or (time points n3 nine) or (time points n3 ten) or (time points n3 eleven) or (time points n3 twelve) or (time points n3 month*) or (time points n3 hour*) or (time points n3 day*) or (time points n3 "more than"))TI ((time points n3 over) or (time points n3 multiple) or (time points n3 three) or (time points n3 four) or (time points n3 five) or (time points n3 six) or (time points n3 seven) or (time points n3 eight) or (time points n3 nine) or (time points n3 ten) or (time points n3 eleven) or (time points n3 twelve) or (time points n3 month*) or (time points n3 hour*) or (time points n3 day*) or (time points n3 "more than")) or AB ((time points n3 over) or (time points n3 multiple) or (time points n3 ...
S50 TI ((control w3 area) or (control w3 cohort*) or (control w3 compar*) or (control w3 condition) or (control w3 group*) or (control w3 intervention*) or (control w3 participant*) or (control w3 study)) or AB ((control w3 area) or (control w3 cohort*) or (control w3 compar*) or (control w3 condition) or (control w3 group*) or (control w3 intervention*) or (control w3 participant*) or (control w3 study))
S49 TI (multicentre or multicenter or multi-centre or multi-center) or AB random*
S48 TI random* OR controlled
S47 TI (trial or (study n3 aim) or "our study") or AB ((study n3 aim) or "our study")
S46 TI (pre-workshop or preworkshop or post-workshop or postworkshop or (before n3 workshop) or (after n3 workshop)) or AB (pre-workshop or preworkshop or post-workshop or postworkshop or (before n3 workshop) or (after n3 workshop))
S45 TI (demonstration project OR demonstration projects OR preimplement* or pre-implement* or post-implement* or postimplement*) or AB (demonstration project OR demonstration projects OR preimplement* or pre-implement* or post-implement* or postimplement*)
S44 (intervention n6 clinician*) or (intervention n6 community) or (intervention n6 complex) or (intervention n6 design*) or (intervention n6 doctor*) or (intervention n6 educational) or (intervention n6 family doctor*) or (intervention n6 family physician*) or (intervention n6 family practitioner*) or (intervention n6 financial) or (intervention n6 GP) or (intervention n6 general practice*) Or (intervention n6 hospital*) or (intervention n6 impact*) Or (intervention n6 improv*) or (intervention n6 individualize*) Or (intervention n6 individualise*) or (intervention n6 individualizing) or (intervention n6 individualising) or (intervention n6 interdisciplin*) or (intervention n6 multicomponent) or (intervention n6 multi-component) or (intervention n6 multidisciplin*) or (intervention n6 multi-disciplin*) or (intervention n6 multifacet*) or (intervention n6 multi-facet*) or (intervention n6 multimodal*) or (intervention n6 multi-modal*) or (intervention n6 personalize*) or(intervention n6 personalise*) or (intervention n6 personalizing) or (intervention n6 personalising) or (intervention n6 pharmaci*) or (intervention n6 pharmacist*) or (intervention n6 pharmacy) or (intervention n6 physician*) or (intervention n6 practitioner*) Or (intervention n6 prescrib*) or (intervention n6 prescription*) or (intervention n6 primary care) or (intervention n6 professional*) or (intervention* n6 provider*) or (intervention* n6 regulatory) or (intervention n6 regulatory) or (intervention n6 tailor*) or (intervention n6 target*) or (intervention n6 team*) or (intervention n6 usual care)(intervention n6 clinician*) or (intervention n6 community) or (intervention n6 complex) or (intervention n6 design*) or (intervention n6 doctor*) or (intervention n6 educational) or (intervention n6 family doctor*) or (intervention n6 family physician*)

or (intervention n6 family practitioner*) or (intervention n6 financial) or (intervention n6 GP) or (intervention n6 general practice*) Or (intervention n6 hospital*) or (intervention n6 impact*) Or (intervention n6 improv*) or (intervention n6 ...
S43 TI (collaborativ* or collaboration* or tailored or personalised or personalized) or AB (collaborativ* or collaboration* or tailored or personalised or personalized)
S42 TI pilot
S41 (MH "Pilot Studies")
S40 AB "before-and-after"
S39 AB time series
S38 TI time series
S37 AB (before* n10 during or before n10 after) or AU (before* n10 during or before n10 after)
S36 TI ((time point*) or (period* n4 interrupted) or (period* n4 multiple) or (period* n4 time) or (period* n4 various) or (period* n4 varying) or (period* n4 week*) or (period* n4 month*) or (period* n4 year*)) or AB ((time point*) or (period* n4 interrupted) or (period* n4 multiple) or (period* n4 time) or (period* n4 various) or (period* n4 varying) or (period* n4 week*) or (period* n4 month*) or (period* n4 year*))
S35 TI ((quasi-experiment* or quasiexperiment* or quasi-random* or quasirandom* or quasi control* or quasicontrol* or quasi* W3 method* or quasi* W3 study or quasi* W3 studies or quasi* W3 trial or quasi* W3 design* or experimental W3 method* or experimental W3 study or experimental W3 studies or experimental W3 trial or experimental W3 design*)) or AB ((quasi-experiment* or quasiexperiment* or quasi-random* or quasirandom* or quasi control* or quasicontrol* or quasi* W3 method* or quasi* W3 study or quasi* W3 studies or quasi* W3 trial or quasi* W3 design* or experimental W3 method* or experimental W3 study or experimental W3 studies or experimental W3 trial or experimental W3 design*))TI ((quasi-experiment* or quasiexperiment* or quasi-random* or quasirandom* or quasi control* or quasicontrol* or quasi* W3 method* or quasi* W3 study or quasi* W3 studies or quasi* W3 trial or quasi* W3 design* or experimental W3 method* or experimental W3 study or experimental W3 studies or experimental W3 trial or experimental W3 design*)) or AB ((quasi-experiment* or quasiexperiment* or quasi-random* or quasirandom* or quasi control* or quasicontrol* or quasi* W3 method* or quasi* W3 s ...
S34 TI pre w7 post or AB pre w7 post
S33 MH "Multiple Time Series" or MH "Time Series"
S32 TI ((comparative N2 study) or (comparative N2 studies) or evaluation study or evaluation studies) or AB ((comparative N2 study) or (comparative N2 studies) or evaluation study or evaluation studies)
S31 MH Experimental Studies or Community Trials or Community Trials or Pretest-Posttest Design + or Quasi-Experimental Studies + Pilot Studies or Policy Studies + Multicenter Studies
S30 TI (pre-test* or pretest* or posttest* or post-test*) or AB (pre-test* or pretest* or posttest* or "post test*) OR TI (preimplement*" or pre-implement*) or AB (pre-implement* or preimplement*)
S29 TI (intervention* or multiintervention* or multi-intervention* or postintervention* or post-intervention* or preintervention* or pre-intervention*) or AB (intervention* or multiintervention* or multi-intervention* or postintervention* or post-intervention* or preintervention* or pre-intervention*)
S28 (MH "Quasi-Experimental Studies")
S27 S21 or S22 or S23 or S24 or S25 or S26
S26 (TI community or MW community) and ((MH "Emergency Service") OR (MH "Trauma Centers"))
S25 (TI (Primary care n2 nurse or primary care n2 nurses or primary care n2 nursing staff or primary care nursing personnel*) or AB (Primary care n2 nurse or primary care n2 nurses or primary care n2 nursing staff or primary care nursing personnel*)) and ((MH "Emergency Service") OR (MH "Trauma Centers"))
S24 (TI (Primary health n2 nurse or primary health n2 nurses or primary health n2 nursing staff or primary health nursing personnel*) or AB (Primary health n2 nurse or primary health n2 nurses or primary health n2 nursing staff or primary health nursing personnel*)) and ((MH "Emergency Service") OR (MH "Trauma Centers"))
S23 (TI (Community n2 nurse or community n2 nurses or community n2 nursing staff or community nursing personnel*) or AB (Community n2 nurse or community n2 nurses or community n2 nursing staff or community nursing personnel*)) and ((MH "Emergency Service") OR (MH "Trauma Centers"))
S22 (MH "Emergency Nurse Practitioners")
S21 S13 and S20
S20 S14 or S15 or S16 or S17 or S18 or S19
S19 TI ("out of hours" or afterhours care or after hours care) or AB ("out of hours" or afterhours care or after hours care)
S18 TI (nurse practitioner* or nurse specialist*) or AB (nurse practitioner* or nurse specialist*)

S17 (MH "Allied Health Personnel") OR (MH "Emergency Medical Technicians") OR (MH "Medical Assistants") OR (MH "Ophthalmic Technologists") OR (MH "Orthopedic Technologists") OR (MH "Pharmacy Technicians") OR (MH "Community Health Workers") OR (MH "Mental Health Personnel+") OR (MH "Pharmacists") OR (MH "Rural Health Personnel") OR (MH "Nursing Assistants") OR (MH "Psychiatric Technicians")
S16 (MH "Clinical Nurse Specialists")
S15 (MH "Nurse Practitioners") OR (MH "Clinical Nurse Specialists") OR (MH "Family Nurse Practitioners") OR (MH "Pediatric Nurse Practitioners") OR (MH "Acute Care Nurse Practitioners") OR (MH "Adult Nurse Practitioners") OR (MH "Gerontologic Nurse Practitioners") OR (MH "OB-GYN Nurse Practitioners")
S14 (MH "Physicians, Family")
S13 S1 or S2 or S3 or S4 or S5 or S6 or S7 or S8 or S9 or S10 or S11 or S12
S12 (MH "Triage")
S11 TI (Emergency n2 patient* or non-emergency n2 patient* or nonemergency n2 patient* or urgent n2 patient* or non-urgent n2 patient* or nonurgent n2 patient* or semiurgent n2 patient or semi-urgent n2 patient*) or AB (Emergency n2 patient* or non-emergency n2 patient* or nonemergency n2 patient* or urgent n2 patient* or non-urgent n2 patient* or nonurgent n2 patient* or semiurgent n2 patient or semi-urgent n2 patient*)
S10 TI (Non-emergen* n2 treatment* or non-emergen* n2 care or non-emergen* n2 visit or non-emergen* n2 visits) or AB (Non-emergen* n2 treatment* or non-emergen* n2 care or non-emergen* n2 visit or non-emergen* n2 visits)
S9 TI (nonemergen* n2 treatment* or nonemergen* n2 care or nonemergen* n2 visit or nonemergen* n2 visits) or AB (nonemergen* n2 treatment* or nonemergen* n2 care or nonemergen* n2 visit or nonemergen* n2 visits)
S8 TI (semi-urgent n2 treatment* or semi-urgent n2 care or semi-urgent n2 visit or semi-urgent n2 visits) or AB (semi-urgent n2 treatment* or semi-urgent n2 care or semi-urgent n2 visit or semi-urgent n2 visits)
S7 TI (semiurgent n2 treatment* or semiurgent n2 care or semiurgent n2 visit or semiurgent n2 visits) or AB (semiurgent n2 treatment* or semiurgent n2 care or semiurgent n2 visit or semiurgent n2 visits)
S6 TI (urgent n2 care or urgent n2 healthcare or urgent n2 health care) or AB (urgent n2 care or urgent n2 healthcare or urgent n2 health care)
S5 TI (triage n2 care or triage n2 healthcare or triage n2 department* or triage n2 unit or triage n2 units or triage n2 treatment* or triage n2 visit or triage n2 visits) or AB (triage n2 care or triage n2 healthcare or triage n2 department* or triage n2 unit or triage n2 units or triage n2 treatment* or triage n2 visit or triage n2 visits)
S4 TI (trauma n2 care or trauma n2 healthcare or trauma n2 department* or trauma n2 unit or trauma n2 units or trauma n2 treatment* or trauma n2 visit or trauma n2 visits) or AB (trauma n2 care or trauma n2 healthcare or trauma n2 department* or trauma n2 unit or trauma n2 units or trauma n2 treatment* or trauma n2 visit or trauma n2 visits)
S3 TI ("accident and emergency" or "accident & emergency" or emergency service*) or AB ("accident and emergency" or "accident & emergency" or emergency service*)
S2 TI (emergency n2 care or emergency n2 healthcare or emergency n2 department* or emergency n2 unit or emergency n2 units or emergency n2 treatment* or emergency n2 visit or emergency n2 visits) or AB (emergency n2 care or emergency n2 healthcare or emergency n2 department* or emergency n2 unit or emergency n2 units or emergency n2 treatment* or emergency n2 visit or emergency n2 visits)

Appendix 6. Sociological Abstracts (CSA Illumina) Search Strategy:

Sociological Abstracts (CSA Illumina) (1952 -):
((DE="emergency medical services") or (emergency within 2 (care or healthcare or department* or unit or units or room* or treatment*)) or ("accident and emergency" or "accident & emergency" or emergency service*) or (trauma within 2 (centre or centres or center or centers or department* or unit or units)) or (triage within 2 (centre or centres or center or centers or department* or unit or units)) or (emergency within 2 visit*) or (urgent within 2 (care or healthcare or health care)) or ((semiurgent or semi-urgent or nonemergen* or non-emergen*) within 2 (treatment* or care or visit*)) or ((emergency or non-emergency or nonemergency or urgent or non-urgent or nonurgent or semi-urgent or semiurgent) within 2 patient*)) and (((general or family) within 3 (practitioner* or physician* or doctor*)) or (nurse practitioner* or nurse specialist*) or (DE="paramedical personnel") or ((allied health or community) within 3 (worker* or personnel or staff*)) or (TI=community))

Appendix 7. Applied Social Sciences Index and Abstracts (CSA Illumina) Search Strategy:

Applied Social Sciences Index and Abstracts (CSA Illumina) (1987 -)

((DE="emergency medical services") or (emergency within 2 (care or healthcare or department* or unit or units or room* or treatment*)) or ("accident and emergency" or "accident & emergency" or emergency service*) or (trauma within 2 (centre or centres or center or centers or department* or unit or units)) or (triage within 2 (centre or centres or center or centers or department* or unit or units)) or (emergency within 2 visit*) or (urgent within 2 (care or healthcare or health care)) or ((semiurgent or semi-urgent or nonemergen* or non-emergen*) within 2 (treatment* or care or visit*)) or ((emergency or non-emergency or nonemergency or urgent or non-urgent or nonurgent or semi-urgent or semiurgent) within 2 patient*)) and (((general or family) within 3 (practitioner* or physician* or doctor*)) or (nurse practitioner* or nurse specialist*) or (DE="paramedical personnel") or ((allied health or community) within 3 (worker* or personnel or staff*)) or (TI=community))

Appendix 8. Health Management Information Consortium (Ovid) Search Strategy:

Health Management Information Consortium (Ovid) (1979 -)

1. accident & emergency departments/ or accident & emergency services/ or hospital emergency services/ or accident & emergency patients/
2. triage/
3. (emergency adj2 (care or healthcare or department? or unit or units or room? or treatment?)).ti,ab.
4. ("accident and emergency" or "accident & emergency" or emergency service?).ti,ab.
5. (trauma adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab.
6. (triage adj2 (centre or centres or center or centers or department? or unit or units)).ti,ab.
7. (emergency adj2 visit?).ti,ab.
8. (urgent adj2 (care or healthcare or health care)).ti,ab.
9. ((semiurgent or semi-urgent or nonemergen$ or non-emergen$) adj2 (treatment? or care or visit?)).ti,ab.
10. ((emergency or non-emergency or nonemergency or urgent or non-urgent or nonurgent or semi-urgent or semiurgent) adj2 patient?).ti,ab.
11. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10
12. general practitioners/ or family practitioners/ or general practice staff/
13. exp allied health professionals/
14. exp nurse practitioners/
15. Community mental health nurses/ or Community nurses/ or exp Primary care nurses/ or exp general practice nurses/
16. ((general or family) adj3 (practitioner? or physician? or doctor?)).ti,ab.
17. (nurse practitioner? or nurse specialist?).ti,ab.
18. ("out of hours" or after hours).ti,ab.
19. "out of hours health services"/
20. ((community or primary care or primary health*) adj3 (nurse or nurses or nursing staff or nursing personnel)).ti,ab.
21. ((community or primary care or primary health*) adj3 (doctor* or physician* or practitioner*)).ti,ab.
22. (("allied health" or community) adj3 (worker* or staff* or personnel*)).ti,ab.
23. ((nursing or nurse or nurses) adj3 (aide or aides or assistant*)).ti,ab.
24. ((physician* or doctor*) adj3 (aide or aides or assistant*)).ti,ab.
25. ((psychiatric or "mental health") adj3 (nurse or nurses or aide or aides or assistant*)).ti,ab.
26. ((radiolog* or ophthalm* or pharmac*) adj3 (nurse or nurses or aide or aides or assistant* or technician*)).ti,ab.
27. 12 or 13 or 14 or 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22 or 23 or 24 or 25 or 26
28. 11 and 27
29. (random* or blind* or allocat* or assign* or trial* or placebo* or crossover* or cross-over*).ti,ab.
30. (intervention* adj6 (clinician* or collaborat* or community or complex or DESIGN* or doctor* or educational or family doctor* or family physician* or family practitioner* or financial or GP or general practice* or hospital* or impact* or improv* or individuali?e* or individuali?ing or interdisciplin* or multicomponent or multi-component or multidisciplin* or multi-disciplin* or multifacet* or multi-facet* or multimodal* or multi-modal* or personali?e* or personali?ing or pharmacies or pharmacist* or pharmacy or physician* or practitioner* or prescrib* or prescription* or primary care or professional* or provider* or regulatory or regulatory or tailor* or target* or team* or usual care)).ab.
31. intervention*.ti.

32. (collaborativ* or collaboration* or tailored or personali?ed).ti,ab.
33. demonstration project*.ti,ab.
34. (pre-post or "pre test*" or pretest* or posttest* or "post test*" or (pre adj5 post)).ti,ab.
35. (pre-workshop or post-workshop or (before adj3 workshop) or (after adj3 workshop)).ti,ab.
36. ((study adj3 aim?) or "our study").ab.
37. (before adj10 (after or during)).ti,ab.
38. ("quasi-experiment*" or quasiexperiment* or "quasi random*" or quasirandom* or "quasi control*" or quasicontrol* or ((quasi* or experimental) adj3 (method* or study or trial or design*))).ti,ab.
39. ("time series" adj2 interrupt*).ti,ab.
40. (time points adj3 (over or multiple or three or four or five or six or seven or eight or nine or ten or eleven or twelve or month* or hour? or day? or "more than")).ab.
41. pilot.ti.
42. (multicentre or multicenter or multi-centre or multi-center).ti.
43. (control adj3 (area or cohort? or compar? or condition or group? or intervention? or participant? or study)).ab.
44. 29 or 30 or 31 or 32 or 33 or 34 or 35 or 36 or 37 or 38 or 39 or 40 or 41 or 42 or 43
45. 28 and 44

Appendix 9. Science Citation Index and Social Science Citation Index (Web of Knowledge) Search Strategy:

Science Citation Index and Social Science Citation Index(Web of Knowledge) (SCI=1945 -, SSCI=1952 -):
#33 #17 and #32
32 #18 or #19 or #20 or #21 or #22 or #23 or #24 or #25 or #26 or #27 or #28 or #29 or #30 or #31
31 Topic=((control SAME (area or cohort* or compar* or condition or group* or intervention* or participant* or study)))
30 Topic=(multicentre or multicenter or multi-centre or multi-center)
29 Topic=((time points SAME (over or multiple or three or four or five or six or seven or eight or nine or ten or eleven or twelve or month* or hour* or day* or "more than")))
28 Topic=(("time series" SAME interrupt*))
27 Topic=(("quasi-experiment*" or quasiexperiment* or "quasi random*" or quasirandom* or "quasi control*" or quasicontrol* or ((quasi* or experimental) SAME (method* or study or trial or design*))))
26 Topic=(((study SAME aim*) or "our study"))
25 Topic=((pre-workshop or post-workshop or (before SAME workshop) or (after SAME workshop)))
24 Topic=((pre-post or "pre test*" or pretest* or posttest* or "post test*" or (pre SAME post)))
23 Title=(pilot)
22 Topic=((demonstration OR pilot) NEXT project*)
21 Topic=(collaborativ* OR collaboration* OR tailored OR personalised OR personalized)
20 Topic=((intervention* SAME (clinician* or collaborat* or community or complex or DESIGN* or doctor* or educational or family doctor* or family physician* or family practitioner* or financial or GP or general practice* or hospital* or impact* or improv* or individuali*e* or individuali*ing or interdisciplin* or multicomponent or multi-component or multidisciplin* or multi-disciplin* or multifacet* or multi-facet* or multimodal* or multi-modal* or personali*e* or personali*ing or pharmacies or pharmacist* or pharmacy or physician* or practitioner* or prescrib* or prescription* or primary care or professional* or provider* or regulatory or regulatory or tailor* or target* or team* or usual care)))
19 Title=(intervention*)
18 Topic=((random* or blind* or allocat* or assign* or trial* or placebo* or crossover* or cross-over*))
17 #16 AND #8
16 #15 OR #14 OR #13 OR #12 OR #11 OR #10 OR #9
15 Topic=((psychiatric OR "mental health") NEXT (nurse OR nurses OR aide OR aides OR assistant*)) OR Topic=((radiolog* OR ophthalm* OR pharmac*) NEXT (nurse OR nurses OR aide OR aides OR assistant* OR technician*))
14 Topic=((nursing OR nurse OR nurses) NEXT (aide OR aides OR assistant*)) OR Topic=((physician* OR doctor*) NEXT (aide OR aides OR assistant*))
13 Topic=(("primary care" OR "primary health*") SAME (physician* OR practitioner* OR doctor*))
12 Topic=(("primary care" OR "primary health*") SAME (nurse OR nurses))

11 Topic=((”allied health“ OR community) NEXT (worker* OR staff* OR personnel*)) OR Topic=(community SAME (nurse OR nurses OR ”nursing staff“ OR ”nursing personnel“))
10 Topic=(”nurse practitioner*“ OR ”nurse specialist*“)
9 Topic=(”general practitioner*“ OR ”family practitioner*“ OR ”general physician*“ OR ”family physician*“ OR ”general doctor*“ OR ”family doctor*“)
8 #7 OR #6 OR #5 OR #4 OR #3 OR #2 OR #1
7 Topic=(((emergency OR non-emergency OR nonemergency OR urgent OR non-urgent OR nonurgent OR semi-urgent OR semiurgent) SAME patient*))
6 Topic=(((semiurgent OR semi-urgent OR nonemergen* OR non-emergen*) SAME (treatment* OR care OR visit*)))
5 Topic=((emergency SAME visit*))
4 Topic=((triage SAME (centre OR centres OR center OR centers OR department* OR unit OR units)))
3 Topic=((trauma SAME (centre OR centres OR center OR centers OR department* OR unit OR units)))
2 Topic=((”accident and emergency“ OR ”accident & emergency“ OR emergency service*))
1 Topic=((emergency SAME (care OR healthcare or department* OR unit OR units OR room* OR treatment*)))

Appendix 10. Data Extraction Form

Study ID No.	1st Author	Year	Contact Email or No.
Title:			
Reviewer:		Date:	

ELIGIBILITY CRITERIA
A. Intervention :

Population	
Intervention	
Comparison	
Outcomes	
Location	

B. Study Design is one of the following; please record the corresponding number in the box.
1. Randomized control trial (RCT)
2. Controlled Before-After (CBA) with
3. Interrupted Time Series (ITS) where the
4. Qualitative
5. Other ····· (not to be included in review).

If the study is either a CBA or RCT, does it meet the following EPOC criteria? [yes or no]

If a Controlled Before-After (CBA), design includes:
o Contemporaneous data collection & minimum 2 control and intervention sites
o Choice of control site / activity appropriate for question asked
o A second comparison site
If an Interrupted Time Series (ITS) study, design includes:
o Intervention occurs at a clearly defined point in time and there are
o Minimum of 3 data points before and 3 after intervention

·····No ·····Yes Excel Code: 0 no; 1 yes

C. Studies must meet the following methodological criteria for inclusion:
(a) study includes objective measurement(s) of outcomes.

Done	(e.g. drug levels a by a test, performance of providers against pre-set criteria, number of tests ordered, number of c-sections performed etc.). Outcome measures like provider or patient satisfaction included if assessed using a questionnaire with known reliability and validity	
Not clear	the paper should be discussed with the contact editor for the review before data extraction is undertaken	
Not done	(e.g. self-reported data, measurement of attitudes, beliefs, perceptions or satisfaction)	

(b) Relevant and interpretable data is presented or obtainable

Done	
Not clear	
Not done	

If criteria B and C above are met, continue. Otherwise, provide reason for exclusion:

INTERVENTIONS
3.1.Type of Intervention(s).

3.2. Triage method used (include def'n or criteria used for 'primary care suitable' cases::

3.3. Describe the study control group(s):

Characteristics of Intervention(s):

Who is delivering intervention?	
Skill type and level of training of health care providers	
Number of staff	
Setting (e.g. inside the A&E? Elsewhere in hospital?)	
Goal(s) of intervention: *Highlight as many reasons as applicable to the study.*	1. Decrease costs 2. Decrease wait times 3. Decrease health resource utilisation 4. Improve quality of care 5. Measure patient satisfaction 6. Measure provider satisfaction 7. Other (list below):
Source of funding:	
Conflicts of interest?	

3.4 Study Timing

When (historically) study took place (Eg. 1990-92)	
Intervention timing: (E.g. Which days? How many hours? How long?)	
Length of time underway (weeks):	
Duration of pre-intervention data collection (weeks):	
Duration post-intervention follow-up period	

PARTICIPANT CHARACTERISTICS
(a) hospital characteristics / Setting

Country:	
City:	
Provide any hospital characteristics, such as: § Rural vs urban § Size (# beds) § Average bed occupancy rate § Average no. visits per year § Academic status (teaching vs non-teaching)	
Hospital ownership: 1. Public or state owned, 2. private, 3. foreign owned, 4. other (provide), 5. not clear	
Type of hospital: 1. military 2. civic 3. not applicable 4. not clear	
Hospital scope 1. full service - i.e. tertiary hospital with access to most specialties 2. limited service - few specialists available 3. other 4. not clear	
System of finance for primary care visits: 1. universal public 2. private insurance 3. patients out of pocket 4. other (specify) 5. not clear	
System of finance for emergency visits: 1. universal public 2. private insurance 3. patient pays user fees or co-payments 4. not clear	
System of renumeration for health care providers in A&E: 1. fee per shift or hours worked 2. captitation 3. salary	

(b) Provider characteristics

Group	Profession (nurse, GP, A&E doctor etc)	Level of training (junior doctor, resident, etc)	Time since graduation (i.e. years in practice)
Intervention			
Control			

(b) participant (patient) characteristics

Group	Age (mean, median, range)	Gender (% female)	Ethnicity (breakdown by %)	Clinical characteristics *	Other Information
Intervention					
Control					

* if study provides breakdown of patients by triage category or by types of problems, include this here

(**c) Summary of numbers included in the study**

	n	Other info?
Patients		
Providers		
Practices		
Hospitals		

METHODS

Unit of allocation	
Unit of analysis	
Power calculation	

Quality Criteria:
6.4 Risk of bias assessment
(If the trial is an ITS go directly to 6.4.2 for the RoB assessment)

6.4.1 Risk of bias assessment for randomised controlled trials (RCTs), controlled clinical trials (CCTs) and controlled before and after studies (CBAs)

a) Was the allocation sequence adequately generated ?(cut and paste from the paper verbatim)

Score YES	If a random component in the sequence generation process is described (e.g. referring to a random numbers table)	
Score NO	If a non-random method is used (e.g. performed by date of submission)	
Score UNCLEAR	If not specified in the paper.	

b) Was the allocation adequately concealed?

Score YES	If the unit of allocation was by institution, team or professional and allocation was performed at all units at the start of the study; or if the unit of allocation was by patient or episode of care and there was some kind of centralised randomisation scheme; an on-site computer system or if sealed opaque envelopes were used	
Score NO	If none of the above mentioned methods were used (or if a CBA)	
Score UNCLEAR	If not specified in the paper.	

1. **Were baseline outcome measurements similar?**

Score YES	If performance or patient outcomes were measured prior to the intervention, and no important differences were present across study groups	
Score NO	If important differences were present and not adjusted for in analysis.**	
Score UNCLEAR	If RCTs have no baseline measure of outcome**	

d) Were baseline characteristics similar?

Score YES	If baseline characteristics of the study and control providers are reported and similar	
Score NO	If there is no report of characteristics in the text or tables or if there are differences between control and intervention providers	
Score UNCLEAR	If it is not clear in the paper (e.g. characteristics are mentioned in the text but no data were presented)	

e) Were incomplete outcome data adequately addressed?

Score YES	If missing outcome variables were unlikely to bias the results (e.g. the proportion of missing data was similar in the intervention and the control group, or the proportion of missing data was less than the effect size, i. e. unlikely to overturn the study results	
Score NO	If missing data was likely to bias the results.	
Score UNCLEAR	If not specified in the paper (Do not assume 100% follow up unless stated explicitly)	

f) Was knowledge of the allocated interventions adequately addressed?*

Score YES	If the authors state explicitly that primary outcome variables was assessed blindly, or the outcomes are objective e.g. length of hospital stay	
Score NO	If the outcomes were not assessed blindly.	
Score UNCLEAR	If not specified in the paper.	

d) Was the study adequately protected against contamination?

Score YES	If allocation was by community, institution or practice and it s unlikely that the control group received the intervention	
Score NO	If it is likely that the control group received the intervention (e.g. if patients rather than professionals were randomised)	
Score UNCLEAR	I professionals were allocated within a clinic or practice and it is possible that communication between intervention and control professionals could have occurred (e.g. physicians within practices were allocated to intervention or control)	

e) Was the study free from selective outcome reporting?

Score YES	If there is no evidence that outcomes were selectively reported (e.g. all relevant outcomes in the methods section are reported in the result section)	
Score NO	If some important outcomes are subsequently omitted from the results	
Score UNCLEAR	If not specified in the paper.	

f) Was the study free from other risks of bias?

Score YES	If no evidence of other risks of bias	
Score NO		
Score UNCLEAR		

*** If some primary outcomes were imbalanced at baseline, assessed blindly or affected by missing data and others were not, each primary outcome can be scored separately.**

****If "UNCLEAR" or "No", but there is sufficient data in the paper to do an adjusted analysis (e.g. Baseline adjustment analysis or Intention to treat analysis) the criteria should be re scored to "Yes".**

6.4.2 Risk of bias assessment for interrupted time series (ITS) designs

Note: If the ITS study has ignored secular (trend) changes and performed a simple t-test of the pre versus post intervention periods without further justification, the study should not be included in the review unless reanalysis is possible.

a) Was the intervention independent of other changes? (cut and paste from the paper verbatim)

Score YES	If there are compelling arguments that the intervention occurred independently of other changes over time and the outcome was not influenced by other confounding variables/historic events during study period	
Score NO	If reported that intervention was not independent of other changes in time *If Events/variables identified, note what they are.*	
Score UNCLEAR	If not specified in the paper.	

b) Was the shape of the intervention effects pre-specified?

Score YES	If point of analysis is the point of intervention OR a rational explanation for the shape of intervention effect was given by the author(s). Where appropriate, this should include an explanation if the point of analysis is NOT the point of intervention;	
Score NO	If it is clear that the condition above is not met	

(*Continued*)

Score UNCLEAR	If not specified in the paper.	

c) Was the intervention unlikely to affect data collection?

Score YES	If reported that intervention itself was unlikely to affect data collection (for example, sources and methods of data collection were the same before and after the intervention	
Score NO	If the intervention itself was likely to affect data collection (for example, any change in source or method of data collection reported)	
Score UNCLEAR	If not stated in the paper.	

d) Was knowledge of the allocated interventions adequately prevented during the study?***

Score YES	If the authors state explicitly that the primary outcome variables were assessed blindly, or the outcomes are objective, e.g. length of hospital stay. Primary outcomes are those variables that correspond to the primary hypothesis or question as defined by the authors	
Score NO	If the outcomes were not assessed blindly	
Score UNCLEAR	If not specified in the paper	

e) Were incomplete outcome data adequately addressed?***

Score YES	If missing outcome measures were unlikely to bias the results (e.g. the proportion of missing data was similar in the pre- and post-intervention periods or the proportion of missing data was less than the effect size i.e. unlikely to overturn the study result)	
Score NO	If missing data was likely to bias the results.	

(*Continued*)

Score UNCLEAR	If not specified in the paper (Do not assume 100% follow up unless stated explicitly)	

f) Was the study free from selective outcome reporting?

Score YES	If there is no evidence that outcomes were selectively reported (e.g. all relevant outcomes in the methods section are reported in the result section)	
Score NO	If some important outcomes are subsequently omitted from the results	
Score UNCLEAR	If not specified in the paper.	

g). Was the study free from other risks of bias?

Score YES	If no evidence of other risks of bias e.g. should consider if seasonality is an issue (i.e. if January to June comprises the pre-intervention period and July to December the post, could the "seasons' have caused a spurious effect)	
Score NO		
Score UNCLEAR		

*** **If some primary outcomes were assessed blindly or affected by missing data and others were not, each primary outcome can be scored separately.**

RESULTS

1. Main Outcomes

	Intervention	**Control**	**Notes (SD, CI, other):**
Mean time (arrival to assessment) in hours for MINORS			
Mean time (arrival to admission/discharge) in hours for MINORS			

(*Continued)*

Mean time (arrival to assessment) in hours for MAJORS			
Mean time (arrival à admission/ discharge) in hours for MAJORS			
% of patients admitted to hospital via A&E (number)			
% discharged from ED			
% left without being seen			

2. Secondary Outcomes

	Intervention	**Control**	**Notes (SD, CI, other):**
Diagnostic tests (overall #)			
Diagnostic tests (mean cost in study currency)			
% of patients referred to consultant			
% of patients referred to primary care			
% of patients for whom treatment initiated			
Arrangement of follow-up care (%)			
% patients who subsequently visit primary care for same problem (w/in 1 mos)			
% patients who reattend A&E for same problem (w/in 1 mos)			

(*Continued*)

Patient education Provided (%)			
Adverse outcome: % incorrect treatment			
Adverse outcome: % death within 1 mos of visit			

3. If reported, economic variables:

Cost of intervention (US $)	
Changes in direct HC costs due to intervention (US $)	
Are costs a/w intervention linked to outcomes?	

Cost-comparison:

Mean cost of	**Intervention**	**Control**	**Notes:**
Diagnostic tests			
Treatment			
Referrals			
Admissions			

OTHER:

HISTORY

Protocol first published: Issue 2, 2000

Review first published: Issue 11, 2012

Date	Event	Description
4 October 2011	Amended	Updated protocol.
18 July 2011	Feedback has been incorporated	Authors added, feedback incorporated.
12 November 2008	Amended	Converted to new review format.

CONTRIBUTIONS OF AUTHORS

All authors have been involved in drafting the review and contributed to the completion of the review.

DECLARATIONS OF INTEREST

None known.

SOURCES OF SUPPORT

Internal sources

- Cochrane Centre, UK.

External sources

- Rhodes Scholarship Trust, UK.
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- Canadian Institutes of Health Research (CIHR), Ottawa, ON, Canada.

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

Edits were made to the order and description of Objectives to reflect the original Outcomes defined in the Protocol.

Non-randomised controlled studies were included in this review after discussion. Our decision arose because after screening over 4575 published papers, only a small proportion of studies screened were relevant to the intervention of interest and none met the reviews initial eligibility criteria. The majority of studies excluded from the review after meeting content criteria were descriptive observational studies, either without a control comparison group or for those with a before and after design, without adequate baseline measurements for comparison. The three included studies provided the highest available level of evidence on this intervention.