WHAT WORKS: CRIME REDUCTION SYSTEMATIC REVIEW SERIES

NO. 7: POLICE PRE-ARREST DIVERSION OF PEOPLE WITH MENTAL HEALTH ISSUES: A SYSTEMATIC REVIEW OF THE IMPACTS ON CRIME AND MENTAL HEALTH

Authors: Karen Schucan Bird, Carol Vigurs, and Katie Quy
With Ian Shemilt contributing the Economics section.

EPPI Centre, UCL Department of Social Science, University College London, UK

Corresponding author:
Karen Schucan Bird
EPPI Centre
Social Science Research Unit (SSRU)
UCL Institute of Education
18 Woburn Square
London WC1H 0NR

Email: k.bird@ucl.ac.uk
Tel: +44 (0)20 7612 6465
Fax: +44 (0)20 7612 6400

Contents

LIST OF TABLES

LIST OF FIGURES

BACKGROUND

Policing and mental health

Mental health problems are common in the Western world. In England, a quarter of all adults report that they have been diagnosed with a mental health problem in their lifetime (Bridges, 2014) and 18% of US adults reported an incidence of mental illness in the past year (Center for Behavioral Health Statistics and Quality, 2015). Despite the lack of officially recorded data (Bradley, 2009), there is indicative evidence that people with mental health problems are more likely than the general population to come into contact with the police, whether as victims or suspects (Butler, 2014). People with mental health problems are more likely to be a victim of crime than the general population (Pettitt et al., 2013) and experience a range of other issues, such as drug, alcohol misuse and/ or homelessness (Sainsbury 2009) that increase their likelihood of coming into contact with the criminal justice system. A significant proportion of individuals being held in custody have been identified as having some kind of mental illness (HM Inspectorate of Prisons and HM Inspectorate of Constabulary, 2012) and a high proportion of convicted offenders have a mental disorder (James and Glaze, 2006; Sirdifield et al., 2009).

Mental health is recognised as a part of the 'core business of policing' (Adebowale, 2013; Butler, 2014). Changes in community mental health services mean that the police constitute the 'first emergency service' for people experiencing a mental health crisis (Lamb et al, 2002). The nature of policing and mental health in England and Wales, however, is complex and challenging. Officers do not have sufficient resources to deal with people with mental health issues (PMHI) or assist individuals in crisis (Home Affairs Select Committee, 2015). PMHI who are suspected of an offence can be cautioned, arrested and/ or taken into police custody. Typically involving low level offences, anti-social behaviour or 'survival crimes' (Hiday, 1999), such arrests are considered to be unnecessary or contributing to the 'criminalisation of mental illness' (Butler, 2014; Reuland et al., 2009; Teplin, 1985). Alternatively, an individual in need of 'immediate care or control' can be detained under section 136 of the Mental Health Act (1983). Such individuals are often taken to police custody cells, rather than NHS Mental Health Section 136 suites, due to lack of capacity in the health system (HMIC, 2013; NHS Confederation, 2015).

The economic implications of these police responses are far reaching. A 2007 report estimated that £1.6 billion is spent annually arresting, convicting, imprisoning and supervising people with identified mental health problems (Corner et al 2007). Processing adult offenders with mental health problems through the criminal justice systems has been found to absorb, on average, more resources (including police, court, prison and probation services), with corollary higher costs, than processing those without mental health problems who have committed an equivalent offence (Corner et al 2007). Similarly, treating the physical health issues of patients with a mental health problems has been estimated to impose up to 45% higher costs on the health system than treating those without, even after the cost of treating the mental health issue has been excluded (Welch et al 2009, Naylor et al 2012).

Following the Bradley report in 2009, policing and mental health has attracted a significant amount of policy attention (Adebowale, 2013; Home Affairs Select Committee, 2015). There has been a renewed interest in the potential of interventions to divert PMHI away from the criminal justice system (CJS) and towards community-based services. Successive UK

Governments have subsequently invested funds in strategies to support the identification and diversion of PMHI away from the CJS.

Whilst PMHI can be diverted at various stages of the criminal justice pathway (Munetz and Griffin, 2006), this review focuses on the early stages, before an arrest takes place. A range of policing strategies have been developed to intercept PMHI at this stage and these are known as police pre-arrest/ pre-booking diversion programmes. Such interventions allow police officers to use their discretion to divert individuals suspected of non-violent, low level offences away from the criminal justice system and towards mental health services. Rather than arresting PMHI, law enforcement officers refer or transport individuals to community based facilities. A range of approaches have been developed and implemented around the world. Three distinct models are outlined below.

Models of police pre-arrest diversion
Police pre-arrest diversion interventions can be usefully categorised as one of three models (Deane et al., 1999; Hails and Borum, 2003), representing either police led or collaborative responses:

Police-Led Responses
- Police-based specialised police response

These interventions are police- led responses, solely using police officer expertise to identify PMHI, divert them away from the criminal justice system and connect them with formal mental health services. The Crisis Intervention Team (CIT) is the most common example of this type of response. The CIT programme provides an intensive period of training for police officers in order that they can recognise individuals with symptoms of mental illness, diffuse potentially inflammatory situations and link PMHI to appropriate support or treatment. The CIT programme was developed in Memphis, Tennessee in 1988 through a collaboration of the National Alliance on Mental Illness (NAMI), the Memphis Police Department, and community stakeholders (hence the CIT programme is commonly referred to as 'The Memphis Model'). CIT training is now replicated in thousands of agencies across the world (Taheri, 2014; Kohrt et al., 2015).

Co-Response (collaboration between police and other agencies)
These interventions include co-responding teams of police and mental health professionals. There are two co-response models, distinguished by the location of the mental health professionals and the agency that is responsible for them. 'Police-based specialised mental health response' refers to the integration of mental health professionals within the police as they are formally based in the police department. 'Mental-health based specialised mental health response' refers to mental health professionals that are based in the mental health services and remain institution separate from the police:
- Police-based specialised mental health response

Mental health professionals are embedded within the police department, providing on-site and telephone support to police officers as well as attending incidences involving PMHI. The Community Service Officer (CSO) Program, originating from Birmingham USA, is often cited as an example of this type of response. CSO units are located within major police departments, responding to all social work related issues including mental health (Butler, 2014).
- Mental-health based specialised mental health response

This type of intervention is similar to model two outlined above but the police and mental health services remain institutionally separate. There are many examples of this approach that demonstrate collaborative, but independent, working between the police and other agencies.

Three interventions from the UK are described here. The Street Triage scheme in England is an example of this type of intervention. The majority of the schemes have mental health nurses 'on call' to police officers to provide on-site or telephone assistance whilst they remain independent of the police department (Irvine et al., 2015). Another example from the UK is the national Liaison and Diversion scheme. This is a broader intervention with diversion strategies available along the criminal justice pathway (not solely before arrest) and for a wider range of needs (not only mental health issues). The scheme allows agencies, including the police, to identify individuals in need of services and refer them to a practitioner for subsequent screening and assessment (Disley et al., 2016). Link schemes are a further example of a co-responding effort. Police teams identify potential individuals with mental health needs during, or prior to, an incident in the community. Officers then refer the PMHI to a Link Worker who identifies their needs and seeks to identify sources of support or treatment (Accendo, 2012).

Different models have been adapted and implemented in different parts of the higher-income world. Within the US, police led CIT models have been the principal response. In Canada, the dominant diversion approaches have been co-responding. In Australia, police forces from different states have been making significant developments in both police-led and co-response models (Reuland et al., 2012). Within the UK, co-responding approaches are most prevalent with a particular emphasis Liaison and Diversion programmes.

Research background

The evidence base surrounding pre-arrest diversion interventions has been developing in recent years, in both quantity and quality. In 2006, Hartford et al (2006: 849) reported that literature was 'mainly descriptive and not evaluative' with existing studies unable to assess long term, comparable outcomes and without the study designs necessary to draw strong conclusions. Ten years later and there is a larger pool of studies with wide variations in focus, approach and methods (Vigurs et al. forthcoming).

Reflecting the surge of diversion programmes in the US, the evidence base has expanded with studies primarily from North America (Parsonage, 2009). The popularity of CIT approaches in the USA means that there is a significant amount of research on CIT models but a lack of studies on co-responding pre-arrest diversion strategies (Shapiro et al., 2015). Within the literature, there has been a growth in outcome evaluations, primarily using quasi-experimental designs (Taheria, 2014). However, these suffer from a number of methodological limitations including small sample sizes and a high risk of bias (Compton et al., 2008; Paton et al., 2016; Scott et al., 2013; Shapiro et al., 2015; Sirotich, 2009) and often fail to examine longer term outcomes (Parsonage, 2009; Sirotich, 2009). A few systematic reviews have attempted to synthesise these studies. The findings from these reviews suggest that there is some evidence that pre-arrest diversion interventions do reduce immediate arrest rates (at the scene of the incident) (Compton et al., 2008; Paton et al., 2016; Shapiro et al., 2015) although 'this is not surprising given that, by definition, the diversion associated with CIT [and other models] occurs at prebooking' (Compton et al., 2008: 52-53). There is, however, a lack of statistically significant support for such findings (Scott et al., 2013; Taheri, 2014). There is promising evidence that interventions have the potential to improve linkages between people with mental health issues and community services (Shapiro et al., 2015; Taheri, 2014) but a lack of longer term assessments of these impacts. Moreover, previous systematic reviews have typically focused on only one model of specialised police response, e.g. CIT. Current knowledge of the effectiveness of such interventions is therefore lacking despite the growing implementation of such programmes and the increase of primary studies in the field. To advance systematic review

level evidence in the field, synthesis of long term outcomes from the evaluation of any and all types of pre-arrest diversion programmes is necessary.

Conceptualizing the effectiveness of police pre-arrest diversion strategies
Pre-arrest diversion strategies are part of a wider diversion infrastructure that aims to identify and divert PMHI away from the criminal justice system. Conceptual models identify the multiple points at which PMHI can be intercepted along the criminal justice pathway (Munetz and Griffin, 2006; NHS England Liaison and Diversion Programme, 2014; Parsonage, 2009). It is widely agreed that PMHI should be intercepted at the earliest possible stage and so police pre-arrest diversion strategies have a central role to play in doing so (Bradley, 2009; Kane et al., 2012; Munetz and Griffin, 2006; Parsonage, 2009). All-stages diversion models also highlight that pre-arrest diversion is a significant but single element of a wider diversion infrastructure (Parsonage, 2009).

There is limited theoretical or empirical analysis of *why* or *how* police pre-arrest diversion schemes are deemed to work (Shapiro et al., 2015). Drawing on existing research and theory, an initial conceptual model of the effectiveness of police pre-arrest diversion strategies is presented in Figure 1. The diagram sets out the main steps that are common to the process of pre-arrest diversion interventions (blue horizontal arrows), the key causal mechanisms for enabling these steps to work (purple vertical arrows) and crime/ mental health outcomes for the PMHI (blue rectangular boxes). Further explanation of the model is outlined below.

Figure 1: Conceptual model of the effectiveness of police pre-arrest diversion interventions
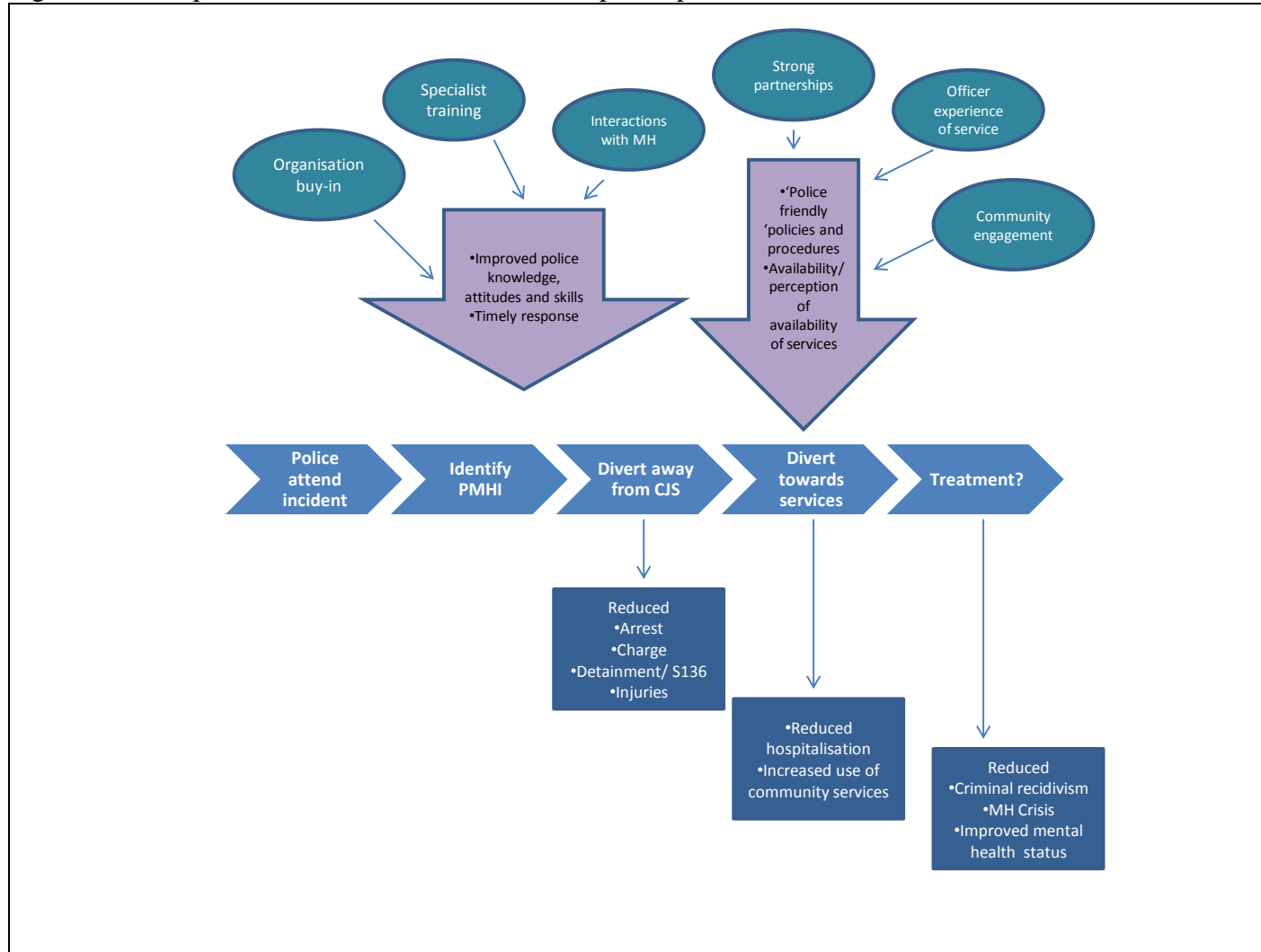
The elements common to most police pre-arrest diversion strategies are plotted in the centre of the diagram (blue horizontal arrows). It is recognised that these steps are not necessarily carried out separately but it is helpful to examine the underlying mechanisms contributing to each step. Initially, police officers respond to an incident (whether alone or in partnership with mental health professions) and follow a process for **identifying PMHI** (whether undertaken by the officer or co-responding professional). Following identification, the PMHI will be **diverted away from the criminal justice system** (the officer actively deciding not to arrest or detain the PHMI). Officer knowledge of mental health issues, tolerant attitudes towards PMHI and skills in de-escalation are considered to improve interactions and the identification and diversion of PMHI (Compton et al., 2008; Shapiro et al., 2015; Steadman et al., 2001; Watson, 2008). Such attitudes and skills may be developed through many pathways: specialist training, interaction with mental health professionals, or prior experience of PMHI. In deciding to divert away from the criminal justice system, there is expected to be a reduction in immediate levels of arrest, charge and detainment of the PMHI. Moreover, improved understanding of mental health issues is understood to improve interactions and reduce the use of police force (and resultant injuries for PMHI) (Compton et al., 2008). The officer or co-responding professional will **divert the PMHI to services** in the community. The likelihood and outcomes associated with this referral is linked to a number of mechanisms: availability of responsive mental health services and police officer perception of availability of linkage services (Watson et al., 2008), 'police friendly' policies and procedures (Steadman et al., 2001) including no refusal policy for police cases (Hartford et al., 2006) and quick turnaround (Steadman et al., 2001). Many factors may inform the presence of these mechanisms: strong partnerships between police and all relevant mental health agencies (Shapiro et al., 2015), a liaison to co-ordinate between different agencies (Hartford et al., 2006; Steadman et al., 2001), community engagement (Shapiro et al., 2015), and officer past experiences of service availability. In referring PMHI to community services, it is hypothesized that there would be a reduction in hospital admissions for people in crisis by diverting to them to other more suitable treatment options (Shapiro et al., 2015). Following the referral to services, PMHI are expected to receive **treatment** for their mental health problems. By linking PMHI specifically to community services rather than psychiatric hospitals, it is anticipated that user engagement with treatment will be improved (Shapiro et al., 2015) and there is indicative evidence to support this assumption (Paton et al., 2016). Such treatment is considered to lead to longer term benefits for PMHI whose mental health would improve and so criminal recidivism would reduce.

The economic case for pre-arrest diversion
In economic terms, pre-arrest diversion has multiple potential outcomes that could combine to produce a positive incremental net benefit to society, *if* the incremental value that accrues from any beneficial effects of intervention, such as crime reduction or improvements in participants' mental health, exceeds the incremental costs of providing the service, compared with alternatives and over a time horizon that is sufficiently long to capture all important costs and effects. Incremental value deriving from the beneficial effects of pre-arrest diversion could plausibly produce net savings both in the short-term (e.g. flowing from reductions in the immediate use of criminal justice services following offences just committed) and in the longer-term (e.g. flowing from reductions in future use of criminal justice and/or community mental health

services, if fewer offenses were committed and/or if [sustained] improvements in mental health were realised).

However, because pre-arrest diversion involves diverting people from the criminal justice system into mental health services, this will inevitably shift resource use and associated costs of treatment in the same direction in the short-term (albeit the short-term costs of treatment of offenders by community mental health services might be expected to be lower than those of treatment by alternative pathways through the criminal justice system). In other words, up-front costs and cost savings can be expected to accrue disproportionately over time within and across these two adjacent systems. Therefore, the extent to which pre-arrest diversion is judged favourably from an economic perspective is likely to depend on: overall impacts on short- and longer-term incremental costs (resource use) and effects within and across the criminal justice and health and social care systems; the distribution of incurred costs (including those associated with changes in resource use flowing from the effects of the intervention) between these two systems; and the perspective of those making the resource allocation decision (i.e. whether the decision makers have a remit to consider impacts on resource use and associated costs in one, or both, of these systems).

Diverting people away from the criminal justice system and into (potentially lower cost) treatment pathways in mental health services is also likely to shift the risks of serious adverse events (for example, deaths in custody or treatment) associated with treating people with mental health issues in same direction. However, if the risks of such adverse events were lower among diverted, compared with non-diverted, offenders (for example, due to the greater prevalence of special expertise to respond to people in crisis among mental health practitioners, compared with police), then associated costs would also be lower among the diverted group. In addition, from an economic perspective, the possibility of implementing pre-arrest diversion for people with mental health issues will in practice depend on the capacity of community mental health service organisations to offer timely access to alternative treatment pathways; which is, in turn, contingent on having sufficient capacity – and funding – available within the mental health care system to meet this demand.

Aims and approach
This systematic review forms part of a larger project that identifies and describes empirical research on policing responses to people with mental health problems (Vigurs et al., forthcoming). A sub-set of studies from this project were identified and screened for inclusion in this systematic review. Using these studies, this review addresses multiple questions about police pre-arrest diversion interventions. To do so, this review uses the EMMIE systematic review appraisal framework (Johnson et al., 2015) to structure the overall approach. Therefore, this systematic review addresses the following questions:

- **E**ffects: What is the impact of police pre-arrest diversion of people with mental health issues on subsequent crime and mental health outcomes?
- **M**echanisms: What are the mechanisms associated with effective pre-arrest diversion?
- **M**oderators: Under what conditions or for what population groups might pre-arrest diversion work best?

- **I**mplementation: What factors that can facilitate or impede the implementation of pre-arrest diversion?
- **E**conomics: What are the economic impacts of pre-arrest diversion?

Scope and definitional issues

Dual diagnosis: An individual presenting with both mental health issues and substance misuse is said to have a 'dual diagnosis'.

People with mental health issues (PMHI): The term 'people with mental health issues' is intended to be broad and encompass a wide range of mental health problems or issues. The definition of 'mental health issues' used in this review draws on the Mental Health Act 2007 which stipulates that mental ill health refers to 'any disorder or disability of the mind'. This understanding is intended to include people with mental health difficulties whether or not they have had a formal diagnosis and recognises that mental ill health is not a fixed state, but can change over time.

Police pre-arrest diversion: Police officers use their discretion to divert individuals suspected of non-violent, low level offences *away* from the criminal justice system and *towards* mental health services. Rather than arresting PMHI, law enforcement officers refer or transport individuals to community based facilities. Arrests and detentions under section 136 of the Mental Health Act (1983) are not considered a pre-arrest diversion strategy.

METHODOLOGY

Stakeholder/ user involvement in the review

To ensure the relevance and usefulness of this project, a range of users/ stakeholders were consulted in the process of developing the protocol. This group of users represents a range of policy, practice and academic perspectives with an interest in the area of policing and mental health review (see Appendix 1 for details).

There were two different user roles: a consultation role and an advisory role. The stakeholder consultation group provided verbal and email input at the initial stages of the project. Consultation with these members was principally undertaken on a one-to-one basis, via telephone, to identify and discuss key issues in the field (in terms of policy, practice and research). These discussions served to inform the development of the scope and direction of the systematic review.

Identifying and selecting studies

Defining relevant studies: Inclusion and exclusion criteria
Eligible studies were defined and identified according to the Inclusion criteria set out in Table 1.

Table 1: Inclusion Criteria

Category	Inclusion Criteria
Date	Published in or after 1995
Geography	Study conducted, or data collected, in an OECD (Organisation for Economic Co-operation and Development) country: Australia, Austria, Belgium, Canada, Chile, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Korea, Luxemburg, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States
Population	People with mental health issues (as defined above): Adults (age 18+) who are experiencing mental health issues, whether formally diagnosed or not. AND Have come into contact with the police/mental health professionals working with police and are eligible for arrest/ detainment
Intervention	Police pre-arrest diversion interventions:

	Diversion of PMHI: On attending an incident, police officers/ professionals working with the police identify people with mental health issues and choose to divert rather than arrest/ detain them. AND Referral to community services: Police/ professionals working with the police refer the PMHI to dedicated services in the community. These cover primary care services such as General Practice, social workers, community mental health nurses.
Comparison	No treatment or treatment as usual (i.e. not diverted) OR Alternative intervention (e.g. diversion after arrest)
Study Type	Experimental or quasi-experimental study. Quasi-experimental designs needed to include a comparable control group (or use propensity score matching). OR Cost analyses/ full economic evaluations (cost-effectiveness or cost-benefit analyses)
Outcome	Criminal justice and Mental health outcomes Crime: Any measure of criminal recidivism (e.g. arrest, charge, incarceration) Mental health: Any measure of mental health status or utilization of mental health services (e.g. counselling) Outcomes needed to be collected after the PMHI had been diverted to services.

The included studies will contribute to a range of different analyses. All included studies will contribute to the meta-analysis, providing outcome data for quantitative synthesis. Analysis of mechanisms, moderators and implementation issues will be carried out using the same set of included studies.

Full economic evaluations and cost analyses that meet eligibility criteria for the primary study synthesis in relation to participants, intervention and comparators, will be identified and used to inform development of an economic commentary. Cost analyses are studies that compare eligible interventions with comparators in terms of their costs

only. Data outputs from full economic evaluations and cost analyses include estimates of the impacts of interventions on resource use, costs and (in the case of full economic evaluations) estimates of intervention cost-effectiveness. The economic commentary will also draw experimental studies that report cost related information such as estimates of resource use or associated costs.

Search strategy and screening
A comprehensive search strategy was developed to identify studies that broadly related to police responses to people with mental health problems. Various search sources were used in the strategy.

The following electronic databases were searched: ASSIA; Criminal Justice Abstracts; Social Science Citation Index; Medline; Proquest Psychology. See Appendix 2 for the search terms used for these databases. These searches were supplemented by handsearching within key journals (Mental Health and Criminal Justice; Policing: A journal of Policy and Practice; Police Practice and Research: An International Journal) and a range of policing and mental health related websites and sources of 'grey' literature.

On completion of the search, all of the references were exported in to EPPI -Reviewer 4 (the EPPI-Centre's comprehensive online software tool for research synthesis) (Thomas et al., 2010). These references were then subject to a process of screening: the title and abstracts of all items identified in the search were manually screened against the inclusion criteria. For the website and journal hand-searching, the title and abstracts/ executive summaries of all potentially relevant items were manually screened during the searching process. This process was undertaken by a single reviewer. The full text of potentially eligible studies was then retrieved and a further round of screening was undertaken to ensure inclusion in the review.

Describing and assessing the quality of the primary studies for the in-depth review

Describing the primary studies
A coding tool was developed to collect substantive and methodological data from each included study. An adapted version of an existing EPPI Centre tool (2007) constituted the main part of a wider data extraction approach, modelled on the EMMIE framework (Johnson et al., 2015). This framework was developed from evaluation scales widely used in health and criminal justice, and developed further to include the information most useful for systematic reviews in the field of criminal justice. The EMMIE framework codes for the **E**ffectiveness of the intervention; the **M**echanism and mediators theorised to be at work, i.e. the theory of change for each programme; the **M**oderators that are likely to affect the response to the intervention; **I**mplementation issues in practice and any **E**conomic costs reported. The data extraction from the included studies was entered directly into the EPPI-Reviewer 4 database (Thomas et al. 2010). See Appendix 3 for the data extraction tool used.

Assessing the quality of the studies
The quality and relevance of included studies were assessed for the **E**ffects synthesis. Quality appraisal of studies was not undertaken to inform the analysis of other

dimensions of the EMMIE framework (Mechanisms, Moderators, Implementation, or Economics).

The three dimensions of the Weight of Evidence framework (Harden and Gough 2012) were used to structure the quality appraisal of the included studies. This approach develops an overall quality rating for each study based on the internal validity of the study (Weight of Evidence A), the appropriateness of the study in answering the review questions (Weight of Evidence B) and the relevance of the study to this review (Weight of Evidence C). The tools and approach used for each dimension are detailed below.

Weight of evidence A (Internal Validity):
An adapted version of a quality assessment checklist for quantitative intervention studies (NICE, 2012) was used to assess the internal validity of each study. This assessed selection bias, performance bias, attrition bias and detection bias (See Appendix 4). In order to determine an overall quality assessment for each study, the reviewers determined the most important domains for determining overall high or low risk of bias (Waddington and Hombrados, 2012). Selection bias was deemed to be particularly important for this review because the allocation of individuals to intervention or control groups in quasi experimental approaches are commonly based on police officers' discretion to refer to treatment (or not). The police officer's decision to divert some individuals and not others, therefore, had the potential to generate unobservable selection bias with potential impact on outcomes. Further guidance on determining selection bias for quasi experimental study designs was drawn from the quality assessment tool used in Baird et al (2013). The overall judgement of internal validity followed the NICE metric outlined below, with one stipulation: the overall assessment could not be higher than the selection bias judgement. An interpretation of this judgement was developed from a simplified version of an EPPI-Centre framework (see Tripney et al., 2013: 26).

Table 2: Interpreting Assessments of Internal Validity

Judgement	Guidance	Interpretation
++	Low risk of bias in all domains, including selection bias. All or most of the checklist criteria have been fulfilled, where they have not been fulfilled the conclusions are very unlikely to alter	Bias, if present, is unlikely to alter the results
+	Low risk of selection bias. Some of the checklist criteria have been fulfilled, where they have not been fulfilled, or not adequately described, the conclusions are unlikely to alter.	A risk of bias that raises some doubts about the results
-	High or unclear risk of selection bias.	Bias is likely to alter the results

	Few or no checklist criteria have been fulfilled and the conclusions are likely or very likely to alter.	

Weight of evidence B (Study appropriateness):
This dimension assessed the appropriateness of the study for addressing the review question. An overall judgement for each study was based on the study design, data collection and data analysis methods. The strengths and weaknesses of each study were identified and an overall judgement was derived.

Weight of evidence C (Study relevance):
This dimension was judged purely on the relevance of the intervention under study. The judgement was reached by assessing how far the intervention fitted with the defining features of a police pre-arrest diversion intervention (as it was understood within this review): 1) identifies people with mental health issues, 2) diverts individuals away from the criminal justice system, and 3) refers individuals to community services. An overall judgement was based on whether the interventions successfully fulfilled these criteria.

Overall study quality/ Weight of evidence
A score was developed for each of the three dimensions (A, B, C) using the following categories: - (Low), -/+ (Low/ Medium), + (Medium), ++ (High). WoE A was given greater prominence in deciding the overall judgement (in recognition of the potential for bias to alter results) so the overall study quality was an average of A, B and C but could not be higher than the assessment of A.

Judging the overall strength of evidence
In order to draw conclusions about the overall effects of pre-arrest diversions and address the review questions, a system was developed to grade the strength of evidence. This draws on the GRADE approach (Guyatt et al. 2008) and builds on similar frameworks used in other systematic reviews (e.g. Sutcliffe et al., 2014).

The strength of evidence was rated according to the quality of the study (based on overall Weight of Evidence) and the consistency of findings. Evidence was rated as:
Inconclusive: where evidence is only available from low quality studies (no matter how many or whether findings are consistent).
Tentative: where evidence is available from 1 medium/ high quality study or 2 low/medium or medium quality studies. With the latter, there should be consistency in findings. The studies have the potential to be corroborated by other studies.
Promising: where evidence is consistent and supported by 2 or more medium/high quality studies.
Strong: where consistent evidence is available from 2 or more medium/high or high quality studies.

Conclusions were then informed by statistically significant findings (reported for individual and summary effects) and the strength of evidence judgement.

Synthesis of evidence: effects

Selecting outcome data

Estimation technique
In both of the included studies, multiple analytical methods were used to generate outcome data. It was therefore necessary to make decisions about which estimation technique to select in order to identify data for synthesis. Our decision was informed by:

- The number and type of covariates used in the model: preference was given to those models that incorporated a rich set of covariates that included key factors that would likely affect outcomes (demographic variables, mental health status, substance abuse, prior contact with criminal justice system).
- The proportion of the sample included in the generation of outcome data: preference was given to techniques that used the entire (intervention specific) sample in generating outcomes in order to ensure comparability with other analytical models in the synthesis.

In the case of the Bonkiewicz et al. (2014) study, this meant that outcomes from the kernel matching technique were selected from the four different techniques used. All estimation techniques used the same set of covariates. The kernel approach uses all individuals in the sample, assigning more weight to those matches that are more similar. This technique could also be considered to be the more efficient approach used by the study (see Tripney et al., 2013: 31-32).

In the case of the interventions evaluated by Broner et al., (2004), all outcome data was selected from the Broner et al., (2004) report because these outcomes were generated by multivariate regression models (compared to the standardized regression models or propensity score models), incorporating a rich set of observed and unobserved covariates, and reported estimates for each intervention/ site.

Outcome measure

The included studies measured a single outcome construct (e.g. crime reduction) in more than one way (e.g. any incident of arrest and number of arrests). To select only one estimate per study in a single synthesis/ meta-analysis, we applied the following rules:

1. Prioritizing objective measures (such as official reports of arrest) over subjective, self-report measures (such as self-reported arrests).
2. If a single study presents multiple objective measures or only self-reported measures then we will prioritize outcomes using the hierarchy below.

Crime outcomes:
- Arrest (being taken into custody by police)
- Time to arrest
- Criminal charges (e.g. filing of a criminal case)
- Incarceration

- Time to incarceration
- Duration of incarceration
- Community safety (rates of crime and disorder, experiences of crime, and fear of crime in the community).

This uses the Sequential intercept model (Munetz and Griffin, 2006) to prioritize measurements of re-entry into the criminal justice system (such as arrest and time to arrest) and subsequent points that mark an individual's movement towards incarceration.

Mental health outcomes:
- Mental health status (i.e. mental wellbeing and illness as identified by diagnostic instruments and surveys)
- Mental health service utilization (e.g. counselling)
- Mental health medication

3. Selecting outcome measures that were conceptually comparable across studies.

Follow up time
Across the included studies, outcome data were collected at numerous points after the intervention: 6 months (Bonkiewicz et al., 2014), 3 and 12 months (Broner et al., 2004). It was decided to use the outcome data with the longest follow-up time in each synthesis due to expectation that outcomes of interest were longer term outcomes (as indicated in the conceptual model of the effectiveness of police pre-arrest diversion interventions, see Figure 1).

Calculating effect sizes
Effect sizes were calculated for each study where sufficient data allowed. Risk ratios (RR also known as Relative Risk) were the metric calculated for all outcomes and used in the meta-analyses. Risk Ratios are a methodologically valid metric for combining dichotomous outcomes (Higgins and Green, 2011) and provide a meaningful and easily understood metric (Grant, 2014).

The following steps were followed to calculate Risk Ratios for different study designs and data. For propensity score matching studies, RR was calculated with the following formula:

RR= Yt / (Yt-ATT)

Yt represents the raw impact of the intervention on the treatment group. ATT represents the average treatment effect on the treated.

For multivariate regression models:
Odds ratios were transformed into Risk Ratios using the baseline risk (of the control group) where reported. When this data were not available, an average baseline risk was used as a reasonable alternative (Grant, 2014; Higgins and Green, 2011). RR were computed using this calculator: http://clincalc.com/stats/convertor.aspx. Standard Errors were then calculated from P values and Risk Ratios and Standard Error were entered into EPPI Reviewer 4 for use in the meta-analyses.

Synthesis methods

A meta-analysis was undertaken when there were at least 2 studies/ sites with conceptually comparable outcomes and data. The data synthesis was carried out using random effects statistical models. This means that the effect sizes were weighted to give greater influence to larger studies using an inverse variance weight (Lipsey & Wilson, 2001). EPPI Reviewer 4 software was used for performing the overall meta-analysis.

Statistical heterogeneity measures the degree of variability between effect sizes estimated among different included studies beyond that which could be expected due to chance alone. Statistical heterogeneity may result from variability in the participants, interventions and/or outcomes studied, and/or from variability in study design and methods. If included studies are too different from each other, naïve interpretation of the summary result obtained from meta-analysis of effects data may generate spurious inferences. A statistical measure of heterogeneity (I^2) was calculated using the EPPI reviewer 4 meta-analysis software. The I^2 statistic describes approximately the proportion of variation in point estimates due to heterogeneity rather than sampling error (chance). We considered I^2 values less than 30% as indicating low heterogeneity, values in the range 30% to 60% as indicating moderate heterogeneity, and values greater than 60% as indicating substantial heterogeneity.

Moderator analyses

Moderator analyses were undertaken to examine the potential variability in effects due to study and intervention characteristics. Specified in advance of the meta-analysis, intervention type was used to determine sub group analysis. Other potential moderator variables that were coded and explored included the time to follow-up (when the outcomes were collected post intervention).

Missing data

When primary studies did not include sufficient information to estimate effect sizes, we contacted authors to try to obtain relevant missing data and additional reports. We did not impute missing effect sizes with one exception. For the calculation of effect sizes for studies using multivariate regression techniques, odds ratios were transformed into Risk Ratios using the baseline risk of the control group. When this data was not available, an average baseline risk was used as a reasonable alternative (Grant, 2014; Higgins and Green, 2011).

Publication bias analyses

Due to a small number of studies, we did not attempt to detect or exclude the existence of publication bias using statistical methods such as funnel plots. Without a sufficient number of studies, the power of such statistical tests is too low to differentiate chance from real asymmetry (Higgins and Green, 2011).

Synthesis of evidence: mechanisms, moderators and implementation

Two reviewers independently coded the mechanisms in each included study. The coding identified mechanisms that were 1) mentioned by the author as part of their understanding/ theorisation of how the intervention might work without supporting evidence from wider literature or their own study findings, 2) mentioned by the author and evidenced by reference to other studies, or 3) mentioned by the author and tested/

evidenced by their own study findings. Drawing on methods of thematic synthesis (Thomas and Harding, 2008), the text in the studies was coded line by line to identify references to how the intervention was expected to work. This text was then organised into 'descriptive' themes. This process entailed looking for similarities and differences across the coded text to identify and group the codes into higher level 'descriptive' themes. These themes were then further interpreted to generate overarching 'analytical' themes. To do so, the descriptive themes were organised and shaped into a narrative that provided a framework for understanding *how* the intervention might work.

A similar process was followed for the identification and synthesis of moderators and implementation issues. Two reviewers independently identified and described findings, references or analysis of potential moderation or implementation factors. The implementation issues were grouped into themes that pertained to particular stages of delivery of the intervention ('descriptive' themes). The identification and organisation of moderators was guided by two pre-existing categories: characteristics of study participants and characteristics of intervention providers.

Synthesis of evidence: economic commentary

An economic commentary was developed alongside – and placed in the context of evidence generated from – the synthesis of effects data (Shemilt et al., 2013, Shemilt et al., 2011). This integrated component of the systematic review drew primarily on identified economic evaluations that have assessed the impacts of police-delivered pre-arrest diversion for people experiencing mental health problems on resource use and/or costs, or their cost-effectiveness, versus eligible comparators. The economic commentary summarised what is known from different studies, conducted in different settings, about these economic impacts of pre-arrest diversion, to inform an understanding of the structure of resource allocation decisions and key economic trade-offs likely to be faced in choosing between this type of intervention and (i) 'no treatment' (treatment as usual), (ii) post-arrest ("post-booking") diversion, and (iii) alternative police first response interventions delivered to people experiencing mental health problems. Types of economic evaluations eligible for inclusion in this component of the review are described in Table 1 (Inclusion criteria: Study Type). To inform the economic commentary, evidence from economic evaluations conducted alongside outcome and/or process evaluations meeting eligibility criteria for the main review will be analysed in conjunction with relevant data (e.g. on intervention effects) extracted from linked study reports.

Given that pre-arrest diversion into treatment is expected to impact on resource use (and associated costs are expected to accrue) within and across the criminal justice and mental health care systems, the economic commentary will adopt a multi-sector perspective that includes both systems, to summarise the key characteristics and results of included economic evaluations. It will encompass consideration of both incremental resource use and costs used to implement pre-arrest diversion, and (where available) the monetized value of the effects of the intervention (e.g. changes in costs incurred within the criminal justice system as a result of crime reduction; changes in value deriving from beneficial effects, such as intangible costs of crime and changes in participants' mental health outcomes). The EMMIE 5 point rating scale for economic data (Manning et al 2015) will be applied to inform assessment of the degree to which

all relevant direct and indirect costs and benefits have been captured among included economic evaluations. Unadjusted estimates of costs and/or cost-effectiveness will be presented alongside information on the currency and price year used, and also (where appropriate and feasible) adjusted to a common currency and price year in order to facilitate comparison between studies (Shemilt et al, 2010).

Quality assurance processes

The inclusion criteria were developed and refined through a series of piloting exercises. This process involved all members of the review team applying the criteria independently, comparing decisions and resolving differences. Further guidance was then developed and the inclusion criteria were refined.

The data extraction tool was independently piloted by two members of the review team using a subset of studies. The reviewers met to discuss the data extraction process, and refine the guidance and codes used in the data extraction tool. This process served to develop a shared understanding of the data extraction tool/ codes and ensure consistency in its application. Using the finalised coding tool, the included studies were coded by two reviewers independently. Any disagreements or discrepancies in the coding were resolved through discussion and, where necessary, a third reviewer was consulted. Guidance on statistical issues was sought from wider members of the EPPI Centre.

The quality assessments were undertaken by two reviewers independently. The reviewers met to establish a consensus and agree the judgements. In cases of disagreement, input from a third reviewer was used to develop the final decisions.

RESULTS

A note on terminology used in this chapter: a 'study' refers to a piece of empirical research with specified methodology; a 'report' refers to a written publication detailing methods and/ or outcomes of a study. Therefore, a single empirical study may have one or multiple reports writing up the findings and/ or other elements of the intervention and research method. When a single study evaluates multiple interventions and sites with the same study design, data collection and analysis then this will be treated as a single study, with different arms/findings for each site/ intervention.

Descriptive overview of included studies
The original literature search identified 10,615 items from database searches and handsearching (see Figure 2). Following the initial screening process, 60 items were identified as potentially eligible studies on title and abstract. The full text was then retrieved and each item was subject to further screening against the inclusion criteria. This resulted in two included studies (Broner et al., 2004; Bonkiewicz et al., 2014), reported across a total of nine reports with four separate samples.

Figure 2: Flow of studies through the review

Items identified through handsearch of journals, websites and 'grey' literature (n= 531)	Items identified through database search (n=10,084)

Items identified in literature search (n=10,615)

Items screened on title and abstract (n=7,871)	Duplicates (n= 2,744) Items excluded on title and abstract (n= 7,811)

Items screened on full text (n= 60)	Items excluded on full text (n= 58) for following reasons: • Exclude population (n=2) • Exclude intervention (n=9) • Exclude study type (n= 14) • Exclude outcomes (n=33)

Included studies (n= 2)
Reports (n= 9)
Independent samples (n=4)

Police-led and co-response models of pre-arrest diversion were evaluated in the included studies. The interventions were all implemented in the USA. The evaluations took place in four distinct study samples. Police-led responses (CIT) were evaluated in samples from Memphis, Tennessee and Portland, Oregon, and co-responding responses (Link Scheme and Crisis Outreach Team) were evaluated in Lincoln, Nebraska and Philadelphia, Pennsylvania respectively. There was no evaluation of a co-response model that integrated mental health professionals into the police department (defined above as 'police-based specialised mental health response').

One included study (Bonkiewicz et al., 2014) evaluated a Link scheme and the other study (Broner et al., 2004) evaluated models of CIT and a Crisis Outreach team. This latter study was reported across a total of eight reports. For the purposes of this systematic review, Broner et al. (2004) will be considered as the main study report, with the other seven reports providing additional details as necessary. This study was part of a large multi-site evaluation of nine criminal justice diversion programs (both pre and post-arrest), funded and implemented by the Substance Abuse and Mental Health Services Administration (SAMHSA). SAMHSA is an agency within the US Department of Health and Human Services that aims to reduce the impact of substance abuse and mental illness on America's communities. Figure 3 illustrates the different arms of the SAMSHA program. This review is only interested in the pre-arrest interventions and sites (identified in the blue arm). These interventions targeted individuals with co-occurring serious mental illness and substance use disorders

Figure 3: SAMSHA program: interventions and sites
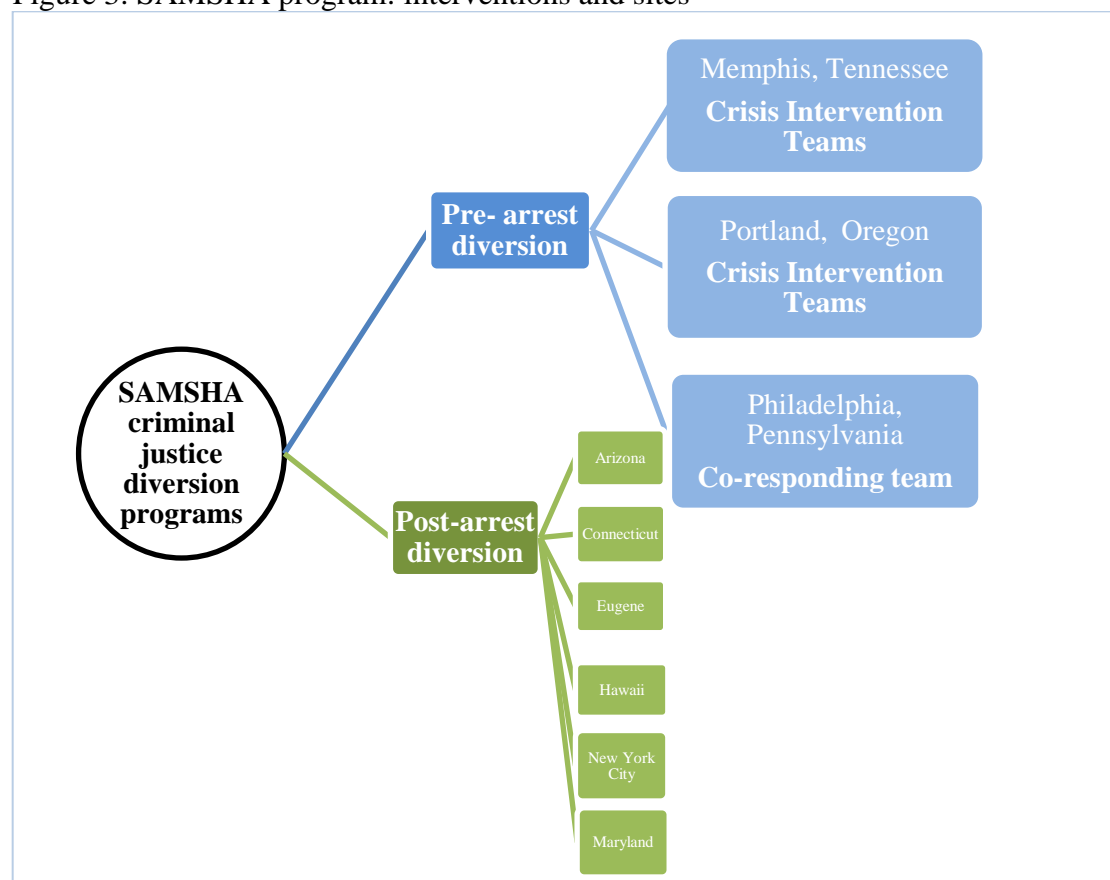
Evaluations of the pre-arrest diversion arm of the SAMSHA program were reported across eight reports. Six of these reports were used to inform the data extractions: four included site and intervention specific information and outcomes (Broner et al., 2004; Cowell et al., 2004; Lattimore et al., 2002; Gratton et al., 2001) and two reports provided additional details on the intervention and study (Lattimore et al., 2003; Steadman et al., 2001). Two reports were not used for the data extractions (Steadman and Naples, 2005; Naples and Steadman, 2003) because they did not include site-specific outcomes, site specific sample data or provide additional details on the intervention (See Appendix 5 which outlines the different study reports that contributed to the evaluation and data extractions).

Police pre-arrest diversion interventions
The two included studies evaluated one police-led response (CIT) and two co-response models (Link Scheme and Crisis Outreach team). All of these interventions can be characterized by three dimensions: 1) identification of people with mental health issues by police/ diversion staff, 2) diversion away from criminal justice system (population suspected of a crime but not arrested), 3) referral to community-based treatment and services. This section provides a narrative summary of each pre-arrest intervention and a comparison of the different interventions (See Table 3 for outline of characteristics of the pre-arrest interventions).

Police-led interventions: Crisis Intervention Teams
CIT interventions were implemented in two different study sites and evaluated by Broner et al. (2004).

CIT: Memphis, Tennessee (Broner et al. 2004)
The Memphis Police Department (MPD) and the University of Tennessee Psychiatric Emergency Service collaborated to deliver the pre-arrest diversion program. The intervention had two key features: CIT training for police officers and a Crisis Triage Centre (CTC). Experienced patrol division officers volunteered to undergo intensive training. On responding to incidences, CIT officers had the option of diverting the individual to the local CTC rather than arresting them. Seeking consent from the individual for the diversion is not required. The CTC is located in the emergency department of the regional medical centre and operates a no-refusal service to police officers. Officers transport the individual to the centre and then there is a 15-30 minute turnaround time. The diverted individual will then be assessed by an emergency room psychiatrist and linked to mental health and substance abuse services in the community.

CIT: Portland, Oregon (Broner et al. 2004)
The pre-arrest diversion program included multiple elements: CIT training for police officers, a 24 hour community based mental health Crisis Triage Centre (CTC), a Case Manager to support diverted individuals and a 'Boundary Spanner' to facilitate multi-system co-operation. Three agencies collaborated in the implementation of this program in Multnomah County: The Portland Police Bureau (CIT program), Providence Health System (CTC), and the Behavioural Health Division of the Multnomah County Community and Family Services Department (the local behavioural health authority). Prior to this specific intervention, there was a longstanding effort within Multnomah

County to develop interventions to improve the police response to people in mental crisis. An Alliance was formed in 1994 which organised and promoted CIT training and established the Crisis Triage Centre in 1997. The County was chosen to be part of the SAMSHA program in 1997. The CIT program, based on the Memphis model, delivered 40 hours of training to volunteer police officers from each Uniform Patrol Precinct. Once certified, the officers were dispatched to incidences that included a person experiencing a mental health crisis or a person who had been arrested and exhibiting signs of mental illness (and potentially substance abuse). These officers then had discretion to divert such individuals from jail and into community based care for minor or low-level crimes. Once identified for diversion (consent for diversion is not required), individuals would be transported to a Crisis Triage Centre- a 24 hour community based mental health centre. This centre provided a 'one-stop centralized crisis service for law enforcement officers' (Steadman et al. 2001: 220), with a no-refusal policy for police referral. The officer provided the necessary information to the centre staff, filling out a form and then returning to duty within 30 minutes. The diverted individual is then subject to a mental health assessment, performed by a Crisis Centre Nurse. Following on from this assessment, the CTC then provided a range of services to the individual including: mental health services, respite care, and referrals to other providers. There are no sanctions if the individual fails to comply. The Case Manager at the CTC provided short-term follow through care to help the individuals develop a longer term mental health/ substance diversion plan. A 'Boundary Spanner' acted as a liaison among all services and facilities.

Co-response interventions: Link Scheme and Crisis Outreach Team

Link scheme (Bonkiewicz et al., 2014)
A Post-Crisis Assistance Program (PCAP) was developed and implemented by the Lincoln Police Department (LPD) in Nebraska, USA. The Program encourages police officers to identify individuals experiencing a mental crisis or with untreated or undiagnosed mental health issues and refer them to appropriate treatment and services. The majority of police officers had undertaken CIT training (65% of officers in the intervention group and 80% in the control group). The LPD police officer makes a referral to the Mental Health Association (MHA) of Nebraska. A 'peer-specialist' (consumers who have personal experience of developing their own long-term mental health plans) contacts the consumer within 24-48 hours and makes them aware of relevant mental health and non-clinical services. Peer-specialists routinely help consumers, for example, to identify mental health professionals, overcome challenges to accessing treatment, secure employment, find housing and obtain substance abuse resources. The support provided by a peer-specialist is intended to help the consumer to take the initial steps towards developing a long-term mental health plan. PCAP is free to the consumer and involvement is voluntary. PCAP is a collaboration between the LDP and MHA of Nebraska, and intended to complement other police led mental health interventions such as Crisis Intervention Training (CIT).

Co-responding Teams, Philadephia (Broner et al., 2004)
The pre-arrest diversion intervention is run by Montgomery County Emergency Services (MCES: a private, not for profit organisation), a freestanding psychiatric hospital that provides a range of services for the county: crisis intervention, telephone helpline, mobile crisis outreach and referral to treatment. The pre-arrest diversion program included the following elements: a Mobile Crisis Outreach, a Crisis Triage

Centre, staff who act as 'Boundary spanners' between systems, and Case Management to link clients with services. On encountering an incident involving a person suspected of mental health issues, the attending police officer can transport the individual to the Crisis Triage Centre (MCES) or request an ambulance or Crisis Outreach team (consent for diversion is not required). This team includes emergency medical technicians and psychiatric crisis specialists. When directly transferring individuals to MCES, police officers spend an average of 20 minutes dropping off the individual and the centre has a no-refusal policy for officers. There are no sanctions if the individual fails to comply with the services.

How distinct are each of the models included in this review? The CIT and Crisis Outreach team models were similar in their referral practices (both referring to Crisis Triage Centres and offering a separate assessment for PMHI) but the Crisis Outreach team did not include an element of training for police officers (a defining feature of the CIT models). The CIT models varied with one site (Portland) also having a Boundary spanner and case manager. CIT training was also a feature of the Link Scheme as the majority of officers had received such training. However, the Link scheme was also quite different in that diversion to services was undertaken via email (rather than physically taking the individual to services), there was no separate assessment of PMHI; no immediate connection to treatment services and a peer specialist provided support rather than a dedicated mental health professional.

Table 3: Characteristics of the police pre-arrest diversion interventions

Intervention details	Identification of people with mental health issues suitable for diversion	Diversion away from criminal justice system	Referral to community services
Type: Co-response**,** Mental-health based specialised mental health response, 'Link Scheme' Lincoln, USA **Funder**: LPD and MHA **Year intervention started**: Not reported **Report:** Bonkiewicz et al (2014)	**Who identifies**: Police officer **How**: Interaction **Where**: On the street **Training**: 65-80% of police officers had undertaken CIT training	**Decision maker**: Police officer **How diverts:** Send an email to Mental Health Association **Separate assessment:** No	**Community services:** Information about available mental health services (e.g. doctors, pharmacists, therapists) **Who provides:** Peer specialist **Training:** Not reported **Duration:** Not reported **Longer term Follow up:** Not reported

Intervention details	Identification of people with mental health issues suitable for diversion	Diversion away from criminal justice system	Referral to community services
			Additional info: No
Type: Police response, 'CIT' Memphis, USA **Funder**: SAMSHA; City of Memphis, Medicaid, Medicare, Private funding sources **Year intervention started**: 1988 **Report:** Broner et al (2004)	**Who identifies**: CIT Police officers **How**: Through interaction **Where**: On the street **Training**: Yes, CIT training	**Decision maker**: CIT Police officer **How diverts:** Transports individual to Crisis Triage Centre **Separate assessment:** Yes, by ER psychiatrist	**Community services:** Mental health services and referrals to other providers. **Who provides:** Crisis Triage Centre staff **Training:** Not reported **Duration:** Not reported **Longer term follow up:** No **Additional info:** No
Type: Police response, 'CIT' Portland, USA **Funder**: SAMSHA; Multnomah County Behavioural Health, Medicaid **Year intervention started**: 1997 **Report:** Broner et al (2004)	**Who identifies**: CIT Police officers **How**: Through interaction **Where**: On the street **Training**: Yes, CIT training	**Decision maker**: CIT Police officer **How diverts:** Transports individual to Crisis Triage Centre **Separate assessment:** Yes, by Nurse	**Community services:** Mental health services, respite care, and referrals to other providers. **Who provides:** Crisis Triage Centre staff, Case Manager **Training:** Not reported **Duration:** Not reported

Intervention details	Identification of people with mental health issues suitable for diversion	Diversion away from criminal justice system	Referral to community services
			Longer term follow up: No

Additional info: Boundary Spanner |
| **Type:** Co-response, Mental-health based specialised mental health response, 'Crisis Outreach team' Philadelphia, USA

Funder: SAMSHA; County Mental Health Authority

Year intervention started: 1992

Report: Broner et al (2004) | **Who identifies**: Police officer, Mobile Crisis Outreach team

How identifies: Observation, information system screening

Where: Anywhere

Training: Not reported | **Decision maker**: Police officer

How diverts: Transport to CTC or request ambulance/ Crisis Outreach team to attend

Separate assessment: Yes by Crisis Staff, psychiatrist | **Community services:** Mental health services, substance abuse treatment, referral to services

Who provides: MCES staff (Crisis Triage Centre and psychiatric hospital)

Training: Not reported

Duration: Not reported

Longer term follow up: No

Additional info: Boundary Spanner |

Methodological characteristics of included studies
The evaluation of the Link scheme intervention by Bonkiewicz et al. (2014) was undertaken by a team of researchers who were affiliated to the organisations that implemented the intervention. The evaluations of the SAMSHA pre-arrest diversion interventions were undertaken by an independent team, with researchers based at universities or research organisations (Broner et al., 2004).

Both studies used a quasi-experimental design to assess the effectiveness of pre-arrest diversion interventions, comparing two groups of study participants (see Appendix 6 for a summary of the methodological features of the included studies). The intervention group included individuals identified with mental health issues and diverted to

community services and the control group included those individuals identified with similar issues but not diverted and so receiving 'treatment as usual'.

For both studies, the recruitment of participants into the groups was based on the identification of diverted/ not diverted individuals over a given time period: October 1998 to May 2000 for the SAMSHA sites (Broner et al., 2004) and August to December 2012 (Bonkiewicz et al., 2014). For the SAMSHA sites, allocation to the intervention group included all individuals that had come into contact with the police due to an incidence of low level crime, were diverted to community services by officers (or diversion staff for the Philadelphia site) and who met a set of eligibility criteria: over 18 years old, have a serious mental illness and substance abuse problems, be competent to give consent and be willing to receive treatment (Lattimore et al., 2002: 6). The control group included individuals that met the same eligibility who were not diverted but arrested and incarcerated. Individuals for the control group were identified in jails. For the Bonkiewicz et al. (2014) study, the intervention group included all individuals who had experienced a police-abated mental health crisis, had data reported on this contact on the Lincoln Police Department database and were referred to mental health services by the attending police officer. Those individuals that were not diverted during the same period constituted the control group. Individuals were excluded from the sample if, during the data collection period, they were arrested, incarcerated, committed to a mental health facility or died. An incentive was offered to participants involved in the SAMSHA studies, an average of US$60 was paid to each individual for participating in the study (including the attendance of the two follow-up interviews) (Broner et al., 2004). No consent or incentives were explicitly reported by Bonkiewizc et al (2014).

In the absence of random allocation to groups, both studies took measures to try to minimise selection bias. Broner et al. (2004) acknowledged that 'diverted and non-diverted groups differed significantly on a number of key measures at baseline' (Lattimore et al., 2002: 22-23) and so used three types of modelling approaches to estimate effects and provide some statistical control for a priori group differences. These included regression models, propensity score models and mixed regression models. Multivariate analysis (mixed regression models) was the estimation technique used to generate the outcomes reported for each intervention site in Broner et al (2004.) and so used in this review. This approach controlled for three sources of bias including "selection bias due to omitted observed variables and unobserved variables and maturation bias" (Lattimore et al., 2002: 39). Bonkiewicz et al (2014) used propensity score matching to balance the treatment and control groups, using four different matching techniques in estimating effects. This study measured average treatment effects on the treated (ATT): the differential impact that the treatment showed for individuals who participated in the Link Scheme.

Both studies measured a range of criminal justice, mental health and other outcomes (see Appendix 7 for summary). Broner et al. (2004: 525) used a one to one interview which 'consisted of a variety of self-report measures, including demographic, psychosocial, service utilization, housing, and criminal justice questions' and imbedded standardized tests to assess mental health (Colorado Symptom Index; Mental Health Scale from the SF-12), physical health (Physical Health Scale of SF-12) and quality of life (Lehman Quality of Life Interview). Interviews were undertaken at baseline, 3 months and 12 months following intake on the study. Bonkiewicz et al. (2014) used

official records (Lincoln Police Department database) of arrest, mental health calls for service (the number of times that police were called to respond to a mental health issue for the individual) and any record of the use of emergency protective custody. The other outcome measures assessed by the study included official reports of whether the individual had been taken into emergency protective custody. Measurements were taken six months after the initial police-abated mental health crisis.

Quality and relevance of included studies

Judgements of the quality and relevance of included studies are outlined in Table 4. Following the process of quality appraisal, studies were judged to be low quality for crime and mental health outcomes. The main weaknesses of both studies were judged to include selection bias (studies unable to account for the potential of unobserved variables influencing the effects), and performance bias (lack of blinding of participants and practitioners and unclear if groups were treated equally). The studies were judged to have low detection bias for certain outcomes: crime (Bonkiewicz et al. 2014) and mental health status (Broner et al., 2004) where official records or validated tools were used. Crime outcomes and service utilisation in Broner et al. (2004), however, were assessed by self-report measures and so judged to be at risk of high detection bias. The level of attrition bias was judged to be unclear for Bonkiewicz et al. (2014) and low for Broner et al. (2004), with the latter undertaking adequate analyses to assess the impact of study attrition across intervention and control groups.

Table 4: Quality Assessment of Included Studies

Study and Site	Overall study quality	A	B	C
Bonkiewicz et al (2014)	-	-	-/+	-
Broner et al (2004) Memphis	-	-	-/+	++
Broner et al (2004) Portland	-	-	-/+	++
Broner et al (2004) Philadelphia	-	-	-/+	+

Key: - Low; -/+ Low/ Medium, + Medium, ++ High

Study participant characteristics

All participants, in both studies, had mental health issues. In Broner et al. (2004), the population had a diagnosable severe mental illness that included one of the following disorders: Schizophrenia, Bipolar, Major Depression, or Psychosis. The evaluation sample also had a substance use disorder as defined by MAST/ DAST scores. In Bonkiewicz et al. (2014: 767), the sample did not necessarily have a diagnosable mental health illness but had experienced a mental health crisis that was identified as such and attended by the police ('e.g. a suicide attempt, acting out, self-reported requests for assistance by consumers'). Some of the study participants did report living with mental health disorders (total number of the sample unreported). The study did not have a measure for substance abuse but reports that 'a review of the police reports indicates that the majority of consumers were either exhibiting symptoms of substance dependency or reported a history of substance abuse' (Bonkiewicz et al., 2014: 773).

The demographic characteristics of the sample in the Bonkiewicz et al. (2014) study included male and female participants, representing different ethnic groups (White, Black, Asian, Hispanic, Other) and an average age of 36 years old (at time of crisis). A proportion of the sample also reported a 'transient status'. The intervention group included a higher proportion of women and White participants.

There is limited site-specific data available for the SAMSHA evaluations. Most reports describe the characteristics of the total sample that was used in the whole program (including pre and post-arrest diversion sites) (such as Broner et al., 2004) or only report on the intervention group (such as Lattimore et al., 2003). Therefore, it is only possible to outline the demographic characteristics of study participants in two of the three pre-arrest sites: Portland and Memphis. For Portland (CIT), the majority of the sample was male. Over half of the sample was White, with remaining participants classified as Black/African American, Mixed Race, American Indian/ Alaskan Native, and Hispanic/ Latino. Less than 3% of the total sample contained participants identified as Asian, Native Hawaiian/ Pacific Islander, and Other. The intervention group contained more American Indian/ Alaskan Native participants but less Mixed Race individuals than the control group. The average age of the total sample was 35 and participants had attained, on average, 12 years of education. The intervention group had a significantly higher level of education compared with the control group (Gratton et al., 2001: 9). In Memphis, study participants were typically in their mid-late 30s, included men and women and from various ethnic groups. The control group had a high proportion of male participants (88%). The majority of the sample (72%) was African American (Cowell et al., 2004).

Effects: The impacts of police pre-arrest diversion on crime and mental health

This synthesis examines the impact of pre-arrest diversion interventions for people with mental health issues on subsequent crime and mental health outcomes. The synthesis of crime outcomes is structured according to two outcomes (following the prioritised list outlined in the Methodology chapter): arrest and 'other' crime outcomes. Numerical syntheses, where possible, are reported. Effect sizes were calculated for each of the four included studies for the 'arrest' outcome. These have been statistically combined using meta-analysis techniques. The studies by Broner et al (2004) appear in the same meta-analysis, specifying the specific site/ intervention. The synthesis of mental health outcomes is structured according to two outcomes (following the prioritised list outlined in the Methodology chapter): mental health status and mental health service use. Statistical meta-analyses, where possible, have been conducted.

All meta-analyses were inverse variance weighted using random effects statistical models. The results of each meta-analysis are presented in a forest plot.

Evidence summary

A summary of the evidence for all interventions and outcomes (at longest follow-up period) is provided in Table 5. Statistically significant findings are highlighted in bold. Table 7 reports the direction of effect found by individual studies: identifying whether any of the risk ratios calculated for individual studies report a positive effect (improving outcomes for the intervention group compared to the control) or negative effect (leading to poorer outcomes in the intervention group compared to control). The 'overall direction of effect' is based on the pooled evidence, from the meta-analyses. These findings are based on data reported at the longest follow up period. The 'strength of evidence' indicates how much confidence we can have in the overall findings, based on the extent and quality of the studies.

Table 5: Summary of findings from individual studies and meta-analyses, at longest follow-up

Intervention	Outcome measure	Direction of effect (individual studies)	Overall direction of effect (meta-analysis)	Strength of evidence
Evidence of reducing crime?				
All	Arrest	**Negative**/ Positive	Negative (Increased risk of arrest)	Inconclusive
Police led (CIT)	Arrest	**Negative**	Negative (Increased risk of arrest)	Inconclusive
Co-responding (Link Scheme and	Arrest	Negative/ Positive	Negative (Increased risk of arrest)	Inconclusive

Crisis Outreach team)				
Evidence of improving mental health outcomes?				
All	Mental Health Counselling	Negative/ Positive	Positive (increased likelihood of receiving of counselling)	Inconclusive
Police led (CIT)	Mental Health Counselling	Positive	Positive (increased likelihood of receiving of counselling)	Inconclusive
Co-responding (Crisis Outreach team)	Mental Health Counselling	Negative	-	
ALL	Mental Health Medication	**Positive**	**Positive** (increased likelihood that medication prescribed)	Tentative
Police led (CIT)	Mental Health Medication	Positive	**Positive** (increased likelihood that medication prescribed)	Tentative
Co-responding (Crisis Outreach team)	Mental Health Medication	**Positive**	-	
ALL	Mental Health Hospitalisation	Negative	Negative (increased risk of hospitalisation)	Tentative
Police led (CIT)	Mental Health Hospitalisation	Negative	Negative (increased risk of hospitalisation)	Tentative
Co-responding (Crisis Outreach team)	Mental Health Hospitalisation	**Negative**	-	

Crime: Arrest

All four sites/ interventions in the included studies reported a post intervention outcome measure for arrest (self-report). The risk ratio/ relative risks calculated from the findings of each study are presented in Table 6, with statistically significant effect sizes highlighted in bold.

Table 6: Risk Ratios for any arrest, by follow up time

Site	RR 3 months	RR 6 months	RR 12 months
Memphis (CIT)	0.983 (95% CI 0.50-1.92)	-	1.369 (95% CI 0.54-3.48)
Portland (CIT)	2.252 (95% CI 0.81-6.27)	-	**2.982 (95% CI 1.00-8.89)**
Philadelphia (Crisis Outreach team)	4.32 (95% CI 0.80-23.45)	-	2.046 (95% CI 0.14-29.71)
Lincoln (Link Scheme)	-	0.68 (95% CI 0.08-5.82)	-

At 12 months follow-up, all three sites of the Broner et al (2004) study found that the intervention group had an increased risk of arrest following pre-arrest diversion compared to the control group. In the shorter term (3 months follow-up), the direction of effect was the same for Portland and Philadelphia but Memphis reported a positive impact on arrest (meaning that the intervention group had a reduced risk of arrest compared to the control). However, with the exception of the Portland site at 12 months, none of these effect size estimates are statistically significant. The effect size calculated for the remaining included study/ site (Bonkiewicz et al., 2014) suggests the intervention group of the pre-arrest Link Scheme were less likely to be arrested than the control group six months following a police-abated mental health crisis (Bonkiewicz et al., 2014: abstract). This finding is not statistically significant. A statistical meta-analysis of the data at longest follow-up was conducted to synthesise these results (see Figure 4).

Figure 4: Forest plot showing estimates of relative risk of arrest following pre-arrest diversion
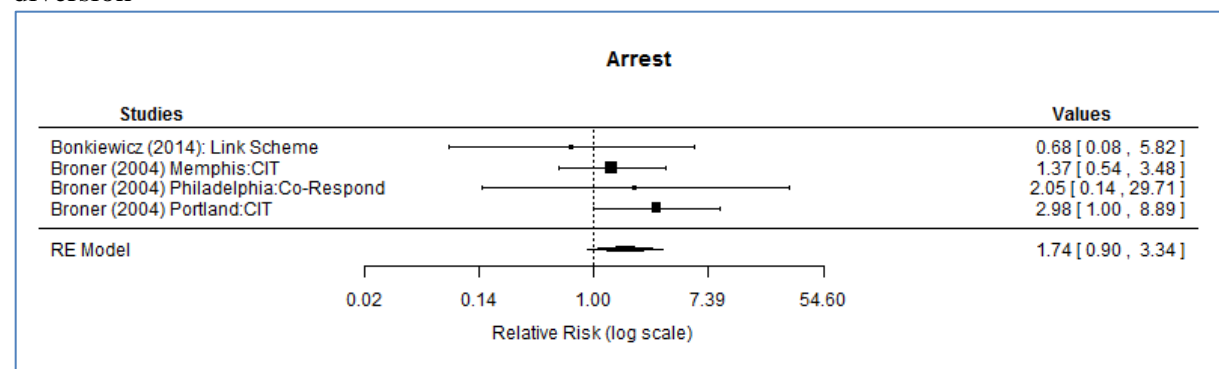
This and subsequent forest plots show the names (date) of each study/ site that has contributed to the meta-analysis on the left-hand side of the forest plot. The effect size for each study is listed to the right of the name and represented by the black square on the forest plot. A risk ratio of 1 indicates no difference in risk (e.g. of arrest) between the intervention and control groups. This is also known as the line of no effect and

visually represented by the solid black line plotted at Risk Ratio=1. The 95% confidence intervals for each risk ratio are represented by the bars that extend out from the square. The confidence interval shows the precision of the estimates of effect whereby a 95% CI contains the true effect in 95% of times, if the study was repeated multiple times. If the confidence interval crosses the line of no effect (1) then the effect size is not considered to be statistically significant. The area of the square represents the weighted contribution of each study in the meta-analysis. The pooled estimate of effect is represented as a diamond at the bottom of the plot, with its confidence intervals represented by the horizontal points of the diamond. Effect sizes plotted to the right of the line of no effect, with RR greater than 1, suggest that the risk of arrest had increased with the intervention (and so the intervention had a negative effect on criminal justice outcomes). Effect sizes plotted on the right hand side therefore favour the control group.

The pooled estimate of effect (RR= 1.74, 95%CI 0.90 to 3.34) suggest that the intervention group had an increased risk of arrest following intervention compared to the control group (effect sizes plotted to the right of the line of no effect, with RR greater than 1, suggest that the risk of arrest had increased with the intervention). The confidence intervals do not exclude the possibility of a reduced relative risk of arrest. However, although the summary effect size appears to suggest that the intervention is likely to lead to an increase in the relative risk of crime, the observed differences were not statistically significant. The confidence intervals of the results of individual studies overlap and statistical tests suggest that there is low heterogeneity between the studies (Q = 1.9411; df = 3; p = 0.5847; I2 = 0.00%; tau2 = 0).

The included studies evaluated different types of pre-arrest diversion intervention. The pooled estimate effect for both police led CIT studies (RR= 1.91, 95% CI 0.90-4.08) and the effect size for the Crisis Outreach Team (RR= 2.05, 95% CI 0.14-29.71)) suggest that the intervention increases the relative risk of arrest increases for the intervention group, compared to the control group. These findings, however, are not statistically significant. In contrast, the relative risk of arrest for the intervention group following involvement in the Link Scheme (RR= 0.68, 95% CI 0.08-5.82) decreased compared to the control group. This effect size is also not statistically significant.

Key messages
There is some statistically significant evidence that pre-arrest diversion has increased arrests (CIT, Portland) but overall the intervention has not had a statistically significant effect on arrests.

There is no statistically significant evidence that the type of pre-arrest diversion intervention influences the direction or strength of effect on crime.

Mental health
Three of the sites/ interventions reported mental health outcomes (Broner et al., 2004). These included measures of mental health status and mental health service utilization. Due to insufficient data, it was not possible to calculate effect sizes or undertake a meta-analysis for data on mental health status. Findings reported on mental health service utilisation were therefore used to draw conclusions about the effects of pre-arrest diversion interventions on mental health outcomes. These included study participants' self-report measures of counselling sessions, use of prescribed medications, and hospitalisation for mental health reasons. These are considered in turn below.

Overall, authors report that there were few significant individual site improvements in mental health measures (Broner et al., 2004: 537). However, site specific findings report mixed effects, depending on the instrument used to identify mental health status. The evaluation of the CIT interventions (Portland and Memphis) report statistically significant improvements in mental health status at 3 months when measured by the CSI tool (Portland: coeff= 4.16, p< .05, Broner et al., 2004: 535; Memphis: coeffi-cient = 2.4, p< .05, Cowell et al., 2004: 306, 308). However, when measured by the MCS, mental health status for the intervention group in one CIT site (Portland) deteriorated compared with the control group (Portland: coeff= -4.52, p< .05, Broner et al., 2004: 535). Whether statistically significant or not, contradictory findings are reported for each site with the CSI consistently measuring improvements and the MCS reported deteriorations in mental health status (see Broner et al., 2004: 533, Table 3).

Mental health: Service Utilization

The impact of pre-arrest diversion interventions on mental health service utilization was captured in terms of study participants' self-report measures of counselling sessions, use of medications, and hospitalisation for mental health reasons. Overall, the authors reported that 'Police diversion was found to be associated with increased odds of mental health medications and mental health hospitalization in all three pre-booking sites' (Broner et al., 2004: 532). The findings for each of the mental health outcomes are reported and further explored below.

Mental Health Counselling

Table 7 sets out the risk ratios calculated at different follow up points from the Broner et al (2004) study. For CIT sites (Memphis and Portland), the effect sizes suggest that CIT led to an increased likelihood that the intervention group received counselling (three or more counselling sessions), compared to control, at both time points. There is no consistency in the strength of effect for the CIT sites over time (Memphis RR decreased whereas Portland RR increased over time). For the Crisis Outreach team (Philadelphia), the findings from individual studies suggest the opposite effect: the intervention group were less likely than the control to receive mental health counselling. However, only two of the effect sizes reached statistical significance (highlighted in bold: Memphis, 3 months and Philadelphia, 3 months).

Table 7: Risk Ratios for mental health counselling, by follow up time

Site	RR 3 months	RR 12 months
Memphis (CIT)	**1.60 (95% CI 0.12-2.50)**	1.26 (95% CI 0.80-1.99)
Portland (CIT)	1.25 (95% CI 0.60-2.62)	1.49 (95% CI 0.76-2.91)
Philadelphia (Crisis Outreach team)	**0.23 (95% CI 0.11-0.48)**	0.77(95% CI 0.32-1.85)

The risk ratio/ relative risks calculated from the findings of each study are plotted onto Figure 5. The confidence intervals of the results of individual studies overlap and statistical tests suggest that there is low heterogeneity between the studies (Q = 1.4038; df = 2; p = 0.4956; I2 = 0.00%; tau2 = 0). The pooled estimate of effect (RR= 1.22,

95%CI 0.86 to 1.73) suggest that the intervention group had an increased likelihood of receiving mental health counselling compared to the control group. However, although the summary effect size appears to suggest that the intervention is likely to lead to an increase in the relative probability of counselling, the observed differences were not statistically significant.

Figure 5: Forest plot showing estimates of relative risk of mental health counselling following pre-arrest diversion, at 12 months
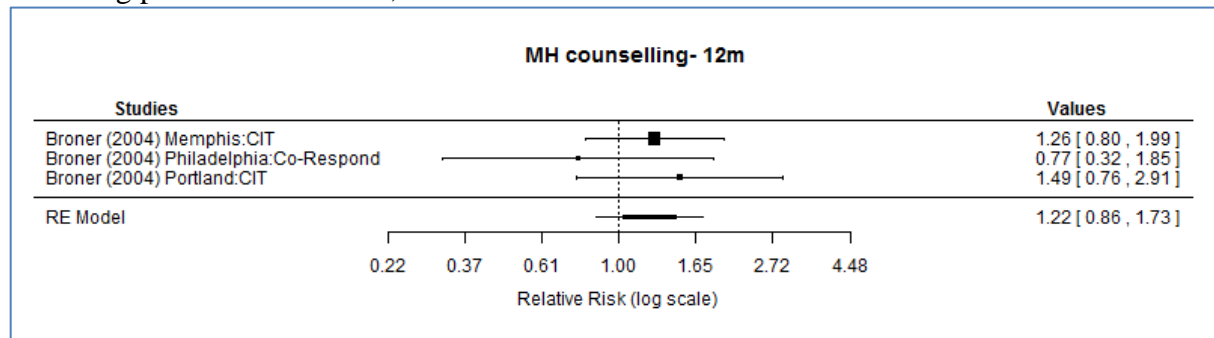
The effect sizes for both CIT sites were pooled and compared to the Crisis Outreach team intervention. The pooled estimate effect for CIT studies (RR= 1.33, 95% CI 0.91-1.93) suggest that CIT led to an increased likelihood that the intervention group received counselling (compared to the control, Q = 0.1515; df = 1; p = 0.6971; I2 = 0%; tau2 = 0.00). In contrast, the effect size for Crisis Outreach team (RR= 0.77, 95% CI 0.32-1.85) suggests that the intervention group were less likely than the control group to have accessed counselling. These findings did not reach statistical significance.

Mental Health Medications
This outcome measure refers to the prescription of mental health medications in the past three months. For all intervention sites, the calculated effect sizes suggest that pre-arrest diversion interventions increased the probability that the intervention group will be prescribed mental health medications (compared to the control group) (see Table 8) . The likelihood of prescription of medications reduces over time for the intervention groups in the CIT sites (Memphis and Portland), although the findings are not statistically significant at the 12 month follow up. The effect size for the Crisis Outreach team (Philadelphia) remain broader similar at both 3 months and 12 months follow up and statistically significant.

Table 8: Risk Ratios for mental health medication, by follow up time

Site	RR 3 months	RR 12 months
Memphis (CIT)	**1.23 (95% CI 1.05-1.44)**	1.09 (95% CI 0.95-1.26)
Portland (CIT)	**1.24 (95% CI 1.03-1.48)**	1.18 (95% CI 0.96-1.46)
Philadelphia (Crisis Outreach team)	**1.32 (95% CI 1.07-1.63)**	**1.34 (95% CI 1.07-1.67)**

The risk ratio/ relative risks calculated from the findings of each study are plotted onto Figure 6. The pooled estimate of effect (RR= 1.17, 95%CI 1.05 to 1.46) suggests that the intervention group had an increased likelihood of being prescribed mental health

medications compared to the control group. This finding is statistically significant and there is low heterogeneity between the studies (Q = 2.2893; df = 2; p = 0.3183; I2 = 12.64%; tau2 = 0.0013).

Figure 6: Forest plot showing estimates of relative risk of mental health medications following pre-arrest diversion, at 12 months
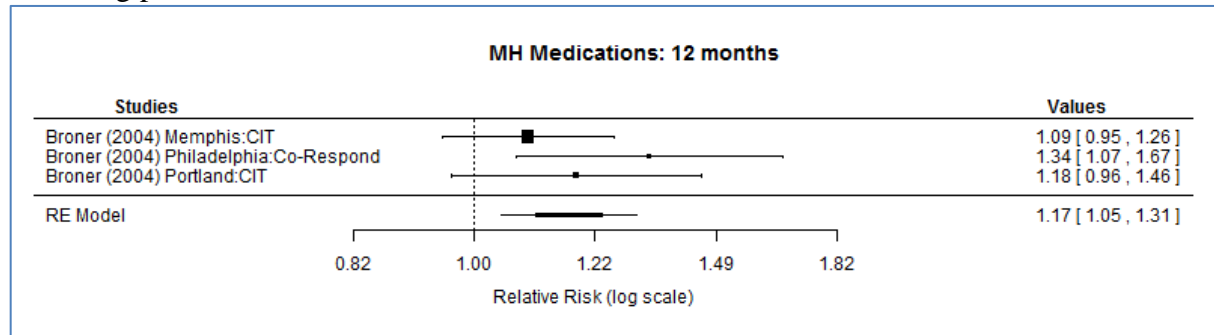
A comparison of the effect sizes for each type of intervention suggests that Crisis Outreach team (RR 1.34, 95% CI 1.07-1.67) led to a stronger effect than CIT studies (pooled estimate of effect RR 1.12, 95% CI 1.00-1.26).

Mental Health Hospitalisation
This outcome measure refers to hospitalisation in the past three months due to a mental health condition. The effect sizes for each site at both follow up times are presented in Table 9.

Table 9: Risk Ratios for mental health hospitalisation, by follow up time

Site	RR 3 months	RR 12 months
Memphis (CIT)	**1.92 (95% CI 1.17-3.16)**	1.22 (95% CI 0.65-2.30)
Portland (CIT)	**2.77 (95% CI 1.28-5.99)**	2.03 (95% CI 0.76-5.47)
Philadelphia (Crisis Outreach team)	**4.84 (95% CI 1.46-16.04)**	**4.14 (95% CI 1.40-12.18)**

All three studies suggest that there is an increased risk that the intervention group will be hospitalised for a mental health condition, compared to the control group, at three months after the intervention. At 12 months follow up, the direction of effect remains the same. The relative risk of the intervention group being hospitalised for a mental health condition reduced over time as all effect sizes demonstrate a reduction between three and 12 month follow up points. However, not all findings reached statistical significance (those that did are highlighted in bold).

The risk ratio/ relative risks calculated from the findings of each study at 12 month follow up are plotted onto Figure 7. The pooled estimate of effect (RR= 1.95, 95%CI 0.97 to 3.93) suggests that pre-arrest diversion increases the risk of participants being hospitalised for mental health reasons compared with the control group. This finding is not statistically significant and there is a moderate level of heterogeneity (Q = 3.7594; df = 2; p = 0.1526; I2 = 46.80%; tau2 = 0.1806).

Figure 7: Forest plot showing estimates of relative risk of mental health hospitalisation following pre-arrest diversion, at 12 months
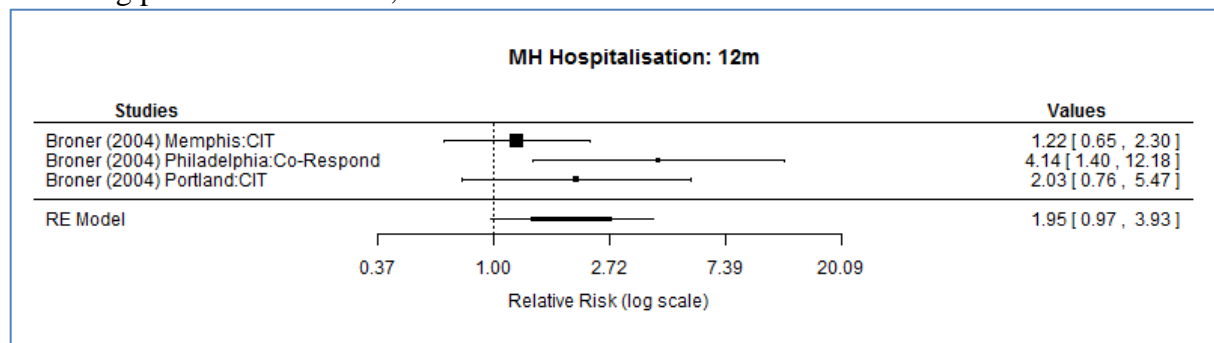
Combining the effect sizes of CIT studies leads to a pooled estimate of effect (RR 1.42, 95% CI 0.83-2.42) that suggests that CIT increases the risk of hospitalisation for the intervention group. There is low heterogeneity amongst studies (Q = 0.7238; df = 1; p = 0.3949; I2 = 0.00%; tau2 = 0) but this finding does not reach statistical significance. In comparison, the effect size for the Crisis Outreach team (RR 4.14, 95% CI 1.40-12.18) is statistically significant and suggests that the intervention group had an increased risk of hospitalisation, compared with the control group, following pre-arrest diversion.

Other Mental Health Related Service Utilization Outcomes
Bonkiewicz et al (2014) examined the impact of the Link Scheme on subsequent 'mental health Calls For Service (CFS)' which refer to the number of calls made to the police for incidences identified as mental health related. There were no comparable outcomes measured in the other included studies. The authors report that the intervention group generated fewer mental health CFS than the control group, six months following a police-abated mental health crisis: "PCAP contact decreased mental health CFS by approximately one call for service (-1.01, -0.92, -0.91, and -0.88; p<0.05). This result may sound minimal, but the average number of post-crisis mental health CFS was 1.2, with a standard deviation of 1.3, meaning that PCAP contact decreases mental health CFS by nearly one standard deviation" (Bonkiewicz et al., 2014: 773).

Key messages
There is some evidence that pre-arrest diversion has either increased (CIT, Memphis) or decreased (Crisis Outreach Team, Philadelphia) the likelihood of receiving mental health counselling, but *overall* the intervention has not had a statistically significant impact on the uptake of counselling.

Overall, the evidence suggests that pre-arrest diversion has increased the likelihood that medication is prescribed. The type of intervention influenced the strength of effect: the Crisis Outreach team led to a statistically stronger effect than CIT studies.

There is some evidence that the intervention has increased the risk of hospitalisation but overall the intervention has not had a statistically significant effect on hospitalization.

Mechanisms: explanations for how police pre-arrest diversion reduces crime and improves mental health

Mechanisms explain how an intervention works (Johnson et al., 2015) and provide a theoretical framework that underpins the intervention (causal mechanisms that are presumed to be responsible for outcomes). The mechanisms identified and discussed below provide a set of different explanations of 'how' and 'why' specialised policing responses to people with mental health issues work to reduce crime and improve mental health outcomes.

In general, the included studies did not provide extensive discussion of theoretical ideas that underpin the effectiveness of the intervention. The authors rarely supported their claims with evidence from the wider literature and did not test these mechanisms with study findings. Table 10 includes mechanisms that were identified in the studies. Mechanisms that have been evidenced by reference to the wider literature are in italics. These mechanisms are organised in descriptive themes (descriptive grouping of original text from the studies) and analytical themes (higher level, analytical categories that include reviewer interpretation)

Table 10: Mechanisms identified in included studies

Descriptive themes	Analytical themes
Police knowledge, skills, and attitudes Awareness/ use of police powers of discretion to divert *'Police friendly' policies and procedures at referral* *Shared professional interests (police and MH professionals)*	Enabling police
Supporting access to services after diversion *Oversight and monitoring after diversion*	Monitoring of compliance
Empathetic and personal response Consent and legitimacy *Specialised response*	Enabling people with mental health issues
Diversion at earliest opportunity Avoiding criminal justice pathway	Early Interception

The following discussion is drawn from the descriptive themes/ mechanisms that were identified in the included studies and then organised into four broader analytical themes. These are described in turn, with reference to the supporting arguments and literature provided by the included studies.

Enabling police
The included studies assume that an effective intervention is driven by factors that enable the police to take appropriate action when interacting with people with mental health issues. This understanding relates to the wider literature that recognises that in the traditional policing model, police do not have sufficient resources to respond to people with mental health issues. Pre-arrest diversion interventions address this deficit by improving police skills and attitudes, developing accessible services and providing support. Pre-arrest diversion programmes heighten the awareness of police officers who 'simply weren't aware of the program or how to make a referral' prior to the intervention (Bonkiewicz et al., 2014: 11) and remove barriers that traditionally inhibit

police action: 'police-friendly' policies and procedures at the referral centres that include a single point of entry, a no refusal policy for officers and access 24 hours a day (Steadman et al., 2001). A central referral site "addresses past difficulties for police in accessing mental health services in response to a psychiatric crisis" and the no-refusal policy "addresses one of the largest barriers in the traditional emergency room model by eliminating unnecessary arrests" (Steadman et al., 2001: 3). Police training and knowledge is seen to "help officers" respond to people with mental health issues (Bonkiewicz et al., 2014: 5) and support from mental health professionals is seen to reduce 'the burden of discriminating between mental health, substance abuse, and other crises' (Steadman et al., 2001: 3). The success of the intervention is therefore associated with enabling factors that allow the police to respond more appropriately to people with mental health issues. This understanding foregrounds the role of the police as the critical agent for success, focusing on a diversion ''from'' the criminal justice system rather than diversion ''to'' the treatment system model" (Broner et al., 2004: 20).

Monitoring of compliance
Both included studies identify that the provision of post-diversion support/ monitoring is central to the success of the intervention. Post- diversion assistance is seen to help people with mental health problems access services, and reduce mental health crises and further contacts with the police (Bonkiewicz et al., 2014). Broner et al. (2004), however, recognise the function of such support in terms of 'monitoring of compliance and re-linking to services' (Lattimore et al., 2002: 14). The authors suggest that for particular groups (those with heavy substance abuse, prior criminal justice involvement, and less robust functioning) "the increased oversight and more directive approach of the post-booking model might be an important variable to the success of the diversion" (Lattimore et al., 2003: 30). Following the identification and diversion of people with mental health problems, the effectiveness of the intervention therefore relies on measures that "ensure that he or she follows appropriate treatment recommendations" (Lattimore et al., 2003: 30). 'Boundary spanners' (someone who acted as a liaison among all services and facilities) and case managers serve the function of 'ensuring that all referrals are linked to services' which is associated with lower crisis recidivism and possibly lower criminal recidivism (Steadman et al., 2001: 4).

Enabling people with mental health issues
In contrast to the themes above, the success of pre-arrest diversion programmes can be understood in terms of enabling people with mental health issues to overcome barriers to accessing services and treatment. Pre-arrest diversion programmes facilitate connections between people with mental health issues and mental health services that would otherwise not occur. The effectiveness of the intervention is therefore derived from the specialist and empathetic response that it provides to people with mental health issues. The availability of specialist responses has "been a critical factor in surmounting many of the problems previously experienced in law enforcement/mental health interactions" (Steadman et al., 2001: 4). The drivers for improved mental health outcomes "may be attributable to factors related to diversion itself, including intervention by a specially trained police team, a specialized receiving facility, and avoidance of the negative experience of being in jail" (Cowell et al., 2004: 18). This understanding relates more broadly to the finding that police are often the 'frontline professionals' for people experiencing mental health crises (Lamb et al., 2002). The response provided by pre-arrest diversion programmes offers access to alternative

services "that are viewed as more helpful than traditional methods, such as hospitalisation" (Bonkiewicz et al., 2014: 7).

Early interception
The Sequential Intercept Model (Munetz and Griffin, 2006) is used as a framework for understanding interactions between people with mental health problems and the criminal justice system. The model identifies potential points at which PMHI can be intercepted to prevent them from entering or heightening their involvement in the criminal justice system. Within the literature, police pre-arrest diversion interventions are commonly recognised as the main form of early interception of PMHI Whilst the included studies do not explicitly refer to this model, they do imply that early interception is an important feature of the interventions: the goal of the program is to "provide diversion at the first interaction between the consumer with mental illness and addiction disorders and the police" Lattimore et al. (2003: 34) and improvements in mental health outcomes are theorised as a consequence of the "avoidance of the negative experience of being in jail" (Cowell et al., 2004: 309). Early interception of PMHI is more widely recognised as an important element in understanding how pre-arrest diversion interventions work (e.g. Compton et al., 2008; Hartford et al., 2006).

Moderators: the population groups for which police pre-arrest diversion is most effective

Moderators specify under what conditions or which population groups the intervention is deemed to work best. The studies examined the influence of the characteristics of the intervention providers and participants on the likely effect of pre-arrest diversion. Studies tested the effects of these characteristics using post hoc analysis of the variables collected about the participants/ providers. These are further discussed below.

Characteristics of the intervention providers
One of the included studies (Bonkiewicz et al., 2014) analysed findings to test the influence of police officer characteristics on the likelihood of the officer diverting PMHI. The study found that there was no significant association between police officer characteristics (age, gender, years of service and CIT training) and their likelihood of referring PMHI to services. The same study also speculated that the attitudes and experiences of police officers might influence their decision to divert an individual but 'due to data limitations' (Bonkiewicz et al., 2014: 773), they were unable to test this theory. Further, the study suggested that the use of 'peer specialists' (to support PMHI access services) who have experience of mental health problems 'gives them considerable credibility when they initiate contact with consumers, especially those consumers who suspect authority figures are trying to control, drug or institutionalise them' (Bonkiewicz et al., 2014: 776) but this was not tested by study data or supported by reference to the wider literature.

Characteristics of people with mental health issues
Both of the included studies identified participant demographics as a factor in determining an individual's involvement in pre-arrest diversion. Broner et al. (2004) compared participants that were diverted prior to arrest to those individuals diverted post-arrest. Lattimore et al (2003: 42) found that particular PMHI were more likely than others to be diverted earlier in the criminal justice pathway. PMHI diverted in pre-arrest diversion interventions were older, less likely to be White or Hispanic, more likely to have a high school diploma or equivalent, and more likely to have been employed compared to participants diverted at post-booking.

Both included studies suggested that the housing status of PMHI may play a role in the likelihood of being diverted and receiving treatment in the intervention. A large minority of participants diverted in the Broner et al., (2004) study had no regular place to live and Bonkiewicz et al. (2014: 767) reported that Peer-specialists did not contact some consumers because they were transients and unable to be located. These associations, however, were not tested in the data.

The nature and severity of mental health problems was considered by both studies to be a potential factor moderating the likelihood that a PMHI would be diverted rather than arrested by a police officer. Bonkiewicz et al (2014: 770) suggest "different mental health conditions might increase the chances of a referral. For instance, officers might be more likely to refer a case of untreated schizophrenia compared to a case involving an anxiety attack, even though both incidents are legitimate referral candidates." Analysis from Broner et al., (2004) supports this theory as the study found that diverted PMHI had significantly more mental health problems than the control group (Broner et al., 2004; Gratton et al., 2001). Moreover, both studies acknowledge that access to

mental health services and treatments were likely to have influenced the likelihood that individuals were identified and diverted by officers. Whilst this moderator was not tested by Bonkiewicz et al (2014), Broner et al (2004) found no significant differences in the reported use of mental health services between the intervention and control group in Portland (Gratton et al., 2001). Both studies assume, without testing, that willingness to accept treatment is an important factor influencing intervention effectiveness.

Study data suggest that PMHI diverted prior to arrest were less substance-involved than PMHI diverted at a later stage in the criminal justice pathway (Gratton et al., 2001; Lattimore et al., 2003). Moreover, PMHI diverted at pre-arrest were more generally more satisfied with their lives than post-booking subjects (Broner et al., 2004; Lattimore et al., 2003). Whilst individuals diverted earlier in the criminal justice system had a high rate of victimisation (compared to the general population), data from Broner et al., (2004; Lattimore et al., 2003) suggests that this was a lower rate than those individuals diverted after arrest.

Both of the included studies identified prior criminal involvement/ contact with the police as potential moderators for the likelihood that PMHI's would be diverted. Bonkiewicz et al (2014) theorised that frequent contact with the police could increase or decrease the likelihood that an officer would refer the PMHI. If an individual was known to them, the police officer may divert the PMHI in order to reduce the repetitive nature of the police contact. Alternatively, Bonkiewicz et al (2014) suggest that such individuals could also be considered problematic and unworthy of assistance. The study did not analyse or test these theories with data from the study or wider literature. Analysing the characteristics of the study participants, Broner et al. (2004) found that individuals identified and referred to treatment services by the police had a less active criminal history than those in the control group. The data suggested that there were differences in the nature and frequency of adult offending for PMHI diverted before and after arrest (Lattimore et al., 2003). Lattimore et al (2002) used the wider literature to suggest that the participants diverted earlier in the criminal justice pathway were less likely to offend in the future than those diverted at later stages. Measures found to predict future criminal activity, prior arrests and age at first arrest, were lower for PMHI diverted pre- rather than post arrest.

Implementation: barriers and facilitators to the implementation of police pre-arrest diversion

Implementation factors are understood as practical, operational and strategic factors that can be manipulated by the intervention providers in the planning and delivery of pre-arrest diversion. There are a number of guides that identify key elements for the successful diversion of people with mental health problems (for example: Durcan, 2014; Reuland, 2004; Schwarzfeld et al., 2008). This section identifies factors that can facilitate or impede the implementation of pre-arrest diversion as identified or discussed in the included studies.

Multi-agency collaboration in the delivery of the intervention
The value of multi-agency collaboration in the planning and implementation of pre-arrest diversion programmes is identified, but rarely tested or evidenced, by both of the included studies. The collaboration of police departments, mental health organisations and wider community advocates is seen by the authors to be integral to the delivery of the intervention (Bonkiewicz et al., 2014; Broner et al., 2004). The studies refer to a number of factors that can support a collaborative working relationship: establishing legal foundations for diversion; information sharing; and mutual understanding of the day-to-day experiences of working with PMHI.

Establishing the legal underpinnings of diversion is seen to engender the smooth running of the intervention, allowing officers to divert PMHI and providing "some degree of protection for mental health clinicians working in what is seen as the high-risk field of 'dangerousness assessment'." (Steadman et al., 2001: 221). A co-operative agreement is identified as supporting multi-agency working, offering "significant strides in terms of information sharing and communication, moderate gains in terms of coordination, and slight gains in terms of collaboration" (Gratton et al., 2001: 6). Bonkiewicz et al (2014: 767-8) refer to the wider literature to highlight the importance of data sharing between policing and mental health agencies, which is seen to "facilitate an informed, cooperative response for consumers in crisis". Indeed, information sharing is widely identified as an important factor for successful implementation of diversion interventions: "essential to achieve desired outcomes by helping responders be more sensitive to individual needs, reduce injury, and enhance their ability to determine next steps" (Schwarzfeld et al., 2008: 7). A mutual empathy of the roles and duties of police officers and mental health professionals is identified as a further factor that can improve collaborative working (Steadman et al., 2001).

Promoting police awareness and acceptance of pre-arrest diversion
Police officer lack of awareness was identified by Bonkiewicz et al (2014) as an impediment to the successful implementation of pre-arrest diversion. The authors conclude that departments should consider "implementing the referral program as a part of their standard operating procedures for mental health calls. Such a practice would ensure that officers are educated about the program, how to make a referral, and how it can reduce future calls for service" (2014: 775). This type of proposal is echoed in the wider literature where communication with officers is seen to be fundamental in promoting police awareness and acceptance of pre-arrest diversion (Schwarzfeld et al., 2008).

Making referral easy for police officers
'Police friendly policies' at referral centres were identified as a key part of the delivery of the intervention by included studies. The authors suggest that a number of operational elements support police referral of PMHI to services. These include a 24 hour, 'streamlined referral process for police' which is quick (typically less than 30 minutes turn around) and has a dedicated support office (Steadman et al., 2001). Whilst none of these features were evidenced by findings from the included studies, they are consistent with recommendations in the wider literature (Durcan, 2014; Schwarzfeld et al., 2008).

Supporting take up of treatment and services
Referring to wider evidence, authors argue that diversion programmes that ensure that treatment is accepted and undertaken by PMHI can lead to improvements in mental health and crime outcomes (Broner et al., 2004). There are a number of implementation factors that were seen to support PMHI connection with treatment: A timely linkage to treatment which was defined as 'often immediate' (Broner et al., 2004) or within 24-48 hours of initial police contact (Bonkiewicz et al., 2014); accurate assessment of the mental health problems (whether by trained police officer or mental health professional); the provision of a crisis centre which provides access to various services (not just mental health specific) and the use of peer specialists who have 'considerable credibility' to support PMHI develop long term treatment plans (Bonkiewicz et al., 2014).

Economics: The impacts of police pre-arrest diversion

Overview of identified economic evidence
The in-depth review identified five eligible economic evaluations of pre-arrest police diversion programmes, compared with either treatment as usual (Cowell 2004, Cowell 2013, Scott 2000, Allen Consulting Group 2012) or diversion initiated subsequent to arrest (Cowell 2015), among people with serious mental health problems. These economic evaluations assessed programmes in terms of costs only (Cowell 2013, Cowell 2015, Scott 2000, Allen Consulting Group 2012), or in terms of their cost-effectiveness (Cowell 2004). All five identified economic evaluations were conducted using the framework of a single study (that is, no model-based economic evaluations of pre-arrest police diversion programmes were identified) and all adopted a multi-sector analytic perspective, encompassing consideration of direct costs (resource use) incurred in both local criminal justice and local health care systems (EMMIE-Q score: 2). In summary, the principal findings of these economic evaluations suggested pre-arrest police diversion is likely to lead to 'cost shifting' from local criminal justice agencies to local health care agencies in the short-term (up to 2 years), (see below for further details), but with the potential to deliver overall cost savings from a multi-sector perspective over a similar time period.

In addition to 5 eligible economic evaluations, we also identified one cost description study that described the impacts of a pre-arrest diversion programme on costs (resource use) (El-Mallakh 2014), as well as 4 other studies – all conducted in the USA – that reported more limited cost information for eligible comparisons (Massachusetts Department of Mental Health Forensic Mental Health Services 2009, Orr 2014, Parsonage 2009, Tartaro 2015).

Further key characteristics and principal findings of these studies are summarised below. To facilitate comparison of estimates between studies, all costs are expressed in a common currency and price year: 2016 GBP (£) (see Methodology section).

Summary of evidence from full economic evaluations and cost analyses

Police led response: CIT
A cost-effectiveness analysis (CEA), based on the study of the effects of SAMSHA pre-arrest diversion interventions described above in this section (Broner et al. 2004), compared CIT diversion with treatment as usual for people in Memphis, USA with co-occurring serious mental illness and substance abuse or dependence disorders (Cowell 2004). This analysis considered incremental costs (resource use) associated with both implementation the diversion programme, and also with differences between diverted and non-diverted clients in their subsequent use of services over 3 months following the intercept point. The main cost categories included in the analysis were: the courts, public defenders' and prosecutors' offices, police, and prisons (direct criminal justice costs), and inpatient, residential, and outpatient treatment for mental health and substance abuse received by clients in the community or in prison (direct health care costs). The underlying evaluation study measured 9 self-reported outcomes at 3 months following the intercept point (criminal behavior, quality of life, substance use, and mental health status outcomes) (Broner et al., 2004). Data for one of these outcome measures – change from baseline on the Colorado Symptoms Inventory (CSI) – was

used in the cost-effectiveness analysis. This outcome measure was selected for incorporation into the CEA on the basis that a difference was found between diverted and non-diverted groups on this measure at 3 months; no differences between diverted and non-diverted groups were identified on the other 8 outcome measures at 3 months. This CEA found that diversion was, on average (mean), associated with higher total direct costs per client at 3 months compared with treatment as usual (£4,147 higher). Although diversion was found to be associated with cost savings accruing to the local criminal justice system, the higher amount and unit cost of health care treatment (primarily in-patient mental health care costs) among diverted clients drove the overall finding that diversion was associated with higher total costs. The CEA also estimated an incremental cost-effectiveness ratio (ICER) of £1,194 per one point improvement on the CSI (95% CI: 475 to 17,132).

A cost analysis by the same authors compared costs (resource use) incurred by CIT diverted clients in one time period (immediately following full implementation of a pre-arrest police diversion programme) with those incurred by non-diverted clients in an earlier time period (which preceded diversion programme development) in the same locality – a county in San Antonio, USA) – and used propensity score methods to adjust for selection bias in the comparative analysis of these costs (Cowell 2013). This analysis considered incremental costs (resource use) associated with both implementation the diversion programme, and also with differences between diverted and non-diverted clients (people with serious mental illness and a misdemeanour offense) in their subsequent use of services over 2 years following the intercept point. The main cost categories included in the analysis were those incurred by police, courts and prisons (direct criminal justice costs), and treatment costs incurred by the local behavioural health care provider, the local hospital system, and medication providers (direct health care costs). This cost analysis found that diversion was associated with lower average (mean) total costs per client at 2 years compared with treatment as usual (£2,240 lower, SE = 655). Higher mental health care treatment costs at two years (£499 higher, SE = 545) among diverted clients, combined with lower costs to the criminal justice system (£2,740 lower, SE = 332), drove the overall finding. A second cost analysis of CIT, conducted by the same authors (Cowell 2015), focused exclusively on comparing the implementation costs of pre-arrest police diversion with those of implementing two forms of post-arrest diversion, as part of the same overarching diversion programme, over its three-year start-up period. This analysis included the same main cost categories as the Cowell 2013 analysis (described above). It found that the total cost of implementing pre-arrest diversion was, on average (mean), higher than the total cost of implementing post-arrest diversion It also found that the local health care provider (health care system) incurred 90% of total pre-arrest diversion implementation costs; in contrast, local courts (criminal justice system) incurred the majority of the total costs of implementing post-arrest diversion programmes (respectively, 55% and 58%).

Co-responding police response

A cost analysis compared a; mobile, co-responding model of a pre-arrest police diversion programme implemented in Georgia, USA, with treatment as usual, among people experiencing psychiatric emergencies (Scott 2000). This involved retrospective analysis of a natural experiment that occurred because diversion was available to clients only when the mobile crisis team was 'on shift'. This analysis investigated incremental costs associated with implementation of the programme, and with differences between diverted and non-diverted clients in their subsequent use of services. The time horizon

for costs was not reported. Main cost categories included in the analysis were: police time spent on programme delivery (direct criminal justice costs), along with mental health professional time spent on programme delivery, and clients' use of psychiatric hospital residential treatment services (direct health care costs). This cost analysis found that diversion was associated with lower average (mean) total costs per client (£445 lower), compared with treatment as usual. Higher incremental direct costs of implementation (£393 higher) among diverted clients were entirely offset by lower direct health care costs (£847 lower) among this group, reflecting the higher probability that clients seen by the mobile crisis team were managed without psychiatric hospitalization.

A second cost analysis compared a mobile co-responding model with treatment as usual, among people with a mental illness or condition and experiencing a crisis, in Victoria, Australia (Allen Consulting Group 2012). This involved a retrospective comparison of cohorts in two geographical regions of Victoria with and without the programme. This cost analysis covered incremental costs (resource use) associated with both implementation the diversion programme, and also with differences between diverted and non-diverted clients in their use of hospital emergency department services immediately following the intercept point. Main cost categories included in the analysis were: police time and equipment (direct criminal justice costs), mental health clinician time, ambulance and transportation use, and use of hospital emergency department resources (direct health care costs). The analysis found that the average (mean) total cost per case was lower among diverted than among non-diverted clients in all four variant scenarios examined (ranging from £36 lower to £203 lower between the most and least conservative scenarios). This finding was driven primarily by a lower proportion of referrals to hospital emergency departments, and shorter average length of stay following admission, among diverted clients. Evaluation findings further suggested that the lower proportion of referrals to hospital emergency departments among diverted clients was accompanied by an increase in the rate of direct referral for psychiatric facilities; however costs of treatment in psychiatric facilities were not included in the analysis.

Summary of findings of identified economic evaluations
A relatively consistent finding among identified economic evaluations was that pre-arrest police diversion led to 'cost shifting' from local criminal justice agencies to local health care agencies in the short-term (up to 2 years), as clients were diverted away from the criminal justice system into treatment by health care agencies. However, in 3 of 4 economic evaluations that quantified and valued the impacts of pre-arrest diversion on subsequent service use, compared with treatment as usual (Cowell 2013, Scott 2000, Allen Consulting Group 2012), diversion was also associated with lower total direct costs (i.e. direct costs incurred by the local criminal justice and health care agencies combined) in the short-term. Conversely, the CEA of a CIT program (Cowell 2004) found pre-arrest diversion was associated with higher total costs per client at 3 months, however it is possible this time horizon is too short to capture all of the important differences in costs and effects between diverted and non-diverted clients.

Another consistent finding among identified economic evaluations was that the costs of implementing pre-arrest police diversion programmes were typically shared between criminal justice and health care agencies and systems. However the distribution of implementation costs between these two systems was inconsistent between the different

models of pre-arrest diversion evaluated. This can be attributed in part to variation between these studies in the main cost components they covered.

We did not subject the 5 economic evaluations described above to formal critical appraisal and do not attempt to draw any firm or general conclusions regarding the relative costs or cost-effectiveness of pre-arrest police diversion programmes. However, we note study authors' concerns about the possible non-equivalence of identified comparison groups, and also the spectrum of approaches used across these studies to control for risk of selection bias in the analysis of costs (and effects if applicable). It is possible these study design issues may have led to over- or underestimation of differences in costs (and effects, if applicable) between diverted and non-diverted groups. We further note that 4 of 5 economic evaluations were set in local criminal justice and health care systems in the USA (and the fifth in Australia), which are likely to differ from the UK context (and other settings) in ways that influence processes underpinning the implementation – and therefore the costs and effects - of pre-arrest police diversion.

It is notable that the CEA (Cowell 2004) was the only full economic evaluation of pre-arrest police diversion identified by the in-depth review. This signals a general disconnect between evidence for the costs and evidence for the outcomes of pre-arrest police diversion, which hampers understanding of the economic case for (or against) pre-arrest police diversion. Overall cost savings from diversion (Cowell 2013, Scott 2000, Allen Consulting Group 2012) may be considered favourably from an economic perspective if diverted clients' outcomes are also better (or, at least, no worse) than those of non-diverted clients; and analyses of costs only cannot address this question. In this context, it is notable that the CEA (Cowell 2004) found higher total costs per client among diverted participants, in conjunction with improvement on only 1 of 9 outcome measures at 3 months (and with no statistically significant differences found between diverted and non-diverted clients on the other outcome measures).

Summary of cost information found in other studies
In addition to the full economic evaluation and cost-analyses described above, the review also identified a cost description of a CIT programme in Louisville, Kentucky, based on retrospective analysis of administrative data (El-Mallakh 2014). This study also suggested pre-arrest diversion can produce overall cost-savings from a multi-sector perspective (criminal justice and health care combined), and highlighted that the majority of implementation costs were incurred by the local health care system, but also that the large majority cost-savings accrued to this system. The review also identified two descriptive reports on pre-arrest jail diversion programs implemented in Massachusetts between 2006 and 2009 (Massachusetts Department of Mental Health Forensic Mental Health Services 2009), and between 2011 and 2014 (Orr 2014), which included brief discussions of impacts on costs and a basic estimate of projected cost-savings at state level (>£1.5M per annum between 2011 and 2014) as a consequence of diverting clients away from treatment by hospital emergency departments, arrest, and prison custody (Orr 2014).

An economic commentary and analysis by Parsonage and colleagues was the only identified study focusing on the UK context that included (partially) relevant cost information (Parsonage 2009). This study focused exclusively on crime-related costs, encompassing both pre- and post-arrest diversion schemes, and addressed the following

questions: "What scale of benefits must be achieved by a diversion scheme to justify investment on value for money grounds?" and "In light of the limited available evidence on outcomes and effectiveness, how likely is it that benefits on this scale can be performed?". The authors reported that "…it is estimated that [diverting offenders with mental health problems towards effective community-based services] will lead to savings in crime-related costs of over [£23,000] per case, including savings to the criminal justice system of up to [£9,220] and benefits from reduced reoffending of around [£18,440]." However, this study did not include assessment of costs or cost-savings (resource use) incurred by/ accruing to the health system.

Finally, the review included a retrospective cohort study (conducted using archival data) that investigated the effects on criminal justice outcomes of a jail diversion program for offenders with serious mental health problems, implemented in New Jersey, USA, that included pre-arrest, post-arrest and re-entry (on release from prison) intercept points (Tartaro 2015). This study included a basic estimate of differences in costs associated with subsequent use of criminal justice resources between diverted and non-diverted clients, based on a finding that diverted clients spent more time in the community before their next incarceration, compared with non-diverted clients (a mean difference of 218 days). Based on the latter finding alone, the study authors estimated an average (mean) cost-saving accruing to the criminal justice system of £12,562 per diverted client. However (like Parsonage 2009), this study did not include consideration of costs (resource use), or cost-savings, incurred by, or accruing to, the health care system; and is only partially within scope of the in-depth review because the programme included both pre- and post-arrest intercept points.

Discussion

This systematic review synthesised findings from two included studies, covering four independent samples, to examine police pre-arrest diversion of PMHI. The review used a multi-dimensional analysis based on the EMMIE framework (Johnson et al., 2015) to interrogate the crime and mental health impacts of the intervention; the causal mechanisms underpinning the effects; the population groups for which the intervention is most likely to work; and the factors supporting or inhibiting the implementation of pre-arrest diversion. Drawing on a wider set of studies, the review also examined the economic costs associated with pre-arrest diversion interventions. This section brings these analyses together, intending to offer a holistic understanding of the effectiveness of pre-arrest diversion interventions and provoke further debates about the intersection of policing, mental health services and PMHI.

Understanding police pre-arrest diversion interventions: summary of main EMMIE findings

Evidence of the effects, moderators and mechanisms associated with pre-arrest diversion interventions are illustrated in Figure 8. The blue horizontal arrows represent steps common to pre-arrest diversion interventions, the key mechanisms that enable these steps to take place are identified in the purple vertical arrows and population characteristics that support the activation of these mechanisms are presented in the green ovals. The outcomes of the intervention are reported in the blue rectangular boxes. Figure 8 is derived from evidence presented by the included studies: statistically significant outcomes (from individual studies or pooled effects), post hoc analysis of moderator variables in the studies; and mechanisms identified by individual studies that were supported by wider literature.

Overall, the impacts of pre-arrest diversion interventions for crime and mental health remain ambiguous. The findings suggest there is no conclusive evidence that pre-arrest diversion interventions reduced crime (see limitations section for brief discussion on the relationship between crime and arrests). There is some evidence that the intervention increased crime (i.e. one study site reported a statistically significant increase in arrests), but overall pre-arrest diversion interventions did not have a statistically significant effect on crime. The overall body of evidence was small, judged to be low quality and solely from the USA so we should interpret this finding with caution. However, this is a similar finding to a previous review which found no evidence to suggest a reduction in long term re-offending (Sirotich, 2009). Evidence on the impact of police pre-arrest diversion strategies on mental health outcomes was mixed. Overall, there is tentative evidence that these interventions increased the likelihood that mental health medication was prescribed. This would suggest, as previous reviews have found, that interventions have the potential to improve linkages between people with mental health issues and community services (Shapiro et al., 2015; Taheri, 2014). However, contrary to this, the review found no evidence to suggest that the intervention had a significant impact on the likelihood of receiving mental health counselling. Moreover, there is indicative evidence that pre-arrest diversion increased the risk of hospitalisation for participants in the intervention group, compared to controls, in the short term.

From an economic perspective, pre-arrest police diversion represents at least a promising strategy, compared with treatment as usual. Identified study findings suggested that pre-arrest diversion programmes can lead to overall cost savings, on average (per client), when costs (resource use) incurred by both criminal justice and health care agencies are considered together over a sufficiently long time-horizon to capture important impacts on clients' subsequent service use. Coupled with the consistent finding between studies that diverting clients towards health care services prior to arrest is likely, at least initially, to shift the costs of managing people with serious mental health problems away from criminal justice agencies and onto the health care system, this potential for overall cost savings suggests that implementation of pre-arrest diversion is likely to require a multi-sector decision-making perspective, with joint commissioning of such programmes by decision makers adopting a multi-sector perspective.

Evidence of mechanisms and moderators can serve to contextualise and improve our understanding of these findings. The following discussion will focus more closely on the component parts of pre-arrest diversion and the mechanisms and moderators that influence the successful undertaking of different stages of the intervention.

Beginning with the early stages of pre-arrest diversion, on the left-hand side of Figure 8, the review identified that certain characteristics of PMHI supported the activation of causal mechanisms at the identification and referral stages of the intervention. The moderating variables, older, high school educated and non-White or Hispanic population groups suggest that particular PMHI are more likely to be identified and referred for pre-arrest diversion than others (represented in the blue oval boxes). Other characteristics that increased the likelihood of identification for pre-arrest diversion included individuals with multiple mental health problems but lower substance-involvement, those that had a less active criminal history and a greater life satisfaction than individuals diverted later in the criminal justice system. Therefore, this means that particular PMHI, with more serious issues, (i.e. higher involvement in substance abuse, higher level of criminal activity) were less likely to be diverted. The moderatorating variables (represented in the blue ovals) potentially interact with the causal mechanisms (identified in purple vertical arrows) in a number of ways to enable the successful identification of PMHI: a specialised, empathetic and personal response by the police may be more effective with particular PMHI; individuals with multiple health problems may be easier to identify and serve to improve officer knowledge, attitudes and skills; particular PMHI may prompt an empathetic and personal response from police officers. There was no evidence to suggest that the characteristics of the intervention providers or CIT training influenced the likelihood of an officer diverting PMHI (Bonkiewicz et al., 2014). This is confirmed by other reviews which find no evidence that CIT training influences the likelihood that officers will divert rather than arrest people with mental health issues (Taheri, 2014). This analysis suggests that it is the *nature* of the police response, rather than a response per se, that is critical for effectively identifying PMHI for diversion.

Figure 8: Model of the effectiveness of police pre-arrest diversion interventions, as evidenced by included studies
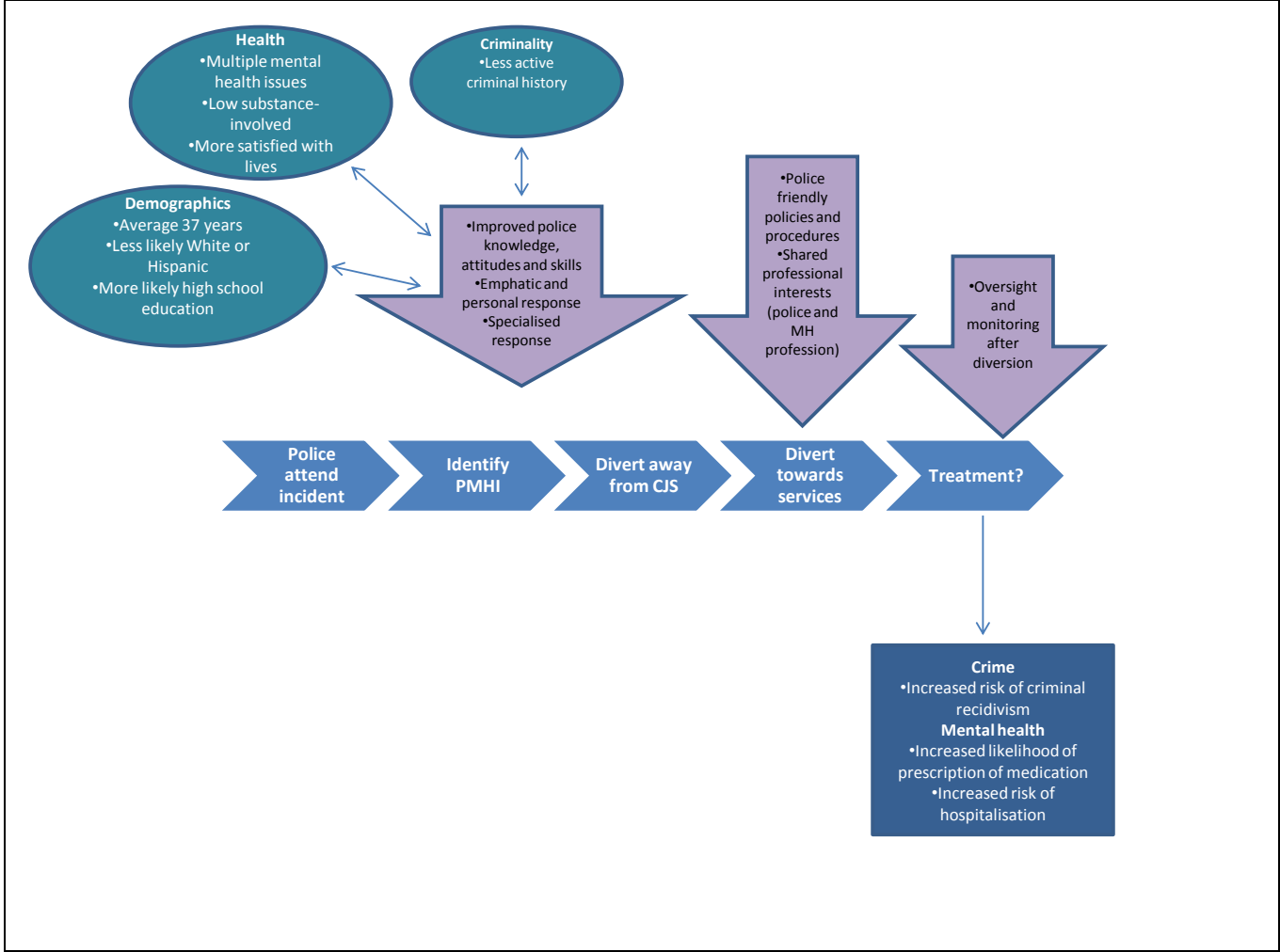
As illustrated by Figure 8, evidence of moderators was only present for the initial steps of pre-arrest diversion (identification and referral of PMHI away from CJS). There is a lack of knowledge about the moderating variables that activate causal mechanisms for the latter stages of the intervention: diverting PMHI towards services and treatment. Indeed, there are indicative findings that diverted individuals were not successfully diverted to services and/ or failed to receive treatment for their mental health problems in the long term. The review found that the intervention group had an increased likelihood of accessing counselling and prescribed medications in the short term (with statistically significant findings from at least two sites, at 3 months follow up). At 12 months, there is evidence that the intervention group continued to have had an increased likelihood of receiving prescription medication (statistically significant meta-analysis), compared to controls, but not of receiving counselling. Indeed, as Broner et al (2004: 532, 535) report 'neither the study group nor comparison subjects received much treatment, particularly by the 12 month post-diversion acceptance point'. Moreover, the included studies did not provide data to evaluate the effectiveness of specific treatments received by PMHI, such as psychological counselling or drug treatment. Further, it remains unclear as to whether the diverted groups received substantially different services and treatment to the control/ incarcerated individuals. The overall lack of treatment and longer term provision for diverted individuals may help to explain the increase in the longer term risk of arrest (CIT, Portland) and hospitalisation (Crisis Outreach team, Philadelphia). Without 'ensuring that all referrals are linked to services' (Steadman et al., 2001: 4) and appropriate treatment recommendations are adhered to, pre-arrest diversion strategies may be limited in their effectiveness for crime and mental health outcomes. This suggests that the causal mechanism of 'oversight and monitoring after diversion' has a central role in understanding how pre-arrest diversion works. Moreover, it is helpful for evaluations of pre-arrest diversion interventions to consider potentially unintended outcomes and harmful effects together with mechanisms that might underpin these (Bonnell et al., 2015).

Limitations of the evidence base

This systematic review only included two studies, evaluating three different types of intervention implemented in four independent sites. With a growing policy impetus and expanding literature in the field of policing and mental health, this is surprisingly few. The search strategy for the review was broad in remit and relatively comprehensive in terms of the sources used. The search generated a reasonable number of potentially eligible studies, based on title and abstract (n=60), and many of these used high quality designs to evaluate an intervention. However, a high number of these (33) were excluded from the review because they did not measure a post intervention, quantitative crime or mental health outcome. Therefore, we can conclude that there continues to be a dearth of studies that measure longer term outcomes (Parsonage, 2009; Sirotich, 2009). Future evaluations should aim to address this gap, using study designs that can address the challenges of evaluating interventions in real world settings which are notoriously 'very difficult to study' (Compton et al., 2008: 53).

The low methodological quality of the included studies suggests that we need to interpret the results/ the outcome evidence with caution. Elements of the study design may have meant that it was difficult to detect effects. A lack of group equivalence at baseline, for example, may mean that any positive or negative effects may have been

masked by selection bias. The included studies compared groups that were selected through the criminal justice process and this is inherently problematic (Sirotich, 2009). Further, the majority of the findings from individual studies and meta-analyses did not reach statistical significance. This reflects the use of small sample sizes which may mean that they were insufficiently powered to detect effects (Broner et al., 2004; Sirotich, 2009).

The included studies undertook a post-hoc analysis of the characteristics of the intervention participants to consider the role of demographic, health and other variables in influencing their involvement in pre-arrest diversion. Whilst this analysis provided an indication of which types of PMHI may be more likely to be diverted, the studies did not examine whether the interventions were more effective with particular groups. Further studies are therefore required to undertake a theoretically grounded, more robust and comprehensive analysis of whether pre-arrest diversion is more likely to be effective for particular PMHI.

The included studies undertook limited analysis of *why* or *how* police pre-arrest diversion schemes are deemed to work. Future evaluations should aim to build on existing theoretical or empirical analysis (e.g. Shapiro et al., 2015; Watson, 2008) to identify moderating variables and underpinning mechanisms. Moreover, particular attention should be focused on the latter stages of pre-arrest diversion strategies which are currently under-explored.

Economic evaluations should adopt a multi-sector perspective and a time horizon that is sufficiently long to capture all of the potentially important differences in costs and effects between the alternative strategies being compared. Full economic evaluations conducted within the framework of a single study of effects will also need to address the thorny challenge of identifying a suitable comparison group (i.e. a comparable group of non-diverted clients), and measuring and adjusting for potential confounding between diverted and non-diverted groups.

Strengths and limitations of the review

Strengths
The strengths of this systematic review lay in the systematic and transparent way in which the reviewing process has been undertaken. Further details of the main strengths are outlined below:

- Search strategy: the approach was purposely broad and inclusive. The strategy combined two key concepts/ search terms ('police' and 'people with mental health issues') in the database searching to capture a wide range of potentially relevant studies. This meant that the search purposely aimed to avoid specifying particular interventions or policing strategies. Moreover, recognising the topical nature of this issue, the search included a number of government and third sector

organisations to identify relevant work that has recently taken place (e.g. Disley et al., 2016).

- Data extraction: the tool used to extract, code and quality appraise studies built on a number of existing tools (EPPI Centre, 2007; Johnson et al., 2015; NICE, 2012) to capture information for a variety of purposes. The process of data extraction and quality appraisal was carried out by two experienced reviewers. These tools and processes build on and apply accumulated methodological knowledge and serve to lend strength to the review findings.
- Meta-analysis: A framework was used to grade the overall strength of evidence and provide an accessible summary of review findings in terms of the quality and consistency.
- Synthesis: the synthesis of mechanisms, moderators and implementation issues drew on and applied established methods of qualitative research synthesis (Thomas and Harding, 2008) to provide a rigorous and transparent approach.

Limitations

The scope of the review has generated a number of limitations which should be considered when interpreting and applying the findings in a policy or practice context:

- Search: as this review is part of a larger project, the original search was completed at the end of 2015. As police pre-arrest diversion is a topical concern for many higher income countries, more evaluations may have been published in 2016 but this review did not systematically search for them.
- Intervention: focusing specifically on pre-arrest (rather than also post-arrest) diversion interventions meant that evaluations of current UK based diversion programmes were not included in the review (e.g. Offender Liaison and Diversion Trial Schemes).
- Limited number of included studies for synthesis: the inclusion criteria stipulated study type and outcome specifications. These restrictions prioritised studies that would contribute to the meta-analysis of impacts and so provide more robust outcome/ economic data. Due to the state of the literature in this field, and challenges in implementing robust outcome evaluations, these restrictions meant that few studies were identified and synthesised (n=2). Moreover, this meant that the synthesis of mechanisms, moderators and implementation issues was based on the same narrow set of studies when other studies may also have contributed further relevant findings (i.e. studies excluded on study type or outcome: n=14 and n= 33 respectively). Further, evidence for the cost-effectiveness of police pre-arrest diversion programmes, compared with treatment as usual, diversion initiated subsequent to arrest, or an alternative intervention is currently almost entirely lacking
- All included studies analysed populations in the USA: The findings reported in the review are based on evidence solely from the USA. Given the different infrastructures and resources for the public criminal justice and healthcare systems, the findings of this review need to be interpreted with caution when applied to other geographical contexts.
- Crime outcomes: the review assessed the impact of police pre-arrest diversion on crime using 'arrest' as an outcome measure. The arrest of people with mental health problems, however, does not necessarily indicate criminality per se. Arrests often result in no charges being brought and may denote a variety of

other outcomes, e.g. the role of the police as first responders to mental health crises and/ or the increased likelihood that people with mental health issues come into contact with the criminal justice system.

- Mental health outcomes: due to insufficient data, it was only possible to examine the effect of pre-arrest diversion on participants' utilization of mental health services rather than their mental health status per se. Participants' access and use of mental health services, however, may not necessary reflect their mental health or wellbeing. A range of other factors may potentially influence the likelihood of an individual accessing and using mental services, e.g. the availability of services or practitioner decisions to treat individuals.

- Synthesis: there are no reported, formal methods for bringing together the findings from the different syntheses. Therefore, the process for doing so was iterative and organic rather than systematic and pre-planned. Whilst this is a helpful developmental step, it may hinder the transparent and transferability of the review methods.

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Appendices

Appendix 1: Stakeholder/ users consulted

Membership of the Advisory Group

Name	Title	Organisation
Professor Ian Cummins	Senior Lecturer in Social Work	University of Salford
Dr Victoria Herrington	Director Research and Learning	Australian Institute of Police Management
Dr Yasmeen Krameddine	Postdoctoral fellow Department of Psychiatry	University of Alberta

Membership of the Stakeholder Consultation Group

Name	Title	Organisation
Sarah Brennan	Chief Executive	YoungMinds
Dr Wendy Dyer	Senior Lecturer in Criminology	University of Northumbria
Ms Stephanie Kilili	Policy advisor	Office of the Durham Police and Crime Commissioner
Simon Thorneycroft	Mental health coordinator	Office of the Police and Crime Commissioner for Dorset

Appendix 2: Search terms for electronic databases

ASSIA

ti(Police OR policing OR "law enforcement" OR officer* OR YOT OR YOTS OR constable*) OR ab(Police OR policing OR "law enforcement" OR officer* OR YOT OR YOTS OR constable*) OR
su(police OR "police officers" OR "community management" OR arrests OR "police-citizen interactions" OR "crisis intervention")
AND
ti(crisis OR crises OR mentally OR Mental* OR psychiatr* OR vulnerab* OR homeless* OR suicid* OR mind OR "at risk") OR ab(crisis OR crises OR mentally OR Mental* OR psychiatr* OR vulnerab* OR homeless* OR suicid* OR mind OR "at risk") OR
su("mental health" OR "psychiatric disorders" OR "mental health services" OR "mental illness" OR suicide OR "mentally ill people" OR vulnerability OR "mental states" OR "emotional disturbance" OR "therapeutic communication" OR sectioning OR "at risk")

Proquest – Criminal justice abstracts, Psychology Journals

ti(Police OR policing OR "law enforcement" OR officer* OR YOT OR YOTS OR constable*) OR ab(Police OR policing OR "law enforcement" OR officer* OR YOT OR YOTS OR constable*)
OR
SU.EXACT("Police") OR SU.EXACT("Law enforcement") OR SU.EXACT("Community policing")
And
ti(crisis OR crises OR mentally OR Mental* OR psychiatr* OR vulnerab* OR homeless* OR suicid* OR mind) OR mentally OR Mental* OR psychiatr* OR vulnerab* OR homeless* OR suicid* OR mind)
OR
SU.exact("mental health care") OR SU.exact("mental disorders") OR SU.EXACT("Suicides & suicide attempts") OR OR SU.EXACT("Behavior disorders") OR SU.EXACT("Psychiatry") OR SU.EXACT("Personality disorders") OR SU.EXACT("Crisis intervention")

Appendix 3: Data Extraction Tool

- Section A. Administrative details
  - A.1.1.Name of reviewer
  - A.2.1.Date of review
  - A.3.1.Paper (1)
    A.3 Please enter the details of each paper which reports on this item/study and which is used to complete this data extraction.
    (1): A paper can be a journal article, a book, or chapter in a book, or an unpublished report.

    - A.3.2. Unique identifier
    - A.3.3. Authors
    - A.3.4. Title
  - A.3.5.. Paper (2)
    - A.3.6. Unique identifier
    - A.3.7. Authors
    - A.3.8. Title
  - A.3.9. Paper (3)
    Linked study
    - A.3.10. Unique Identifier
    - A.3.11 Authors
    - A.3.12. Title
  - A.3.13. paper (4)
    - A.3.14 Unique identifier
    - A.3.15. Authors
    - A.3.16. Title
  - A.4. If the study has a broad focus and this data extraction focuses on just one component of the study, please specify this here.
    - A.4.1 Not applicable (whole study is focus of data extraction)
    - A.4.2 Specific focus of this data extraction (please specify)
- Section B. Study Aims and Rationale
  - B.1. What are the broad aims of the study?
    Please write in authors' description if there is one. Elaborate if necessary, but indicate which aspects are reviewers' interpretation. Other, more specific questions about the research questions and hypotheses are asked later.
    - B.1.1 Explicitly stated (please specify)
    - B.1.2 Implicit (please specify)
    - B.1.3 Not stated/unclear (please specify)
  - B2. What is the purpose of the study?

- B.2.1 A: Description

  A: Description

  Please use this code for studies in which the aim is to produce a description of a state of affairs or a particular phenomenon, and/or to document its characteristics. In these types of studies there is no attempt to evaluate a particular intervention programme (according to either the processes involved in its implementation or its effects on outcomes), or to examine the associations between one or more variables. These types of studies are usually, but not always, conducted at one point in time (i.e. cross sectional). They can include studies such as an interview of head teachers to count how many have explicit policies on continuing professional development for teachers; a study documenting student attitudes to national examinations using focus groups; a survey of the felt needs of parents using self-completion questionnaires, about whether they want a school bus service.

- B.2.2 B: Exploration of relationships

  B: Exploration of relationships

  Please use this code for a study type which examines relationships and/or statistical associations between variables in order to build theories and develop hypotheses. These studies may describe a process or processes (what goes on) in order to explore how a particular state of affairs might be produced, maintained and changed.

  These relationships may be discovered using qualitative techniques, and/or statistical analyses. For instance, observations of children at play may elucidate the process of gender stereotyping, and suggest the kinds of interventions which may be appropriate to reduce any negative effects in the classroom. Complex statistical analysis may be helpful in modelling the relationships between parents' social class and language in the home. These may lead to the development of theories about the mechanisms of language acquisition, and possible policies to intervene in a causal pathway.

  These studies often consider variables such as social class and gender which are not interventions, although these studies may aid understanding, and may suggest possible interventions, as well as ways in which a programme design and implementation could be improved. These studies do not directly evaluate the effects of policies and practices

- B.2.3 C: What works?

  C: What works

A study will only fall within this category if it measures effectiveness - i.e. the impact of a specific intervention or programme on a defined sample of recipients or subjects of the programme or intervention

- B.2.4 D: Methods development
  D: Methods development
  Studies where the principle focus is on methodology.

- B.2.5 E: Reviewing/synthesising research
  E: Reviewing/Synthesising research
  Studies which summarise and synthesise primary research studies.

- B3. Do authors report how the study was funded?
  - B.3.1 Explicitly stated (please specify)
  - B.3.2 Implicit (please specify)
  - B.3.3 Not stated/unclear (please specify)
- B4. When was the study carried out?
  If the authors give a year, or range of years, then put that in. If not, give a 'not later than' date by looking for a date of first submission to the journal, or for clues like the publication dates of other reports from the study.
  - B.4.1 Explicitly stated (please specify)
  - B.4.2 Implicit (please specify)
  - B.4.3 Not stated/unclear (please specify)
- B5. What are the study research questions and/or hypotheses?
  Research questions or hypotheses operationalise the aims of the study. Please write in authors'description if there is one. Elaborate if necessary, but indicate which aspects are reviewers' interpretation
  - B.5.1 Explicitly stated (please specify)
  - B.5.2 Implicit (please specify)
  - B.5.3 Not stated/ unclear (please specify)
- Section C. Actual sample
  - C1. What was the total number of participants in the study (the actual sample)?
    if more than one group is being compared, please give numbers for each group
    - C.1.1 Not applicable (e.g study of policies, documents etc)
    - C.1.2 Explicitly stated -total
    - C.1.3. Explicititly stated - intervention
    - C.1.4.Explicitly stated - control group
    - C.1.5 Not stated/ unclear (please specify)

- C.2 What is the sex of the individuals in the actual sample?
  Please give the numbers of the sample that fall within each of the given categories. If necessary refer to a page number in the report (e.g. for a useful table).

  If more than one group is being compared, please describe for each group.

  - C.2.1 Not applicable (e.g. study of policies, documents etc)
  - C.2.2 Explicitly stated (please specify)
  - C.2.3 Implicit (please specify)
  - C.2.4 Not stated/unclear (please specify)
- C.3 What is the socio-economic status of the individuals within the actual sample?
  If more than one group is being compared, please describe for each group.
  - C.3.1 Not applicable (e.g. study of policies, documents etc)
  - C.3.2 Explicitly stated (please specify)
  - C.3.3 Implicit (please specify)
  - C.3.4 Not stated/unclear (please specify)
- C.4. What is the ethnicity of the individuals within the actual sample?
  If more than one group is being compared, please describe for each group.
  - C.4.1 Not applicable (e.g. study of policies, documents etc)
  - C.4.2 Explicitly stated (please specify)
  - C.4.3 Implicit (please specify)
  - C.4.4 Not stated/unclear (please specify)
- C.5. Other characteristics of the sample (ADD)
  - Age
  - Alcohol/ drug use or dependance
  - Dual diagnosis
  - Mental health status
  - Employment status
  - Homeless
  - Income
  - Married or living with a partner
  - Physical health
  - Previous arrests/ convictions
  - Education (level)
  - Victimization
  - Trauma history
  - Symptomology
  - Quality of Life
  - Characteristics of police officer
- Section D. Programme or Intervention description

- D.1 Country where intervention carried out; SEE MAP
  Add child codes for new countires (as selectable) or select country code
  - USA
  - United Kingdom
  - Sweden
  - Finland
  - Cyprus
  - Germany
  - Spain
  - not stated
  - Canada
  - Western Europe
  - New Zealand
  - Australia
- D.2 Location of intervention
  - D.2.1 Community
  - Not clear
  - Other (ADD)
- D.3 Type of Intervention (SEE MAP - NAME OF PROGRAMME/ MODEL)
  - Crisis Intervention Team
  - Co-responding police and mental health staff team
  - Co-responding police and mental health staff support
    Includes street triage
  - Link scheme
- Funder
  - SAMHSA
  - Implicit
- D.6 Content of the intervention package
  - D.6.1 Details
  - 24 hour community based mental health crisis centre
  - Brokerage
  - Identification
  - Mental health assessment
  - Police officer decision to divert instead of arrest
    the decision to divert was taken by police officers at the time of the police encounter.
  - Policy
  - Psychiatric service, Medical centre
  - Peer specialist
    People who have had experience of mental health problems and of accessing services

- Police officer training
- Multiagency working
- D.7 Aim(s) of the intervention (theory of change)
  - D.7.1 Not stated
  - D.7.2 Implicit (Write in, as worded by the reviewer)
    - Criminalization hypothesis
    - Theory of recovery
  - D.7.3 Explicitly Stated (Write in, as stated by the authors)
    - Labelling theory
- D.8 Year intervention started
  Where relevant
  - D.8.1 Details
  - D. 8. 2 Not stated
- D.9 Duration of the intervention
  Choose the relevant category and write in the exact intervention length if specified in the report

  When the intervention is ongoing, tick 'OTHER' and indicate the length of intervention as the length of the outcome assessment period

  - D.9.1 Not stated
  - D.9.2 Not applicable
  - D.9.3 Unclear
  - D.9.4 One day or less (please specify)
  - D.9.5 1 day to 1 week (please specify)
  - D.9.6 1 week (and 1 day) to 1 month (please specify)
  - D.9.7 1 month (and 1 day) to 3 months (please specify)
  - D.9.8 3 months (and 1 day) to 6 months (please specify)
  - D.9.9 6 months (and 1 day) to 1 year (please specify)
  - D.9.10 1 year (and 1 day) to 2 years (please specify)
  - D.9.11 2 years (and 1 day) to 3 years (please specify)
  - D.9.12 3 years (and 1 day) to 5 years (please specify)
  - D.9.13 more than 5 years (please specify)
  - D.9.14 Other (please specify)
- D.11 Person providing the intervention
  (tick as many as appropriate)
  - D.11.2 mental health professional (please specify)
  - D.11.6 Researcher
  - D.11.7 Social worker
  - Link worker
  - D.11.12 Police Officer
  - D.11.13 Other (specify)

- D.11.14 Unstated/ not clear
- Peers
- D.12 Was special training given to people providing the intervention?
  - D.12.1 Not stated
  - D.12.2 Unclear
  - D.12.3 Yes (please specify)
  - D.12.4 No
  - not applicable
- D.10 Intensity of the Intervention
  - D.10.1 Daily
  - D.10.3 2-4 per week
  - D.10.2 1-2 per week
  - D.10.4 less than weekly (give frequency)
  - D.10.5 Unclear/ not stated
  - not applicable
- D.13 What treatment/ intervention did the control/comparison group receive
  - D.13.1 No control group
    Use this code if participants acted as own control e.g. in pre-post test design
  - D.13.2 treatment as usual (please specify)
  - D.13.4 Not stated/ unclear
  - Post-booking
  - Not diverted
  - no treatment
- Section E. results and conclusions
  - E.1 outcomes
    - Primary: Crime
      - Arrest
      - Charged with new offence
      - Days incarceration
      - Rearrest
      - Recidivism
      - Time to recidivism
      - Time to arrest
    - Secondary: Mental Health and Wellbeing
      - Mental health
      - Physical health
      - Quality of life
      - Substance use
    - Other Outcomes

- Homeless
- Victimization
- Service use
- Symptomology
- Text
- E.2. Follow up period
  - E.2.1.At completion of the programme
  - E.2.2. One month
  - E.2.3. Three months
  - E.2.4. Six months
  - E.2.9 Eight months
  - E.2.10 Eleven months
  - E.2.5. Twelve months
  - E.2.6. 13-21 months
  - E.2.7. 22-36 months
  - E.2.8 4-5 years
  - E.2.11 Throughout treatment
    Adva (2008)
  - E.2.0. No follow up period reported
- E.3. Programme completion rate/ attrition
  - E.3.1. Attrition rate reported (details)
  - E.3.2. Attrition rate not reported
  - Not applicable
- E.7 What do the author(s) conclude about the findings of the study?
  Please give details and refer to page numbers in the report of the study, where necessary.
  - E.7.1 Details
- *Section F. Study Method
  - F.1 Study Timing
    Please indicate all that apply and give further details where possible
    - F.1.1 Cross-sectional
      -If the study examines one or more samples but each at only one point in time it is cross-sectional
    - F.1.2 Retrospective
      If the study examines the same samples but as they have changed over time, it is a retrospective, provided that the interest is in starting at one timepoint and looking backwards over time
    - F.1.3 Prospective

      -If the study examines the same samples as they have changed over time and if data are collected forward over time, it is prospective

provided that the interest is in starting at one timepoint and looking forward in time

- F.1.4 Not stated/ unclear (please specify)
- Longitudinal
- F.2 when were the measurements of the variable(s) used as outcome measures made, in relation to the intervention
  Use only if the purpose of the study is to measure the effectiveness or impact of an intervention or programme i.e its purpose is coded as 'What Works' in Section B2 -

  If at least one of the outcome variables is measured both before and after the intervention, please use the 'before and after' category.

  - F.2.1 Not applicable (not an evaluation)
  - F.2.2 Before and after
  - F.2.3 Only after
  - F.2.4 Other (please specify)
  - F.2.5 Not stated/unclear (please specify)
- F.3 What is the method used in the study?
  NB: Studies may use more than one method please code each method used for which data extraction is being completed and the respective outcomes for each method.
  - F.3.1 A=Random experiment with random allocation to groups
    F.3 What is the method used in the study?
    NB: Studies may use more than one method please code each method used for which data extraction is being completed and the respective outcomes for each method.

    A=Please use this code if the outcome evaluation employed the design of a randomised controlled trial. To be classified as an RCT, the evaluation must:

    i). compare two or more groups which receive different interventions or different intensities/levels of an intervention with each other; and/or with a group which does not receive any intervention at all AND
    ii) allocate participants (individuals, groups, classes, schools, LEAs etc) or sequences to the different groups based on a fully random schedule (e.g a random numbers table is used). If the report states that random allocation was used and no further information is given then please keyword as RCT. If the allocation is NOT fully randomised (e.g allocation by alternate numbers by date of birth) then please keyword

as a non-randomised controlled trial

- F.3.2 B=Experiment with non-random allocation to groups
  B=Please use this code if the evaluation compared two or more groups which receive different interventions, or different intensities/levels of an intervention to each other and/or with a group which does not receive any intervention at all BUT DOES NOT allocate participants (individuals, groups, classes, schools, LEAs etc) or sequences in a fully random manner. This keyword should be used for studies which describe groups being allocated using a quasi-random method (e.g allocation by alternate numbers or by date of birth) or other non-random method
- F.3.3 C=One group pre-post test
  C=Please use this code where a group of subjects is tested on outcome of interest before being given an intervention which is being evaluated. After receiving the intervention the same test is administered again to the same subjects. The outcome is the difference between the pre and post test scores of the subjects.
- F.3.4 D=one group post-test only
  D=Please use this code where one group of subjects is tested on outcome of interest after receiving the intervention which is being evaluated
- F.3.5 E=Cohort study
  E=Please use this code where researchers prospectively study a sample (e.g learners), collect data on the different aspects of policies or practices experienced by members of the sample (e.g teaching methods, class sizes), look forward in time to measure their later outcomes (e.g achievement) and relate the experiences to the outcomes achieved. The purpose is to assess the effect of the different experiences on outcomes.

- F.3.6 F=Case-control study
  F=Please use this code where researchers compare two or more groups of individuals on the basis of their current situation (e.g 16 year old pupils with high current educational performance compared to those with average educational performance), and look back in time to examine the statistical association with different policies or practices which they have experienced (e.g class size; attendance at single sex or mixed sex schools; non school activities etc).
- F.3.7 G=Statistical survey
  G= please use this code where researchers have used a quesionnaire to collect quantitative information about items in a sample or population e.g parents views on education

- F.3.8 H=Views study

  H= Please use this code where the the researchers try to understand phenonmenon from the point of the 'worldview' of a particular, group, culture or society. In these studies there is attention to subjective meaning, perspectives and experience'.

- F.3.9 I=Ethnography

  I= please use this code when the researchers present a qualitative description of human social phenomena, based on fieldwork

- F.3.10 J=Systematic review

  J= please use this code if the review is explicit in its reporting of a systematic strategy used for (i) searching for studies (i.e it reports which databases have been searched and the keywords used to search the database, the list of journals hand searched, and describes attempts to find unpublished or 'grey' literature; (ii) the criteria for including and excluding studies in the review and, (iii) methods used for assessing the quality and collating the findings of included studies.

- F.3.11 K=Other review (non systematic)

  K= Please use this code for cases where the review discusses a particular issue bringing together the opinions/findings/conclusions from a range of previous studies but where the review does not meet the criteria for a systematic review (as defined above)

- F.3.12 L=Case study

  L= please use this code when researchers refer specifically to their design/ approach as a 'case study'. Where possible further information about the methods used in the case study should be coded

- F.3.13 M= Document study

  M=please use this code where researchers have used documents as a source of data e.g newspaper reports

- F.3.14 N=Action research

  N=Please use this code where practitioners or institutions (with or without the help of researchers) have used research as part of a process of development and/or change. Where possible further information about the research methods used should be coded

- F.3.15 O= Methodological study

  O=please use this keyword for studies which focus on the development or discussion of methods; for example discussions of a statistical technique, a recruitment or sampling procedure, a particular way of collecting or analysing data etc. It may also refer to a description of the processes or stages involved in developing an 'instrument' (e.g an assessment procedure).

- F.3.16 P=Secondary data analysis
  P= Please use this code where researchers have used data from a pre-existing dataset e.g The British Household Panel Survey to answer their 'new' research question.

- **Section G: Methods-treatment of groups
  - G.1 If Comparisons are being made between two or more groups*, s
    please specify the basis of any decisions made for making these comparison
    Please give further details where possible

    *If no comparisons are being made between groups please continue to Section I (Methods - sampling strategy)

    - G.1.1 Not applicable (not more than one group)
    - G.1.2 Prospective allocation into more than one group
      e.g allocation to different interventions, or allocation to intervention and control groups
    - G.1.3 No prospective allocation but use of pre-existing differences to create comparison groups
      e.g. receiving different interventions or characterised by different levels of a variable such as social class
  - G.2 How do the groups differ?
    - G.2.1 Not applicable (not in more than one group)
    - G.2.2 Explicitly stated (please specify)
    - G.2.3 Implicit (please specify)
    - G.2.4 Not stated/ unclear (please specify)
  - G.3 Number of groups
    For instance, in studies in which comparisons are made between group, this may be the number of groups into which the dataset is divided for analysis (e.g social class, or form size), or the number of groups allocated to, or receiving, an intervention.
    - G.3.1 Not applicable (not more than one group)
    - G.3.2 One
    - G.3.3 Two
    - G.3.4 Three
    - G.3.5 Four or more (please specify)
    - G.3.6 Other/ unclear (please specify)
  - G.4 If prospective allocation into more than one group, what was the unit of allocation?
    - G.4.1 Not applicable (not more than one group)
    - G.4.2 Not applicable (no prospective allocation)

- G.4.3 Individuals
- G.4.4 Groupings or clusters of individuals (e.g classes or schools) please specify
- G.4.5 Other (e.g individuals or groups acting as their own controls - please specify)
- G.4.6 Not stated/ unclear (please specify)
- G.5 If prospective allocation into more than one group, which method was used to generate the allocation sequence?
  - G.5.1 Not applicable (not more than one group)
  - G.5.2 Not applicable (no prospective allocation)
  - G.5.3 Random
  - G.5.4 Quasi-random
  - G.5.5 Non-random
  - G.5.6 Not stated/unclear (please specify)
- G.6 If prospective allocation into more than one group, was the allocation sequence concealed?
  Bias can be introduced, consciously or otherwise, if the allocation of pupils or classes or schools to a programme or intervention is made in the knowledge of key characteristics of those allocated. For example, children with more serious reading difficulty might be seen as in greater need and might be more likely to be allocated to the 'new' programme, or the opposite might happen. Either would introduce bias.
  - G.6.1 Not applicable (not more than one group)
  - G.6.2 Not applicable (no prospective allocation)
  - G.6.3 Yes (please specify)
  - G.6.4 No (please specify)
  - G.6.5 Not stated/unclear (please specify)
- **Section H: Methods - Sampling strategy
  - H.1 What is the sampling frame (if any) from which the partipants are chosen?
    e.g.court records etc

    - H.1.1 Not applicable (please specify)
    - H.1.2 Explicitly stated (please specify)
    - H.1.3 Implicit (please specify)
    - H.1.4 Not stated/unclear (please specify)
  - H.2 Which method does the study use to select people, or groups of people (from the sampling frame)?
    e.g. selecting people at random, systematically - selecting, for example, every 5th person, purposively, in order to reach a quota for a given characteristic.

- - H.2.1 Not applicable (no sampling frame)
  - H.2.2 Explicitly stated (please specify)
  - H.2.3 Implicit (please specify)
  - H.2.4 Not stated/unclear (please specify)
  - H.3 How representative was the achieved sample (as recruited at the start of the study) in relation to the aims of the sampling frame?
    - H.3.1 Not applicable (e.g. study of policies, documents, etc.)
    - H.3.2 Not applicable (no sampling frame)
    - H.3.3 High (please specify)
    - H.3.4 Medium (please specify)
    - H.3.5 Low (please specify)
    - H.3.6 Unclear (please specify)
  - H.4 If the study involves studying samples prospectively over time, what proportion of the sample dropped out over the course of the study?
    If the study involves more than one group, please give drop-out rates for each group separately. If necessary, refer to a page number in the report (e.g. for a useful table).
    - H.4.1 Not applicable (e.g. study of policies, documents, etc.)
    - H.4.2 Not applicable (not following samples prospectively over time)
    - H.4.3 Explicitly stated (please specify)
    - H.4.4 Implicit (please specify)
    - H.4.5 Not stated/unclear (please specify)
  - H.5 For studies that involve following samples prospectively over time, do the authors provide any information on whether, and/or how, those who dropped out of the study differ from those who remained in the study?
    - H.5.1 Not applicable (e.g. study of policies, documents, etc.)
    - H.5.2 Not applicable (not following samples prospectively over time)
    - H.5.3 Not applicable (no drop outs)
    - H.5.4 Yes (please specify)
    - H.5.5 No
  - H.6 If the study involves following samples prospectively over time, do authors provide baseline values of key variables, such as those being used as outcomes, and relevant socio-demographic variables?
    - H.6.1 Not applicable (e.g. study of policies, documents, etc.)
    - H.6.2 Not applicable (not following samples prospectively over time)
    - H.6.3 Yes (please specify)
    - H.6.4 No
- **Section I: Methods - recruitment and consent
  - I.1 Which methods are used to recruit people into the study?
    e.g.voluntary, court-mandated
    - I.1.1 Not applicable (please specify)
    - I.1.2 Explicitly stated (please specify)

- I.1.3 Implicit (please specify)
- I.1.4 Not stated/unclear (please specify)
- I.1.5 Please specify any other details relevant to recruitment and consent
- I.2 Were any incentives/ disincentives (such as penalties for non-participation) provided to recruit people into the study?
  - I.2.1 Not applicable (please specify)
  - I.2.2 Explicitly stated (please specify)
  - I.2.3 Not stated/unclear (please specify)
- I.3 Was consent sought?
  Please comment on the quality of consent, if relevant.
  - I.3.1 Not applicable (please specify)
  - I.3.2 Participant consent sought
  - I.3.3 Other consent sought
  - I.3.4 Consent not sought
  - I.3.5 Not stated/unclear (please specify)
- **Section J: Methods - Data Collection
  - J.1 Which methods were used to collect the data?
    Please indicate all that apply and give further detail where possible
    - J.1.1 Criminal Justice System records
    - J.1.2 Focus group interview
    - J.1.3 One-to-one interview (face to face or by phone)
    - J.1.4 Observation
    - J.1.5 Self-completion questionnaire (unspecified)
    - J.1.6 self-completion report or diary
    - J.1.7 Examinations
    - J.1.8 Clinical test
    - J.1.9 Practical test
    - J.1.10 Psychological test (unspecified)
    - J.1.11 Hypothetical scenario including vignettes
    - J.1.12 Secondary data such as publicly available statistics
    - J.1.13 Other documentation
    - J.1.14 Not stated/ unclear (please specify)
    - J.1.15 Please specify any other important features of data collection
  - J.2 Details of data collection instruments or tool(s).
    Please provide details including names for all tools used to collect data, and examples of any questions/items given. Also, please state whether source is cited in the report
    - J.2.3 Not stated/ unclear (please specify)
    - Not applicable (no instruments/ tools used)
    - Antisocial Personality Symptoms
      This measure is a checklist of 11 symptoms described in the fourth

> *edition of the Diagnostic and Statistical Manual of Mental Disorders*
> *(American Psychiatric Association 1994) under Antisocial*
> *Personality Disorder.*

- Borderline Personality Organization
  BPO; Oldham et al.
  1985). This instrument is a 30-item self-report measure that assesses
  components of a borderline personality
- Colorado Symptom Index (CSI)
  Shern et al 1994
  mental health status
- Drug Abuse Screening Test (DAST)
  Skinner 1982
- The Dartmouth Drug/Alcohol 6-Month Follow-Back Calendar
  A follow-back approach using a calendar and
  other cues to prompt memory.
  (Follow-Back Calendar; Dartmouth Psy-
  chiatric Research Center, 1997)
  Lehman Quality of Life Interview (QOLI)
  Lehman 1988
  general life satisfaction, finances, living situation, health/ daily
  activities, personal safety
- Michigan Alcohol Screening Test (MAST)
  Storgaard et al 1994
- SF-12 Health Survey
  derived from the SF-36, a general health and wellbeing measure

- J.3 Do the authors' describe any ways they addressed the repeatability or reliability of their data collection tools/methods?
  e.g test-re-test methods
  (where more than one tool was employed, please provide details for each)

  - J.3.1 Yes. DetailsJ.3.2. No

- J.4 Do the authors describe any ways they have addressed the validity or trustworthiness of their data collection tools/methods?
  e.g mention previous piloting or validation of tools, published version of tools, involvement of target population in development of tools.
  (Where more than one tool was employed, please provide details for each)

  - J.4.1 Yes. Details
  - No

- J.5 Was there a concealment of which group that subjects were assigned to (i.e. the intervention or control) or other key factors from those carrying out measurement of outcome - if relevant?
  Not applicable - e.g analysis of existing data, qualitative study.

No - e.g assessment of reading progress for dyslexic pupils done by teacher who provided intervention

Yes - e.g researcher assessing pupil knowledge of drugs - unaware of whether pupil received the intervention or not.

- J.5.1 Not applicable (please say why)
- J.5.2 Yes (please specify)
- J.5.3 No (please specify)
- Section K: Methods - data analysis
  - K.1 Which methods were used to analyse the data?
    Please give details of of approach methods including statistical methods.
    - K.1.1 Explicitly stated (please specify)
    - K.1.2 Implicit (please specify)
    - K.1.3 Not stated/unclear (please specify)
    - K.1.4 Please specify any important analytic or statistical issues
- Section M. MMI
  Mechanisms, Moderators and implementation
  - Mechanisms
    - Mentioned by author
      - Assistance in accessing care post-crisis
      - Diversion at earliest opportunity
      - Identification of mental illness
      - No refusal policy
      - Peer Specialist
      - Police powers of discretion
      - PWMI Consent and Participation
      - Skills and beliefs of police officers
    - Evidenced in wider literature
      - Criminal justice process
      - No refusal policy
      - Personalisation of response
        Risk-Needs-Responsivity
      - Skills and beliefs of police officers
    - Evidenced by findings in the study
      - Police powers of discretion
  - Moderators
    - Mentioned by author
      - Barriers/ facilitators to access
      - Criminal history
      - Days at risk

- Homelessness
- Substance abuse
- Mental health condfition
- Prior contact with mental health services
- Specialised police response model
- Study design
- treatment maintenance
- Social support
- Life history
- Perceived likelihood and severity of criminal justice sanctions
- Offense type
  - Evidenced in wider literature
    - Substance abuse
    - Criminal history
  - Evidenced by the findings in the study
    - Referring officer characteristics
    - Interview tools
    - Demographic characteristics
    - Criminal history
    - Mental health
    - Substance use
    - Days at risk
      defined as days not institutionalised, in jail, prison, hospital, or residential treatment, as so potentially free to commit offences
    - Functioning and quality of life
    - Barriers/ facilitators to access
    - Treatment maintenance
    - Comparability of groups
    - Study design
  - SEE ALSO Section C: sample characteristics
- Implementation
  - Mentioned by author
    - Awareness of the programme
    - Multiagency working
    - Timing of response
    - Police training
    - Crisis centre
    - Police time
    - Access to emergency department
    - access to services
    - Attrition

- Assessment
- Evidenced in wider literature
  - Information sharing
  - Access to services
- Evidenced by findings in the study

Appendix 4: Quality Appraisal Tool- Internal Validity

- Theoretical approach
  This section deals with the underlying theory and principles applied to the research
  - 1. Is a cohort study approach appropriate?
    - Appropriate
    - Inappropriate
    - Not sure
    - Comments (write in)
      Click to write in comments.
      So that the grade awarded for each study aspect is as transparent as possible.

  - 2. Is the study clear in what it seeks to do?
    For example:
    Is the purpose of the study discussed – aims/objectives/research question(s)?
    Is there adequate/appropriate reference to the literature?
    Are underpinning values/ assumptions/ theory discussed? [p.199]
    - Clear
    - Unclear
    - Mixed
    - Comments (write in)
      Click to write in comments.
      So that the grade awarded for each study aspect is as transparent as possible.
- A. Selection bias
  Systematic differences between the comparison groups
  - A1: Allocation unrelated to confounding factors?
    The method of allocation to intervention groups was unrelated to potential confounding factors (that is, the reason for participant allocation to intervention groups is not expected to affect the outocme(s) under study).
    - Yes
    - No
    - Unclear
    - N/A
  - A2: Attempts made to balance the comparison groups?
    Attempts were made within the design or analysis to balance the comparison groups for potential confounders.
    - Yes

- No
- Unclear
- N/A
- A3: Groups comparable at baseline?
  The groups were comparable at baseline, including all major confounding factors
  - Yes
  - No
  - Unclear
  - N/A
- A4: Was selection bias present?
  Base your overall assessment on your previous answers within A.
  - Low risk of bias
  - Unclear/unknown risk
  - High risk of bias
- A5: Likely direction of selection bias effect
  Describe the influence bias might have on the study results
- B. Performance bias
  Systematic differences between groups in the care provided, apart from the intervention under investigation
  - B1: Equal treatment?
    Did the comparison groups receive the same care and support apart from the intervention/s studied?
    - Yes
    - No
    - Unclear
    - N/A
  - B2: Allocation - participants
    Were the participants receiving care and support kept 'blind' to how the intervention/s were allocated?
    - Yes
    - No
    - Unclear
    - N/A
  - B3: Allocation - practitioners
    Were individuals who administered the care and support kep 'blind' to the intervention allocation?
    - Yes
    - No
    - Unclear
    - N/A

- B4: Performance bias appraisal
  Tick one of the options below and justify your choice by adding information.
  - Low risk of bias
    Please state why there is low risk of bias in this study.
  - Unclear/unknown risk of bias
    Specify why you are unclear as to whether there is a risk of bias in this study. Could more information be obtained to ascertain questions of bias?
  - High risk of bias (also A5)
    Specify why there is a risk of bias in this study.
- B5: Likely direction of performance bias effect
- C. Attrition bias
  Systematic differences between the comparison groups with respect to loss of participants.
  - C1: Follow-up
    Where all groups followed up for an equal length of time, or was the analysis adjusted to allow for differences in length to follow-up?
    - Yes
    - No
    - Unclear
    - N/A
  - C2 a) Drop-out numbers
    How many people dropped out of each group, include both intervention group/s and comparison group/s. If more than one of each group, let lead systematic reviewer know and an additional child code will be added to accommodate this.
    - Intervention drop-outs
      State number of drop-outs in the intervention group
    - Comparison drop-outs
      State number of drop-outs in the comparison/control group
  - C2 b) Groups comparable on intervention completion?
    Were the groups comparable in terms of who completed the intervention, where there any systematic differences between those who did not complete the intervention?
    - Yes
    - No
    - Unclear
    - N/A
  - C3 a) Missing outcome data
    For how many participants in each group were no outcome data available? If more than one of each group, let lead systematic reviewer know and an additional child code will be added to accommodate this.

- Interv. missing outcome data
  State number of participants with missing outcomes in the intervention group.
- Compar. missing outcome data
  State number of participants with missing outcome data in the comparison group

- C3 b) Groups comparable on available data?
  Were the groups comparable with respect to the availability of outcome data: that is, there were no important or systematic differences between groups in terms of those for whom outcome data were not available?
  - Yes
  - No
  - Unclear
  - N/A
- C4: Attrition bias appraisal
  Attrition bias relates to whether there was systematic differences between the comparison groups with respect to loss of participants.
  - Low risk of bias
  - Unclear/unknown risk of bias
    Specify why you are unclear as to whether there is a risk of bias in this study. Could more information be obtained to ascertain questions of bias?
  - High risk of bias (also A5)
    Specify why there is a risk of bias in this study.
- C5: Likely direction of attrition bias effect
  Describe the influence bias might have on the study results.

- D. Detection bias
  Bias in how outcomes are ascertained, diagnosed or verified.
  - D1: Did the study have an appropriate length to follow-up?
    It seems reasonable to expect a change for service users, and for this change to be measurable, in the time between study implementation and the time when outcome data were selected.
    - Yes
    - No
    - Unclear
    - N/A
  - D2 Did the study use a precise definition of outcome?
    - Yes
    - No
    - Unclear
    - N/A
  - D3: Was the method used to determine the outcome valid and reliable?

- Yes
- No
- Unclear
- N/A
- D4: Were investigators kept 'blind' to participants' explosure to the intervention?
    - Yes
    - No
    - Unclear
    - N/A
- D4: Were investigators kept 'blind' to other important confounding factors?
    - Yes
    - No
    - Unclear
    - N/A
- E. Do conclusions match findings?
  Do the authors overall conclusions match with the findings presented in tables and more detailed text? Do you agree with the overall findings, considering the evidence they have presented in the article? For example, authors might emphasise statistically significant results only, and the text might ignore important non-significant or harmful effects which are evidenced in the findings tables. For narrative reviews it is worth looking at whether you agree with their overall assessment of the effectiveness on outcomes considering the quality of studies and the strengths of their effects.
    - Yes
      The conclusions you get from reading the tables/detailed findings descriptions match the authors' conclusions.
    - Partly
      There is overall a good match between findings and conclusions, although findings in relation to one important outcome appear to have been ignored/overlooked by the conclusions.
    - No
      The conclusions you draw from the detailed descriptions and tables of findings are different from the authors. Please provide detailed reasons why this is so.
- Overall Internal validity Score
  Overall assessment of internal validity
    - ++
      All or most of the checklist criteria have been fulfilled, where they have not been fulfilled the conclusions are very unlikely to alter

- +

  Some of the checklist criteria have been fulfilled, where they have not been fulfilled, or not adequately described, the conclusions are unlikely to alter.

- -

  Few or no checklist criteria have been fulfilled and the conclusions are likely or very likely to alter.

Appendix 5: SAMSHA reports used in the review

Site	Intervention	Study reports	
		Site and intervention specific details and outcomes	Additional detail
Memphis	Police led: CIT	Broner et al., 2004 Cowell et al., 2004 Lattimore et al., 2002	Lattimore et al., 2003 Steadman et al., 2001
Portland	Police led: CIT	Broner et al., 2004 Lattimore et al., 2002 Gratton et al., 2001	Lattimore et al., 2003 Steadman et al., 2001
Philadelphia	Co-responding team: Crisis Outreach Team	Broner et al., 2004 Lattimore et al., 2002	Lattimore et al., 2003 Steadman et al., 2001

Appendix 6: Summary of methodological features of included studies

Features	Bonkiewicz et al (2014) Link Scheme	Broner et al (2004) CIT, Memphis	Broner et al (2004) CIT, Portland	Broner et al (2004), Crisis Outreach Team, Philadelphia
Study design	Matched comparison group using post-hoc analysis to control for differences	Matched comparison group using post-hoc analysis to control for differences	Matched comparison group using post-hoc analysis to control for differences	Matched comparison group using post-hoc analysis to control for differences
Intervention group (n)	166	301	73	64
Control group (n)	573	308	132	69
Total (n)	739	609	205	133
Data collection	One-to-one interviews	One-to-one interviews	One-to-one interviews	One-to-one interviews
Data analysis	Propensity Score Matching	Multivariate analysis	Multivariate analysis	Multivariate analysis

Appendix 7: Crime and mental health outcomes measured by included studies

Study	Criminal Justice Outcomes	Mental Health Outcomes	Mental Health Related Service Use Outcomes	Timing of outcomes after diversion
Broner et al. 2004	Any arrest Number of arrests (Self-report)	Mental health status (CSI and MHC)	• Use of mental health counseling • Use of mental health medications • Use of mental health hospitalization (Self-report)	3 months 12 months
Bonkiewicz et al 2014	Any arrest (Official records)		• Number of mental health calls for service • Any incidence of emergency protective custody (Official records)	6 months