Compulsory Community and Involuntary Outpatient Treatment for People With Severe Mental Disorders — Source link ↗

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Compulsory community and involuntary outpatient treatment for people with severe mental disorders

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A B S T R A C T

Background

There is controversy as to whether compulsory community treatment (CCT) for people with severe mental illness (SMI) reduces health service use, or improves clinical outcome and social functioning.

Objectives

To examine the effectiveness of CCT for people with SMI.

Search methods

We searched the Cochrane Schizophrenia Group's Trials Register and Science Citation Index (2003, 2008, and 2012). We obtained all references of identified studies and contacted authors where necessary. We further updated this search on the 8 November 2013.

Selection criteria

All relevant randomised controlled clinical trials (RCTs) of CCT compared with standard care for people with SMI (mainly schizophrenia and schizophrenia-like disorders, bipolar disorder, or depression with psychotic features). Standard care could be voluntary treatment in the community or another pre-existing form of compulsory community treatment such as supervised discharge.

Data collection and analysis

Review authors independently selected studies, assessed their quality and extracted data. We used The Cochrane Collaboration's tool for assessing risk of bias. For binary outcomes, we calculated a fixed-effect risk ratio (RR), its 95% confidence interval (CI) and, where possible, the weighted number needed to treat statistic (NNT). For continuous outcomes, we calculated a fixed-effect mean difference (MD) and its 95% CI. We used the GRADE (Grading of Recommendations Assessment, Development and Evaluation) approach to create a 'Summary of findings' table for outcomes we rated as important and assessed the risk of bias of included studies.

Main results

All studies (n=3) involved patients in community settings who were followed up over 12 months (n = 752 participants).

Two RCTs from the USA (total n = 416) compared court-ordered 'Outpatient Commitment' (OPC) with voluntary community treatment. OPC did not result in significant differences compared to voluntary treatment in any of the main outcome indices: health

service use (2 RCTs, n = 416, RR for readmission to hospital by 11-12 months 0.98 CI 0.79 to 1.21, *low grade evidence*); social functioning (2 RCTs, n = 416, RR for arrested at least once by 11-12 months 0.97 CI 0.62 to 1.52, *low grade evidence*); mental state; quality of life (2 RCTs, n = 416, RR for homelessness 0.67 CI 0.39 to 1.15, *low grade evidence*) or satisfaction with care (2 RCTs, n = 416, RR for perceived coercion 1.36 CI 0.97 to 1.89, *low grade evidence*). However, risk of victimisation decreased with OPC (1 RCT, n = 264, RR 0.50 CI 0.31 to 0.80). Other than perceived coercion, no adverse outcomes were reported. In terms of numbers needed to treat (NNT), it would take 85 OPC orders to prevent one readmission, 27 to prevent one episode of homelessness and 238 to prevent one arrest. The NNT for the reduction of victimisation was lower at six (CI 6 to 6.5).

One further RCT compared community treatment orders (CTOs) with less intensive supervised discharge in England and found no difference between the two for either the main outcome of readmission (1 RCT, n = 333, RR for readmission to hospital by 12 months 0.99 CI 0.74 to 1.32, *medium grade evidence*), or any of the secondary outcomes including social functioning and mental state. It was not possible to calculate the NNT. The English study met three out of the seven criteria of The Cochrane Collaboration's tool for assessing risk of bias, the others only one, the majority being rated unclear.

Authors' conclusions

CCT results in no significant difference in service use, social functioning or quality of life compared with standard voluntary care. People receiving CCT were, however, less likely to be victims of violent or non-violent crime. It is unclear whether this benefit is due to the intensity of treatment or its compulsory nature. Short periods of conditional leave may be as effective (or non-effective) as formal compulsory treatment in the community. Evaluation of a wide range of outcomes should be considered when this legislation is introduced. However, conclusions are based on three relatively small trials, with high or unclear risk of blinding bias, and evidence we rated as low to medium quality.

PLAIN LANGUAGE SUMMARY

Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Compulsory community treatment (CCT) for people with severe mental health problems is used in many countries, including Australia, Israel, New Zealand, the United Kingdom, and the United States. Supporters of this approach suggest that it is less restrictive and better to compulsorily treat someone in the community than to subject them to repeated hospital admissions. They also argue that it is effective in bringing stability to the lives of people with severe mental illness. Opponents of CCT fear treatment and support will be replaced by a greater emphasis on control, restraint, and threat. CCT may also undermine the relationship between healthcare professionals and patients, leading to feelings of mistrust and being controlled, which may drive people with severe mental illnesses away from services.

Given the widespread use of such powers, which effectively force people in the community to compulsorily undergo treatment, it is important to assess the benefits, effectiveness or possible hazards of compulsory treatment.

Update searches for randomised trials were run in 2012 and 2013 and this review now includes three trials with a total of 752 people. Two of these trials compared a form of CCT called 'Outpatient Commitment' (OPC) versus standard care and the third trial compared a form of CCT called Community Treatment Order to supervised discharge. The review authors rated the quality of evidence for the main outcomes to be low to medium grade. Results from the trials showed overall CCT was no more likely to result in better service use, social functioning, mental state or quality of life compared with standard 'voluntary' care. People receiving CCT were less likely to be victims of violent or non-violent crime. It is unclear whether this benefit is due to the intensity of treatment or its compulsory nature. Other than feelings of coercion or being controlled, there were no other negative outcomes. Short periods of conditional leave may be as effective (or non-effective) as compulsory treatment in the community. However, there is very limited information available, all results are based on three relatively small trials of low to medium quality, making it difficult to draw firm conclusions, so further research into the effects of different types of compulsory community treatment is much needed.

Ben Gray, Senior Peer Researcher, McPin Foundation http://mcpin.org/.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON [Explanation]

COMPULSORY COMMUNITY TREATMENT compared with STANDARD CARE for people with severe mental disorders					
Patient or population: people with severe mental disorders **Settings:** patients in community settings **Intervention:** COMPULSORY COMMUNITY TREATMENT **Comparison:** STANDARD CARE					
Outcomes	**Illustrative comparative risks* (95% CI)**		**Relative effect (95% CI)**	**No of Participants (studies)**	**Quality of the evidence (GRADE) Comments**
	Assumed risk	**Corresponding risk**			
	STANDARD CARE	**COMPULSORY COMMUNITY TREATMENT**			
Health service outcomes: 1. Readmission to hospital - by 11-12 months	Study population		RR 0.98 (0.79 to 1.21)	416 (2 studies)	⊕⊕◯◯ **low**[1,2]
	460 per 1000	451 per 1000 (363 to 557)			
	Medium risk population				
	446 per 1000	437 per 1000 (352 to 540)			
Health service outcomes: 2. Compliance with medication - by 11-12 months	Study population		RR 0.99 (0.83 to 1.19)	416 (2 studies)	⊕⊕◯◯ **low**[1,2]
	505 per 1000	500 per 1000 (419 to 601)			
	Medium risk population				
	554 per 1000	548 per 1000 (460 to 659)			

Patient level outcomes: 1a. Social functioning: trouble with police - by 11-12 months - at least one arrest	Study population		RR 0.97 (0.62 to 1.52)	416 (2 studies)	⊕⊕○○ low[1,2]
	158 per 1000	153 per 1000 (98 to 240)			
	Medium risk population				
	156 per 1000	151 per 1000 (97 to 237)			
Patient level outcomes: 1a. Social functioning: trouble with police - by 11-12 months - ever arrested/ picked up by police for violence against a person	Study population		RR 0.82 (0.56 to 1.21)	416 (2 studies)	⊕⊕○○ low[1,2]
	208 per 1000	171 per 1000 (116 to 252)			
	Medium risk population				
	156 per 1000	128 per 1000 (87 to 189)			
Patient level outcomes: 1b. Social functioning: homeless - by 11-12 months	Study population		RR 0.67 (0.39 to 1.15)	416 (2 studies)	⊕⊕○○ low[1,2]
	134 per 1000	90 per 1000 (52 to 154)			
	Medium risk population				
	145 per 1000	97 per 1000 (57 to 167)			
Patient level outcomes: 2. Quality of life: victimisation - by 11-12 months	Study population		RR 0.5 (0.31 to 0.8)	264 (1 study)	⊕⊕○○ low[1,3]
	311 per 1000	156 per 1000 (96 to 249)			
	Medium risk population				

			RR / effect	No of participants	Quality
	311 per 1000	**156 per 1000** (96 to 249)			
Patient level outcomes: 3. Satisfaction with care/adverse events: perceived coercion - by 11-12 months	**Study population**		**RR 1.36** (0.97 to 1.89)	416 (2 studies)	⊕⊕◯◯ **low**[1,2]
	218 per 1000	**296 per 1000** (211 to 412)			
	Medium risk population				
	227 per 1000	**309 per 1000** (220 to 429)			

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

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Allocation concealment and blinding unclear
[2] Only 2 studies, both from the United States of America
[3] Only 1 study

B A C K G R O U N D

Description of the condition

Compulsory treatment for people with severe mental disorders in the community is used in many countries, including Australia, Israel, New Zealand, the United Kingdom (UK), and the United States of America (USA) (Wilk 1988; Kanter 1995; Torrey 1995; McIvor 1998; McIvor 2001). In the USA, the majority of states have some form of compulsory community treatment (Torrey 1995) and there are similar provisions in Australia, Canada and New Zealand (Dedman 1990; Mulvany 1993; Torrey 1995; Gray 2001; O'Reilly 2001). Early initiatives in the UK included extended leave for patients leaving hospital and a 'supervision register' (Sensky 1991; Holloway 1996), with the more recent implementation of legislation for compulsory community treatment in Scotland, England, and Wales (Lawton-Smith 2008; Wooley 2010).

Supporters of this approach suggest that it is less restrictive to compulsorily treat someone in the community than to subject them to repeated hospital admissions (Pinfold 2001). They also argue that it is effective in bringing stability to the lives of people with severe mental illness (O'Reilly 2001). Opponents of compulsory community treatment fear treatment and support will be replaced by a greater emphasis on control, restraint, and threat (Pinfold 2001). They argue that compulsion may be used as an alternative to intensive case management or assertive community treatment, which may be all that is needed (Swartz 1995). Compulsory community treatment may also adversely effect the therapeutic alliance between healthcare professionals and patients and drive people with severe mental illnesses away from services (Pinfold 2001), although the limited data to date do not suggest that this has happened (O'Reilly 2001).

Description of the intervention

Extended leave or supervised discharge is applied at the time of discharge from compulsory inpatient treatment. These methods are used in Canada (Gray 2001), the UK (Sensky 1991), and New Hampshire, USA (Torrey 1995). They give mental health professionals the right to return a patient to hospital against their wishes if they do not comply with treatment.

Community treatment orders (CTOs) are used in Australia (Vaughan 2000) and Canada (Gray 2001) and give mental health professionals the right to place an individual on an order, whether they are in hospital or not. This is in contrast to extended leave or supervised discharge, which only applies to patients who are being discharged from inpatient care (Gray 2001). Community treatment orders are designed to divert people from possibly having to be admitted as inpatients. In addition, unlike leave, the individual

may not have to meet the same criteria for treatment as an inpatient (Gray 2001). Involuntary outpatient treatment or commitment is the preferred term in the USA and covers court-ordered community treatment (O'Reilly 2001). In this case, a judge, not a healthcare professional, decides on the appropriateness of the order.

The range of different interventions and ways of reporting frequency of use make it difficult to estimate how often compulsory community treatment is used. The situation is complicated by the fact that in some jurisdictions, different forms of community treatment such as extended release and involuntary outpatient treatment exist in parallel. Use varies widely across jurisdictions with the highest rates being recorded in the Australian state of Victoria with a prevalence of (98.8 per 100,000). By contrast, in the USA, involuntary outpatient treatment was only used in approximately three per 100,000 of the general population, 9.8% of new outpatient admissions and 7.1% of continuing outpatients (Ridgely 2001). However, even within the USA, the use of involuntary outpatient treatment varied. For instance, survey data from respondents in 13 states and the District of Columbia indicated they used it commonly or very commonly, while in a further 21 states, use was rare or very rare. Some of this variation may be explained by the use of alternative provisions such as extended release (Torrey 1995). Canadian studies indicate a prevalence of around 5 per 100,000 of the general population (O'Reilly 2000).

How the intervention might work

Studies indicating limited but improved outcomes in terms of readmission to hospital, length of stay, and adherence to treatment have often not controlled for selection bias, variations in treatment, and differing criteria for compulsory treatment in the community (McIvor 1998). In South Carolina, the duration of psychosis was an important determining factor for compulsory treatment in the community (Schied-Cook 1987). In England and Wales, extended leave has been used as a proxy for compulsory treatment in the community and researchers have identified both recent dangerousness and non-adherence as determining factors for being placed on this provision (Sensky 1991). Community treatment orders in New South Wales are mostly used for unmarried men with schizophrenia (Vaughan 2000). Involuntary outpatient treatment in many American states does not include the power to give medication forcibly in a community setting, but CTOs in Australasia do. In addition, studies often do not include a control group to take into account the possibility that participants were recruited when particularly disturbed and that subsequent reductions in hospital use may be due to other factors. In one study with a control group of patients not subject to a compulsory treatment order, the control group showed a similar reduction in time in hospital (Bursten 1986).

In England and Wales the extended leave provision of the Mental Health Act has been evaluated as a proxy for the community

treatment order, although it does not cover compulsory treatment in the community. One group of researchers found that extended leave improved adherence, reduced time spent in hospital, and reduced levels of dangerousness (Sensky 1991). The introduction of supervised discharge meant that a patient could be conveyed to a designated location for medical treatment, occupation, or training but was still not obliged to accept treatment; this legislative measure has never been formally evaluated.

Why it is important to do this review

Even when studies have used controls, it is difficult to know whether to attribute any improvement to compulsory community treatment, or to the non-specific effects of increased contact with healthcare professionals (Swartz 1995; Torrey 1995; Geller 1998; Swartz 1999). A research group found that although patients who received prolonged involuntary community treatment had reduced hospital readmissions and bed-days, it was difficult to separate out how much of the improvement was due to compulsory treatment and how much to intensive community management (Swartz 1999). In the case of non-randomised designs, a further difficulty is ensuring that the control group is as severely ill as the group placed on a community treatment order (Vaughan 2000).

In summary, it remains unclear whether compulsory community treatment can improve patient outcome or reduce health service use. Given the widespread use of such powers in Australasia, Israel, North America, and the UK, it is important to assess the benefit and potential harms of this type of legislation.

OBJECTIVES

1. To compare compulsory community treatment of any form with standard voluntary care.

2. To compare different types of compulsory treatment (see Types of Compulsory community treatment 1.1 in Types of interventions). We did not pool data from different studies unless the types of compulsory community treatment in both arms of trials closely resembled each other.

METHODS

Criteria for considering studies for this review

Types of studies

We considered all relevant randomised controlled trials (RCTs), and conducted economic evaluations alongside the included RCTs. We excluded quasi-randomised studies, such as those allocating by using alternate days of the week. Where trials were described in some way as to suggest or imply that the study was randomised and where the demographic details of each group's participants were similar, we included trials and undertook Sensitivity analysis to the presence or absence of these data.

Types of participants

We included trials of adults with severe mental illnesses (mainly schizophrenia and schizophrenia-like disorders, bipolar disorder, or depression with psychotic features), however diagnosed, who were managed in a community setting. Substance abuse was not considered to be a severe mental disorder in its own right. However, studies were eligible if they dealt with people with both diagnoses, i.e. people with severe mental illness plus substance abuse.

Types of interventions

1. Comparison I: Compulsory community treatment versus standard care

1.1 Compulsory community treatment

For an intervention to be accepted as compulsory community treatment, it had to be described in the trial using the following terms: community treatment order, involuntary outpatient treatment, involuntary outpatient commitment, supervised community treatment, extended leave, extended release or supervised discharge.

1.2 Standard care

The care that a person would normally receive had they not been included in the research trial, as long as it did not involve compulsory community treatment in any form.

2. Comparison II: Community treatment orders (CTOs) versus supervised discharge

Two different types of compulsory treatment, CTOs and supervised discharge (see Types of interventions 1.1). We did not pool data from different studies unless the compulsory treatments in both arms of the trials closely resembled each other.

Types of outcome measures

We did not plan to report highly specific outcomes (such as, for example, 'sense of safety') because multiple testing of sub-components of outcome scales carries a risk of type I errors (finding a

difference when none was present). Outcomes relating to the process of the interventions themselves, such as number of outpatient visits, were not reported (Wagner 2003). We did not consider loss to follow-up for study purposes to be the same as loss to follow-up to clinical services, as consent to treatment is not necessarily the same as consent to participate in a study.

In the original protocol for this study we stated we would group outcomes into short term (within 12 weeks of the start of therapy), medium term (between 13 to 24 weeks after the beginning of therapy), and long term (more than 24 weeks after the start of therapy). Only the Swartz 1999 study reported results of intermediate periods of follow-up, and these were not from their RCT. We have therefore only been able to report outcomes at 11 to 12 months of follow-up.

Primary outcomes

1. Health service contact and utilisation

1.1 Admission to hospital
1.2 Bed-days spent in hospital

2. Social functioning

2.1 Specific - imprisonment, police contact, and arrests

Secondary outcomes

1. Health service contact and utilisation

1.1 Remaining in contact with psychiatric services - leaving the study early

2. Social functioning

2.1 General
2.2 Specific - employment
2.3 Specific - accommodation status

3. Mental state

3.1 General
3.2 Specific - psychopathology

4. Global state

4.1 General
4.2 Specific

5. Quality of life

5.1 General
5.2 Self-esteem

6. Satisfaction

6.1 Number of needs for care
6.2 Patient satisfaction
6.3 Carer satisfaction
6.4 Perceived coercion

7. Adverse events

7.1 Mortality

8. Economic outcomes

9. 'Summary of findings' table

We used the GRADE approach to interpret findings (Schünemann 2008) and used the GRADE profiler to import data from Review Manager (Revman) to create 'Summary of findings' tables. These tables provide outcome-specific information concerning the overall quality of evidence from each included study in the comparison, the magnitude of effect of the interventions examined, and the sum of available data on all outcomes we rated as important to patient-care and decision making. We have been able to add more outcomes to the table and selected the following main outcomes for inclusion in the 'Summary of findings' table.

9.1 Health service and utilisation

9.1.1 Admission/readmission to hospital
9.1.2 Bed-days in hospital
9.1.3 Compliance with medication
9.1.4 Number with multiple readmissions by 12 months
9.1.4 Days in community before first admission

9.2 Patient level outcomes

9.2.1 Social functioning: trouble with police, homeless
9.2.2 Quality of life: victimisation
9.2.3 Satisfaction with care; perceived coercion
9.2.4 Mental state: Brief Psychiatric Rating Scale
9.2.5 Global state: Global Assessmant of Functioning

Search methods for identification of studies

Electronic searches

1. Cochrane Schizophrenia Group's Trials Register

The Trials Search Coordinator (TSC) searched the Cochrane Schizophrenia Group's Registry of Trials (2012 and 8 November 2013) using the following search strategies:
((*treatment* and *order*) or (*involuntar* and *outpatient*) or (*extend* and *leave*) or (*supervis* and *discharg*) or (*compulsor* or *compulsion*)) in Title, Abstract and Keyword Fields of REFERENCE and (Involuntary* or Outpatient*) in Intervention Field of STUDY

The Cochrane Schizophrenia Group's Registry of Trials is compiled by systematic searches of major resources (including AMED, BIOSIS, CINAHL, EMBASE, MEDLINE, PsycINFO, PubMed, and registries of clinical trials) and their monthly updates, handsearches, grey literature, and conference proceedings (see Group Module). There is no language, date, document type, or publication status limitations for inclusion of records into the register. For previous searches, see Appendix 1.

Searching other resources

1. Reference searching

We also inspected the references of all identified studies (including those rejected from the review) for more studies.

2. Personal contact

We contacted the first author of each included study and known experts who had published reviews in the field for information regarding unpublished trials and extra data on the published trials.

Data collection and analysis

For previous data collection and analysis please see Appendix 2.

Selection of studies

For this 2013 update review author SK independently inspected citations from the new electronic search and identified relevant abstracts. SK also inspected full articles of the abstracts meeting inclusion criteria. SK carried out the reliability check of all citations from the new electronic search.

Data extraction and management

1. Extraction

We extracted data presented only in graphs and figures whenever possible. When further information was necessary, we contacted authors of studies in order to obtain missing data or for clarification. If studies were multi-centre, where possible, we extracted data relevant to each component centre separately.

2. Management

2.1 Forms

We extracted data onto standard, simple forms.

2.2 Scale-derived data

We included continuous data from rating scales only if:
a. the psychometric properties of the measuring instrument have been described in a peer-reviewed journal (Marshall 2000); and
b. the measuring instrument has not been written or modified by one of the trialists for that particular trial.

Ideally, the measuring instrument should either be i. a self-report or ii. completed by an independent rater or relative (not the therapist). We realise that this is not often reported clearly; we have noted whether or not this is the case in Description of studies.

2.3 Endpoint versus change data

There are advantages of both endpoint and change data. Change data can remove a component of between-person variability from the analysis. On the other hand, calculation of change needs two assessments (baseline and endpoint), which can be difficult in unstable and difficult to measure conditions such as schizophrenia. We decided primarily to use endpoint data, and only use change data if the former were not available. We combined endpoint and change data in the analysis as we used mean differences (MD) rather than standardised mean differences (SMD) throughout (Higgins 2011, Chapter 9.4.5.2).

2.4 Skewed data

Continuous data on clinical and social outcomes are often not normally distributed. To avoid the pitfall of applying parametric tests to non-parametric data, we aimed to apply the following standards to all data before inclusion:
a) standard deviations (SDs) and means are reported in the paper or obtainable from the authors;
b) when a scale starts from the finite number zero, the SD, when multiplied by two, is less than the mean (as otherwise the mean is unlikely to be an appropriate measure of the centre of the distribution (Altman 1996));
c) if a scale started from a positive value (such as the Positive and Negative Syndrome Scale (PANSS, Kay 1986), which can have values from 30 to 210), we planned to modify the calculation described above to take the scale starting point into account. In these cases skew is present if 2 SD > (S-S min), where S is the mean score and S min is the minimum score.

Endpoint scores on scales often have a finite start and end point and these rules can be applied. We planned to enter skewed endpoint data from studies of fewer than 200 participants in additional tables rather than into an analysis. Skewed data pose less of a

problem when looking at mean if the sample size is large; we entered such endpoint data into syntheses.

When continuous data are presented on a scale that includes a possibility of negative values (such as change data), it is difficult to tell whether data are skewed or not. We therefore entered skewed change data into analyses regardless of size of study.

2.5 Common measure

To facilitate comparison between trials, we intended to convert variables that can be reported in different metrics, such as days in hospital (mean days per year, per week or per month) to a common metric (e.g. mean days per month).

2.6 Conversion of continuous to binary

Where possible, we made efforts to convert outcome measures to dichotomous data. This can be done by identifying cut-off points on rating scales and dividing participants accordingly into 'clinically improved' or 'not clinically improved'. It is generally assumed that if there is a 50% reduction in a scale-derived score such as the Brief Psychiatric Rating Scale (BPRS, Overall 1962) or the Positive and Negative Syndrome Scale (PANSS, Kay 1986), this could be considered as a clinically significant response (Leucht 2005; Leucht 2005a). If data based on these thresholds were not available, we used the primary cut-off presented by the original authors.

2.7 Direction of graphs

Where possible, we entered data in such a way that the area to the left of the line of no effect indicated a favourable outcome for compulsory community and involuntary outpatient treatment. Where keeping to this made it impossible to avoid outcome titles with clumsy double-negatives (e.g. 'Not improved'), we reported data where the left of the line indicates an unfavourable outcome. This was noted in the relevant graphs.

Assessment of risk of bias in included studies

Review author SK worked independently by using criteria described in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011) to assess trial quality. This new set of criteria is based on evidence of associations between overestimate of effect and high risk of bias of the article such as sequence generation, allocation concealment, blinding, incomplete outcome data and selective reporting.

Where inadequate details of randomisation and other characteristics of trials were provided, we contacted authors of the studies in order to obtain additional information.

We have noted the level of risk of bias in both the text of the review and in the Summary of findings for the main comparison.

Measures of treatment effect

1. Binary data

For binary outcomes we calculated a standard estimation of the risk ratio (RR) and its 95% confidence interval (CI). It has been shown that RR is more intuitive (Boissel 1999) than odds ratios and that odds ratios tend to be interpreted as RR by clinicians (Deeks 2000). The Number Needed to Treat/Harm (NNT/H) statistic is intuitively attractive to clinicians but needs to be interpreted with caution (Hutton 2009). For binary data presented in the 'Summary of findings' table/s, where possible, we calculated illustrative comparative risks.

2. Continuous data

For continuous outcomes we estimated mean difference (MD) between groups. We preferred not to calculate effect size measures (standardised mean difference (SMD)). However, if scales of very considerable similarity were used, we presumed there was a small difference in measurement, calculated effect size, and transformed the effect back to the units of one or more of the specific instruments.

Unit of analysis issues

1. Cluster trials

Studies increasingly employ 'cluster randomisation' (such as randomisation by clinician or practice), but analysis and pooling of clustered data poses problems. Authors often fail to account for intra-class correlation in clustered studies, leading to a 'unit of analysis' error (Divine 1992) whereby P values are spuriously low, confidence intervals unduly narrow and statistical significance overestimated. This causes type I errors (Bland 1997; Gulliford 1999). Cluster trials were eligible for inclusion, however, the three studies identified in our review were randomised by participant, not by clinician or practice. Had we included cluster trials, where clustering was not accounted for in primary studies, we planned to present data in a table, with a (*) symbol to indicate the presence of a probable unit of analysis error. In subsequent versions of this review, and if relevant, we will seek to contact first authors of studies to obtain intra-class correlation coefficients (ICCs) for their clustered data and to adjust for this by using accepted methods (Gulliford 1999). Where clustering has been incorporated into the analysis of primary studies, we will present these data as if from a non-cluster randomised study, but adjust for the clustering effect. We have sought statistical advice and have been advised that if the binary data are presented in a report, they should be divided by a 'design effect'. This is calculated using the mean number of participants per cluster (m) and the ICC [Design effect = 1+(m-1)*ICC] (Donner 2002). If the ICC is not reported it will be assumed to be 0.1 (Ukoumunne 1999).

If cluster studies are appropriately analysed taking into account ICCs and relevant data documented in the report, data can be synthesised with other studies using the generic inverse variance technique.

2. Cross-over trials

A major concern of cross-over trials is the carry-over effect. It occurs if an effect (e.g. pharmacological, physiological or psychological) of the treatment in the first phase is carried over to the second phase. As a consequence, on entry to the second phase the participants can differ systematically from their initial state despite a wash-out phase. For the same reason cross-over trials are not appropriate if the condition of interest is unstable (Elbourne 2002). As both effects are very likely in severe mental illness, had we included cross-over trials, we planned to use only the data of the first phase of any cross-over studies.

3. Studies with multiple treatment groups

If we had included any studies that involved more than two treatment arms, if relevant, we planned to present the additional treatment arms in comparisons. If data were binary, we would simply have added these and combined them within the two-by-two table. If data were continuous, we would have combined data following the formula in section 7.7.3.8 (Combining groups) of the *Cochrane Handbook for Systemic reviews of Interventions* (Higgins 2011). Where additional treatment arms were not relevant, we would not have presented these data.

Dealing with missing data

1. Overall loss of credibility

At some degree of loss of follow-up, data must lose credibility (Xia 2009). We chose that, for any particular outcome, should more than 50% of data be unaccounted for, we would not reproduce these data or use them within analyses. If, however, more than 50% of those in one arm of a study were lost, but the total loss was less than 50%, we addressed this within the 'Summary of findings' table/s by down-rating quality. Finally, we also downgraded quality within the 'Summary of findings' table/s should loss be 25% to 50% in total.

2. Binary

In the case where attrition for a binary outcome was between 0% and 50% and where these data were not clearly described, we presented data on a 'once-randomised-always-analyse' basis (an intention-to-treat analysis). Those leaving the study early are all assumed to have the same rates of negative outcome as those who completed, with the exception of the outcome of death and adverse effects. For these outcomes, the rate of those who stay in the study - in that particular arm of the trial - were used for those who did not. We undertook a sensitivity analysis to test how prone the primary outcomes were to change when data only from people who completed the study to that point were compared to the intention-to-treat analysis using the above assumptions.

3. Continuous

3.1 Attrition

In the case where attrition for a continuous outcome was between 0% and 50%, and data only from people who completed the study to that point were reported, we presented and used these data.

3.2 Standard deviations

If standard deviations (SDs) were not reported, we first tried to obtain the missing values from the authors. If not available, where there were missing measures of variance for continuous data, but an exact standard error (SE) and confidence intervals available for group means, and either a P value or T value available for differences in mean, we calculated them according to the rules described in the *Cochrane Handbook for Systemic reviews of Interventions* (Higgins 2011): When only the standard error (SE) was reported, SDs were calculated by the formula SD = SE * square root (n). Chapters 7.7.3 and 16.1.3 of the *Cochrane Handbook for Systemic reviews of Interventions* (Higgins 2011) present detailed formulae for estimating SDs from P values, T or F values, confidence intervals, ranges or other statistics. If these formulae did not apply, we calculated the SDs according to a validated imputation method which is based on the SDs of the other included studies (Furukawa 2006). Although some of these imputation strategies can introduce error, the alternative was to exclude a given study's outcome and thus to lose information. We nevertheless examined the validity of the imputations in a sensitivity analysis excluding imputed values.

3.3 Last observation carried forward

We anticipated that in some studies the method of last observation carried forward (LOCF) would be employed within the study report. As with all methods of imputation to deal with missing data, LOCF introduces uncertainty about the reliability of the results (Leucht 2007). Therefore, we intended where LOCF data were used in the trial, if less than 50% of the data have been assumed, we would reproduce these data and indicate that they were the product of LOCF assumptions.

Assessment of heterogeneity

1. Clinical heterogeneity

We considered all included studies initially, without seeing comparison data, to judge clinical heterogeneity. We simply inspected all studies for clearly outlying people or situations which we had not predicted would arise. When such situations or participant groups arose, we fully discussed these.

2. Methodological heterogeneity

We considered all included studies initially, without seeing comparison data, to judge methodological heterogeneity. We simply inspected all studies for clearly outlying methods which we had not predicted would arise. When such methodological outliers arose, we fully discussed these.

3. Statistical heterogeneity

3.1 Visual inspection

We visually inspected graphs to investigate the possibility of statistical heterogeneity.

3.2 Employing the I^2 statistic

We investigated heterogeneity between studies by considering the I^2 method alongside the Chi^2 P value. The I^2 provides an estimate of the percentage of inconsistency thought to be due to chance (Higgins 2003). The importance of the observed value of I^2 depends on i. magnitude and direction of effects and ii. strength of evidence for heterogeneity (e.g. P value from Chi^2 test, or a confidence interval for I^2). An I^2 estimate greater than or equal to around 50% accompanied by a statistically significant Chi^2 statistic was interpreted as evidence of substantial levels of heterogeneity (Higgins 2011). When substantial levels of heterogeneity were found in the primary outcome, we explored reasons for heterogeneity (Subgroup analysis and investigation of heterogeneity).

Assessment of reporting biases

Reporting biases arise when the dissemination of research findings is influenced by the nature and direction of results (Egger 1997). These are described in Section 10 of the *Handbook* (Higgins 2011). We are aware that funnel plots may be useful in investigating reporting biases but are of limited power to detect small-study effects. Only three trials were included in this version of the review. We planned to use funnel plots for outcomes where there were 10 or more studies, or where all studies were of similar sizes. In other cases, where funnel plots are possible, we would have sought statistical advice in their interpretation.

Data synthesis

We understand that there is no closed argument for preference for use of fixed-effect or random-effects models. The random-effects method incorporates an assumption that the different studies are estimating different, yet related, intervention effects. This often seems to be true to us and the random-effects model takes into account differences between studies even if there is no statistically significant heterogeneity. There is, however, a disadvantage to the random-effects model: it puts added weight onto small studies which often are the most biased ones. Depending on the direction of effect, these studies can either inflate or deflate the effect size. We chose the fixed-effect model for all analyses given the similarity of patients and interventions in each comparison. The reader is, however, able to choose to inspect the data using the random-effects model.

Subgroup analysis and investigation of heterogeneity

1. Subgroup analyses - only primary outcomes

1.1 Clinical state, stage or problem

We had hoped to investigate subgroups including different variations of types of intervention (e.g. CTOs, involuntary outpatient treatment, involuntary outpatient commitment (OPC) or supervised discharge). Because there were never more than two studies for each outcome, we could not undertake such sensitivity analyses as we had hoped.

2. Investigation of heterogeneity

If inconsistency was high, we have reported this. First, we investigated whether data had been entered correctly. Second, if data were correct, we visually inspected the graph and successively removed outlying studies to see if homogeneity was restored. For this review we had decided that should this occur with data contributing to the summary finding of no more than around 10% of the total weighting, we would present data. If not, then we would not pool data but would discuss issues. We know of no supporting research for this 10% cut-off, but we used prediction intervals as an alternative to this unsatisfactory state.

Sensitivity analysis

We applied all sensitivity analyses to the primary outcomes of this review.

1. Implication of randomisation

We aimed to include trials in a sensitivity analysis if they were described in some way so as to imply randomisation. For the primary

outcomes we included these studies and if there was no substantive difference when the implied randomised studies were added to those with a better description of randomisation, we entered all data from these studies.

2. Assumptions for lost binary data

Where assumptions had to be made regarding people lost to follow-up (see Dealing with missing data), we compared the findings of the primary outcomes when we used our assumption/s and when we used data only from people who completed the study to that point. If there was a substantial difference, we reported results and discussed them but continued to employ our assumption.

Where assumptions had to be made regarding missing SDs data (see Dealing with missing data), we compared the findings of the primary outcomes when we used our assumption/s and when we used data only from people who completed the study to that point. A sensitivity analysis was undertaken to test how prone results are to change when completer-only data only are compared to the imputed data using the above assumption. If there was a substantial difference, we reported results and discussed them but continued to employ our assumption

3. Risk of bias

We analysed the effects of excluding trials that were judged to be at high risk of bias across one or more of the domains of randomisation (implied as randomised with no further details available): allocation concealment, blinding and outcome reporting for the meta-analysis of the primary outcome. If the exclusion of trials at high risk of bias did not substantially alter the direction of effect or the precision of the effect estimates, then we included data from these trials in the analysis.

4. Imputed values

We also planned to undertake a sensitivity analysis to assess the effects of including data from trials where we used imputed values for ICC in calculating the design effect in cluster randomised trials but this was not required for the current version of the review. If we noted substantial differences in the direction or precision of effect estimates in any of the sensitivity analyses listed above, we did not pool data from the excluded trials with the other trials contributing to the outcome, but presented them separately.

R E S U L T S

Description of studies

Also see Characteristics of included studies and Characteristics of excluded studies

Results of the search

Figure 1 gives details of our search results. We found 74 papers that were potentially relevant in our original search. Later searches identified six further publications, Of these, we excluded 65 records because they did not meet our inclusion criteria, lacked relevant data, or were evaluating different types of treatment such as joint crisis plans. This left 15 publications covering three studies (Figure 1).

Figure 1.   Study flow diagram of trial selection.
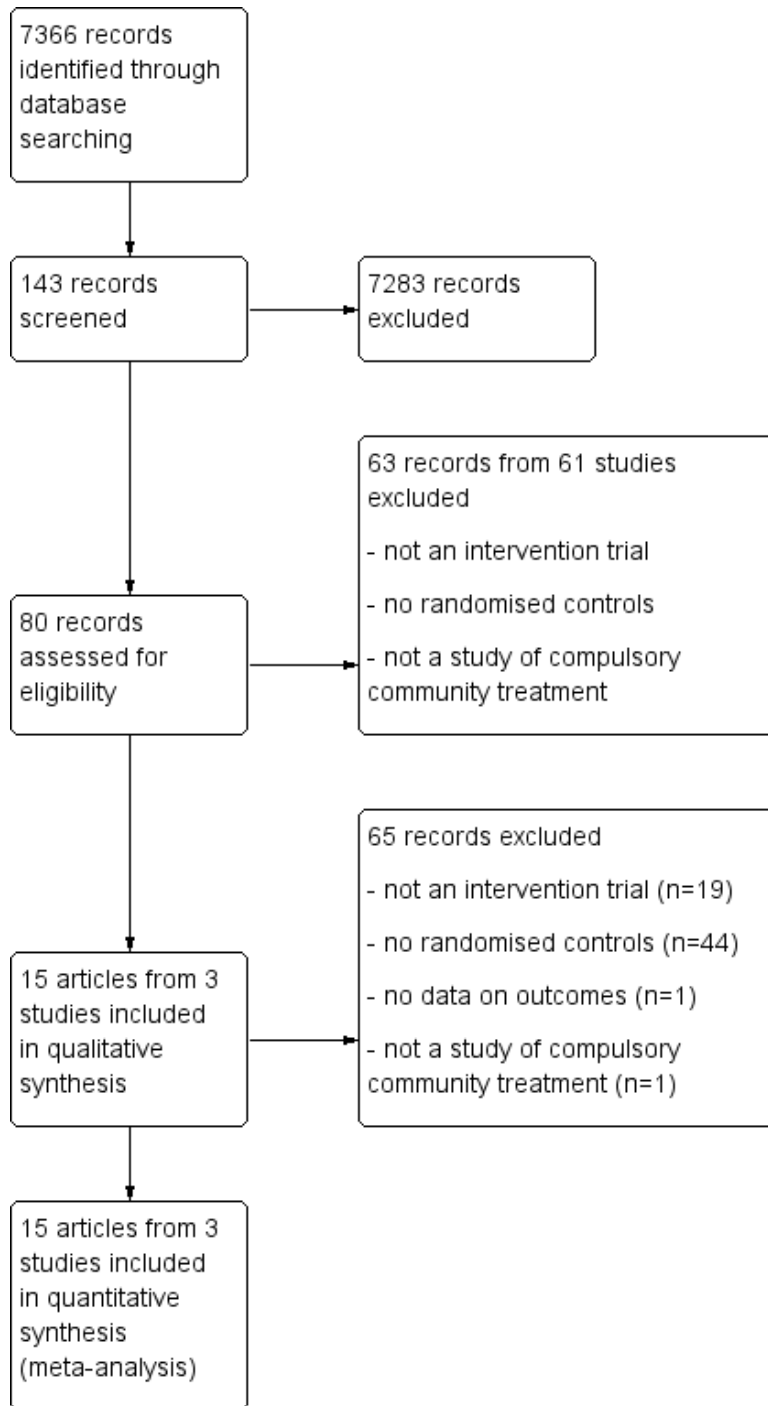
Included studies

We included three studies involving 752 participants: two studies from the USA comparing outpatient commitment (OPC) with entirely voluntary treatment and one UK study, the Oxford Community Treatment Order Evaluation Trial (OCTET) comparing two types of compulsory community treatment interspaced with voluntary care. OCTET randomised patients discharged from hospital to an experimental group (community treatment order (CTO)) or a control group (extended leave under section 17 of the Mental Health Act) and compared their outcomes at 12 months (Burns 2013). "Leave of absence" under Section 17 of the Act allows patients to leave hospital for some hours or days, or even exceptionally weeks, while still subject to recall.

Excluded studies

We excluded 65 publications. All but two were excluded as they were not RCTs, did not contain primary data, or were reviews of intervention studies. Of the two excluded RCTs, one trial was excluded (Wagner 2003) as it reported outcomes inherent to the process of OPC, namely the number of outpatient visits for medication review, counselling and case management. The other was a study of joint crisis plans (Thornicroft 2013).

Risk of bias in included studies

Allocation

Steadman 2001, the New York study, did describe randomisation, using a random number list to identify assignment to either the intervention or control group. In this study, a random number list was generated by computer, which then split 200 numbers between one group and the other. The printed list was maintained in the research team's office in a locked file. When the treatment team had completed their treatment plan, they called the research team who checked the computer list to see whether the client was to be assigned to the experimental or comparison group. In the OCTET study, consenting participants were randomly assigned (ratio 1: 1) by an independent statistician to be discharged from hospital either on CTO or Section 17 leave (Burns 2013). Randomisation used random permuted blocks with lengths of two, four, and six, and stratified for sex (male or female), schizophrenic diagnosis (yes or no) and duration of illness (< 2 years or ≥ 2 years). Assignments were enclosed in sequentially numbered, opaque, sealed envelopes and stored by a researcher independent to the trial team. The details of the sequence remained unknown to all members of the trial team until recruitment, data collection, and analyses were completed. Randomisation took place after consent was obtained and the baseline interview was done. The envelope was opened on the day of the interview by the independent researcher after recording the participant's trial identification number on the envelope. She then communicated the randomised allocation to the recruiting researcher by telephone. Both studies were rated as being of low risk (Figure 2; Figure 3). Swartz 1999 was stated to be randomised but did not provide a description of the randomisation method and was therefore rated unclear (Figure 2; Figure 3).

Figure 2.   'Risk of bias' graph: review authors' judgements about each risk of bias item presented as percentages across all included studies
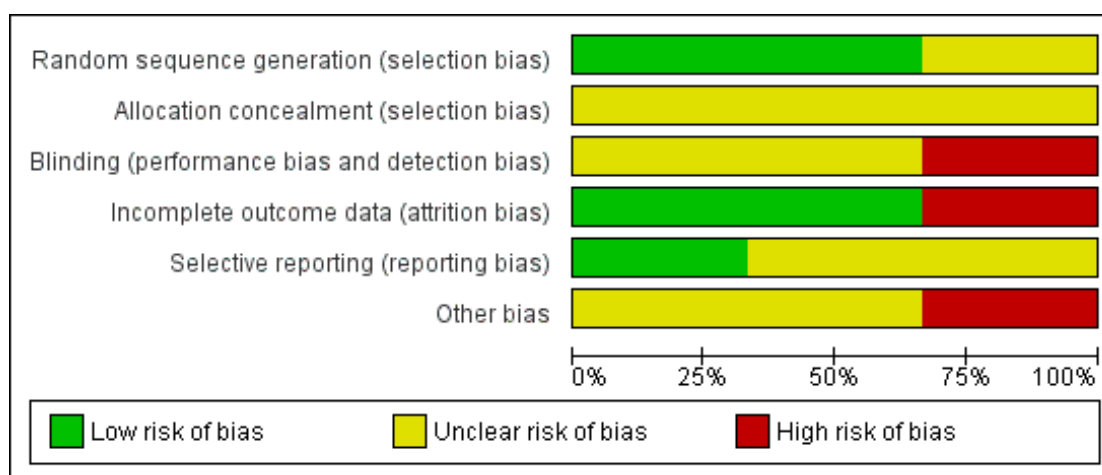
Figure 3. 'Risk of bias' summary: review authors' judgements about each risk of bias item for each included study

Blinding

All three studies used self-report measures for at least some of the outcomes, which are effectively self-blinding so although blindness was not mentioned in two of the studies (Steadman 2001; Swartz 1999), we rated risk of bias for blinding as unclear. In Burns 2013 the study involved allocation to two different types of legal status. It was therefore both impossible and unlawful to mask research assistants, treating clinicians or patients, thus we rated this as high

risk.

Incomplete outcome data

Attrition rates for Steadman 2001 were 45%. In the case of OCTET, follow-up for the main outcomes reached 100% although data on psychiatric symptoms and the Global Assessment of Functioning Scale (GAF) were only available on 70% of the sample. Reporting on numbers leaving the Swartz 1999 study early

was sometimes unclear.

Selective reporting

We could not use continuous data for several outcomes, as no variance was reported. In Swartz 1999 data from randomised and non-randomised analyses were sometimes amalgamated and reported percentages rather than absolute numbers of people who had been followed up. We contacted the authors who kindly confirmed that, at follow-up, there were 114 people in the control group, 102 in the court-ordered outpatient commitment (OPC) group and 46 who were not randomised. Similarly, the authors of the Steadman 2001 study kindly supplied the additional data for an intention-to-treat analysis of their paper. Our overall rating was 'unclear'.

Other potential sources of bias

No conflicts of interest were reported in any of the studies. A potential source of bias in the OCTET study was that participants were only included if they were equally suitable for a relatively short Section 17 or CTO. Patients who might have especially benefited from a CTO may therefore have been excluded. A further potential source of bias in the same study was allowing clinicians to make decisions independent of initial randomisation-whereby 40 participants (25%) allocated to Section 17 were subsequently placed on a CTO during the study and 35 patients randomised to CTOs (22%) did not actually receive the intervention. A sensitivity analysis to remove these protocol violations may, in turn, have left the study underpowered and not removed the possibility that Section 17 patients swapped to a CTO might have been more severely ill than those remaining on Section 17 as per the protocol. There is uncertainty concerning the control condition. Although the length of **initial** compulsory outpatient treatment differed widely between the two groups (medians of 183 versus eight days), Section 17 patients averaged three months on some form of compulsory treatment. On the other hand, an alternative interpretation of this finding is that the controls still spent the majority of the study in voluntary treatment. A final issue is generalisability. Around 20% of the participants were ineligible or refused to take part. These people may have been the most unwell or lacking in insight, and therefore, the ones most likely to benefit from CTOs.

Effects of interventions

See: **Summary of findings for the main comparison** COMPULSORY COMMUNITY TREATMENT compared with STANDARD CARE for people with severe mental disorders; **Summary of findings 2** COMMUNITY TREATMENT ORDERS compared with SECTION 17 for people with severe mental disorders

COMPARISON 1: COMPULSORY COMMUNITY TREATMENT versus STANDARD CARE

Results are presented as fixed-effect risk ratios (RR) with 95% confidence intervals (CIs) or fixed-effect mean difference (MD) with 95% CIs.

1.1 Health service outcomes: 1. Readmission to hospital - by 11-12 months

For this outcome we found two relevant trials (n = 416). There was no significant difference between compulsory community treatment and standard care (RR 0.98 CI 0.79 to 1.21, Analysis 1.1).

1.2 Health service outcomes: 2. Compliance with medication - by 11-12 months

For this outcome we found two relevant trials (n = 416). There was no significant difference between compulsory community treatment and standard care (RR 0.99 CI 0.83 to 1.19, Analysis 1.2).

1.3 Patient level outcomes: 1a. Social functioning: trouble with police - by 11-12 months

1.3.1 at least one arrest

In this subgroup we found two relevant trials (n = 416). There was no significant difference between compulsory community treatment and standard care (RR 0.97 CI 0.62 to 1.52, Analysis 1.3).

1.3.2 ever arrested/picked up by police for violence against a person

In this subgroup we found two relevant trials (n = 416). There was no significant difference between compulsory community treatment and standard care (RR 0.82 CI 0.56 to 1.21, Analysis 1.3).

1.4 Patient level outcomes: 1b. Social functioning: homeless - by 11-12 months

For this outcome we found two relevant trials (n = 416). There was no significant difference between compulsory community treatment and standard care (RR 0.67 CI 0.39 to 1.15, Analysis 1.4).

1.5 Patient level outcomes: 2. Quality of life: victimisation - by 11-12 months

For this outcome we only found one relevant trial (n = 264) (Swartz 1999). There was a statistically significant difference between compulsory community treatment and standard care (RR 0.50 CI 0.31 to 0.80, Analysis 1.5).

1.6 Patient level outcomes: 3. Satisfaction with care: perceived coercion - by 11-12 months

For this outcome we found two relevant trials (n = 416). There was no significant difference between compulsory community treatment and standard care (RR 1.36 CI 0.97 to 1.89, Analysis 1.6).

1.7 Economics

We identified no directly relevant costings.

1.8 Numbers needed to treat

In terms of numbers needed to treat (NNT), compared to placebo, it would take 85 OPC orders to prevent one readmission, 27 to prevent one episode of homelessness and 238 to prevent one arrest. The NNT for the reduction of victimisation was lower at six (CI 6 to 6.5).

COMPARISON 2: COMMUNITY TREATMENT ORDERS versus INTERMITTENT SUPERVISED DISCHARGE (SECTION 17)

2.1 Health service outcomes: 1. Readmission to hospital - by 12 months

For this outcome we only found one relevant trial (n = 333) (Burns 2013). There was no significant difference between community treatment orders and section 17 (RR 0.99 CI 0.74 to 1.32, Analysis 2.1). It was not possible to calculate the NNT/H as the proportions of readmissions in both groups were identical (36%).

2.2 Health service outcomes: 2.Hospital bed-days

For this outcome we only found one relevant trial (n = 333) (Burns 2013). There was no significant difference between community treatment orders and section 17 (MD -8.70 CI -30.88 to 13.48, Analysis 2.2).

2.3 Health service outcomes: 3. Number of readmissions by 12 months

For this outcome we only found one relevant trial (n = 119) (Burns 2013). There was a statistically significant difference between community treatment orders and section 17 (MD -0.20 CI -0.45 to 0.05, Analysis 2.3).

2.4 Health service outcomes: 4. Number with multiple readmissions by 12 months

For this outcome we only found one relevant trial (n = 333) (Burns 2013). There was no significant difference between community treatment orders and section 17 (RR 0.56 CI 0.27 to 1.17, Analysis 2.4).

2.5 Days in community till first admission

For this outcome we only found one relevant trial (n = 333) (Burns 2013). There was no significant difference between community treatment orders and section 17 (MD 5.00 CI -21.74 to 31.74, Analysis 2.5).

2.6 Patient level outcomes: Brief Psychiatric Rating Scale (BPRS)

For this outcome we only found one relevant trial (n = 234) (Burns 2013). There was no significant difference between community treatment orders and section 17 (MD -0.10 CI -3.17 to 2.97, Analysis 2.6).

2.7 Patient level outcomes: Global Assessment of Functioning Scale (GAF)

For this outcome we only found one relevant trial (n = 237) (Burns 2013). There was no significant difference between community treatment orders and section 17 (MD -0.70 CI -3.91 to 2.51, Analysis 2.7).

2.8 Economics

We identified no directly relevant costings.

2.9 Adverse events

There were no data on this outcome.

A D D I T I O N A L   S U M M A R Y   O F   F I N D I N G S   [Explanation]

COMMUNITY TREATMENT ORDERS compared with SECTION 17 for people with severe mental disorders

Patient or population: people with severe mental disorders
Settings: patients in community settings
Intervention: COMMUNITY TREATMENT ORDERS
Comparison: SECTION 17

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of Participants (studies)	Quality of the evidence (GRADE) Comments
	Assumed risk	**Corresponding risk**			
	SECTION 17	**COMMUNITY TREATMENT ORDERS**			
Health service outcomes: 1. Readmission to hospital - by 12 months	**Study population**		**RR 0.99** (0.74 to 1.32)	333 (1 study)	⊕⊕⊕◯ **moderate**[1,2]
	359 per 1000	**356 per 1000** (266 to 474)			
	Moderate risk population				
	359 per 1000	**355 per 1000** (266 to 474)			
Health service outcomes: 2. Total duration of psychiatric hospital stays over 12 months		The mean health service outcomes: 2. total duration of psychiatric hospital stays over 12 months in the intervention groups was **8.7 lower** (30.88 lower to 13.48 higher)		333 (1 study)	⊕⊕⊕◯ **moderate**[1,2]

Health service outcomes: 3. Number of readmissions by 12 months		The mean health service outcomes: 3. number of readmissions by 12 months in the intervention groups was **0.2 lower** (0.45 lower to 0.05 higher)		119 (1 study)	⊕⊕⊕◯ **moderate**[1,2]
Health service outcomes: 4. Number with multiple readmissions by 12 months	**Study population**		**RR 0.56** (0.27 to 1.17)	333 (1 study)	⊕⊕⊕◯ **moderate**[1,2]
	108 per 1000	60 per 1000 (29 to 126)			
	Moderate risk population				
	108 per 1000	60 per 1000 (29 to 126)			
Days in community till 1st admission		The mean days in community till 1st admission in the intervention groups was **5 higher** (21.74 lower to 31.74 higher)		333 (1 study)	⊕⊕⊕◯ **moderate**[1,2]
Patient level outcomes: BPRS		The mean patient level outcomes: BPRS in the intervention groups was **0.1 lower** (3.17 lower to 2.97 higher)		234 (1 study)	⊕⊕⊕◯ **moderate**[1,2]

Patient level out-comes: GAF [3]		The mean patient level outcomes: GAF in the intervention groups was **0.7 lower** (3.91 lower to 2.51 higher)	237 (1 study)	$\oplus\oplus\oplus\bigcirc$ **moderate**[1,2]

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

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Forty people (25%) allocated to Section 17 were subsequently placed on a CTO during the study
[2] 35 people randomised to CTOs (22%) did not actually receive the intervention
[3] No adverse events were reported

DISCUSSION

In spite of the widespread use of compulsory community treatment and the continued controversy as to its effectiveness, we were struck by the limited number of studies that have been conducted in this area. We have therefore attempted to draw modest conclusions, based on available evidence, and to highlight areas requiring further study, rather than draw firm conclusions that may not be based on evidence of high quality.

This review revealed little evidence for the effectiveness of compulsory community treatment in any of the main outcome indices: health service use, costs, social functioning, mental state, quality of life or satisfaction with care. We were only able to establish a statistically significant effect for one outcome, social functioning (victimisation).

Although we identified 15 papers, these represented only three trials. Two of these were of court-ordered 'Outpatient Commitment' (OPC) in the USA with limited generalisability to jurisdictions where compulsory community treatment is ordered by clinicians. The third trial, from the UK, was a study of clinician-initiated, rather than court-ordered treatment as in the American RCTs and thus, more relevant to other jurisdictions such as Australia, Canada and New Zealand. However, there is controversy as to whether this was a comparison of a compulsory intervention with voluntary treatment, or was a comparison of two different types of compulsory treatment. Problems common to all three studies included relatively small numbers of participants and questions concerning bias. This illustrates the difficult, but not impossible, task of using trial methods to study the effect of such legislation.

In the case of the North Carolina study (Swartz 1999), the study was supplemented by follow-up of an additional non-randomised group of patients with a recent history of violence who were also placed on OPC. It was sometimes difficult to separate the results of the randomised trials from the non-randomised study. In the case of the New York study (Steadman 2001), there was a relatively small number of participants and the suggestion that members of the control group and their case managers thought that they were actually on OPC (NASMHPD 2001). These factors would minimise any effect of the intervention. In addition to the question around the voluntary or compulsory nature of the controls in the OCTET study, the other major issue was that a quarter of patients did not receive the intervention to which they were initially randomised. This was because treating physicians were able to make clinical decisions irrespective of initial randomisation, and was necessary for the trial to be lawful.

Summary of main results

All three studies were of patients in community settings who were followed up over 12 months. Two trials from the USA were of court-ordered 'Outpatient Commitment' (OPC) compared with voluntary community treatment. The third compared clinician-initiated community treatment orders (CTOs) with another type of supervised discharge in England.

Comparison 1. Compulsory community treatment versus standard care

1.1 Health service outcomes

Only data for two health service outcomes (readmission to hospital and compliance with medication) were usable. In both cases there were no differences between groups. By one year, people were no more likely to be readmitted to hospital if they were placed on OPC than if they had received standard care. They were also just as likely to comply with medication. It should be noted, however, that these results are based on two studies only and in one (Steadman 2001) attrition rates were 45%, so no firm conclusions can be made.

1.2 Patient level outcomes

Four patient level outcomes (trouble with the police, homelessness, coercion, and victimisation) were presented.

By one year, the number of arrests by police were similar for both groups and people in the compulsory community treatment group did not commit any more acts of violence than those in standard care. A problem with the data in this area was a possibility of selection bias as patients with a history of violence were explicitly excluded from both trials. This limits their applicability as recent dangerousness, particularly violence against others, is often the reason for compulsory treatment in hospital or the community (Sensky 1991; Lansing 1997). There is also a risk of bias when outcome data are not assessed blind to group status and the results of people who were not randomised or post hoc analyses are included in papers. The number of people who were homeless by one year was also similar in both groups.

The only significant benefit from compulsory community treatment was for victimisation. People on compulsory community treatment were less likely to be victims of a violent or non-violent crime by one year in the North Carolina study (Swartz 1999).

In terms of possible adverse effects of compulsory community treatment, perceived coercion was higher in the North Carolina study (Swartz 1999), but not in the one from New York (Steadman 2001). Combining the findings from both studies in a meta-analysis gave a non-significant result.

2. Comparison II. Compulsory community treatment versus intermittent supervised discharge (Section 17)

1.1 Health service outcomes

There were no differences between patients on CTO and supervised discharge in terms of readmissions, bed-days or survival in the community.

1.2 Patient-level outcomes

Similarly there were no differences between the two groups in terms of psychiatric symptoms or social functioning as measured by standardised instruments.

Overall completeness and applicability of evidence

Two trials were of OPC in the USA. This is a court-ordered compulsory treatment plan and the findings may not be applicable to other jurisdictions where compulsory community treatment is initiated by clinicians. In addition, both trials explicitly excluded patients with a history of violence. Although understandable from an ethical and legal standpoint, this limits applicability, as recent dangerousness, particularly violence against others, is often the reason for compulsory treatment in hospital or the community. The OCTET study was a comparison of two types of clinician-ordered community treatment, CTOs and supervised discharge in England. This may be more applicable to other jurisdictions. However, around 20% of the sample were ineligible or refused to take part. These people may have been the most unwell or particularly lacking in insight, and therefore the ones most likely to benefit from CTOs

Quality of the evidence

This is a difficult area to research using randomised controlled trials (RCTs) and so all three studies contained a number of potential biases. OCTET met three out of the seven criteria of The Cochrane Collaboration's tool for assessing risk of bias, the others only one, the majority being rated unclear. There was no difference in these ratings by outcome. One issue in all three RCTS was selection and blindness bias and consequent applicability to everyday care. Depending on the study, patients with a history of dangerousness were excluded and around 20% of eligible patients lacked capacity to consent to the study, or refused to take part. This limits their applicability as recent dangerousness, particularly violence against others, is often the reason for compulsory treatment in hospital or the community (Sensky 1991; Lansing 1997). Only two of the three studies describe the process of randomisation in detail (Steadman 2001; Burns 2013) There is also a risk of bias when outcome data are not assessed blind to group status and the results of people who were not randomised or post hoc analyses are included in papers. All three studies used included intention-to-treat analyses for all (Swartz 1999; Burns 2013), or at least some of the outcomes of interest (Steadman 2001).

In the case of the North Carolina study (Swartz 1999), it was sometimes difficult to separate the results of the randomised trial from those of the follow-up of an additional non-randomised group of patients with a recent history of violence who were also placed on OPC. In the case of the New York study (Steadman 2001), there were a relatively small number of participants and the suggestion that members of the control group and their case managers thought that they were actually on OPC (NASMHPD 2001). These factors would minimise any effect of the intervention There were also fewer data on psychosocial outcomes as measured by standardised instruments.

All these issues illustrate the difficult, but not impossible, task of using trial methods to study the effect of such legislation.

Potential biases in the review process

1. Post hoc decisions

We changed the level of acceptable loss to follow-up. We felt that, in retrospect, the cut-off of 35% was too restricting and changed this to 50% (see above). We also added an additional comparison of two different types of compulsory community treatment. We took both decisions in the light of only finding three studies. We have tried to present data in a way as to allow the reader to evaluate the effects of doing this.

2. Authors' area of interest

The two authors of this review were also authors of some of the papers considered for inclusion (Preston 2002; Kisely 2004; Kisely 2005; Kisely 2006; Kisely 2007a). We excluded all of these papers. It is possible that we, through detailed knowledge of these studies excluded them, but for others, where our knowledge is not first hand, we allowed inclusion.

Agreements and disagreements with other studies or reviews

Our findings are consistent with two other independent reviews of compulsory community treatment, which have all found little evidence that compulsory community treatment have a positive effect on outcomes such as hospital admissions, length of stay or compliance with medication (Ridgely 2001; Churchill 2007).

A U T H O R S ' C O N C L U S I O N S

Implications for practice

1. For people with serious mental illnesses

Patients and carers should question the rationale for compulsory community treatment and advocate more effective treatments. Brief supervised discharge may be as effective (or non-effective) as formal community treatment orders (CTOs).

2. For clinicians

Clinicians and health service planners who wish to reduce hospital admissions should consider alternatives with stronger evidence for effectiveness such as Intensive Case Management (Dieterich 2010). One possible interpretation of the results from the OCTET study (is that a short period of conditional leave may suffice for some patients, and should therefore be considered when both CTO and conditional leave are available.

3. For policy makers

Based on results from this review, there is no strong evidence to support the claims made for compulsory community treatment that make it so attractive for legislators. It does not appear to reduce health service use or improve patients' social functioning. It also does not significantly reduce perceived coercion. Lack of data made it impossible to assess its effect on costs, mental state, and other aspects of patient/carer satisfaction. Legislation in this area may detract from the introduction of interventions that are of benefit to individuals with severe mental disorder such as Intensive Case Management (Dieterich 2010), but which are more expensive than legislative solutions to the problem. If governments continue to introduce this type of legislation, without further evidence for effectiveness, some evaluation of outcome should be included. Burns et al suggest that more coercive treatments such as CTOs are no more effective than briefer supervised discharge.

Implications for research

1. General

Greater adherence to CONSORT standards of reporting (Begg 1996; Moher 2001) would have enabled us to include more data from one of the included studies (Steadman 2001).

2. Specific

In spite of the widespread use of compulsory community treatment, the only studies we could find were two of court-ordered community treatment (outpatient commitment (OPC)) in the USA, and one of CTOs in England. There were much less data, and no randomised trials, of other forms of compulsory community treatment. Further research into the clinical effects of different types of compulsory community treatment is indicated.

2.1 Types of studies

The trials presented in this review show that this methodology is difficult, but not impossible, to apply to compulsory community treatment. It has, however, been argued that the level of difficulty involved means that further studies using this methodology may not be feasible (Bindman 2002). The analysis of routine administrative data sets may be an alternative. Although the analysis of such data is subject to biases and difficulties of its own, the use of epidemiological sampling frames that cover all patients placed on compulsory community treatment would help to minimise selection or follow-up bias (Preston 2002). In particular, using these would have meant that people with a history of violence who were explicitly excluded from both trials could have been included. The difficulty of such studies is the identification of suitable controls. Quasi-experimental designs comparing people from jurisdictions with similar health systems where one allows compulsory community treatment and the other does not, may be an answer. Regardless of the source, rigorous multivariable statistical controls are essential (Swanson 2014).

If further RCTs are attempted, all eligible patients should be randomised, with adequate sample sizes, entirely voluntary controls and minimal protocol violations (Rugkasa 2014).They should examine multiple outcomes, not simply hospitalisation, and consider if there is an optimum length of treatment for different cases. In addition to quantitative research, qualitative techniques may give additional insights into the effect of compulsory community treatment on patients, carers, and healthcare professionals (O'Reilly 2001). We may also need to consider the place of compulsory community treatment in the range of coercive measures used to improve compliance with treatment, and look at additional outcomes such as risk reduction (Bindman 2002).

2.2 Setting

Another interesting finding was the absence of any work from outside the English-speaking world, even though our literature search was not restricted to publications in English. We do not know whether this is due to publication bias, or because such legislation is either absent or accepted without controversy.

2.3 Participants

Further research may determine whether there are particular people with specific problems best managed with CTOs.

2.4 Interventions

We require further well-conducted studies to establish whether it is the intensity of treatment, its compulsory nature or legislative framework that affects outcomes.

2.5 Outcomes

Although the outcomes that were recorded were highly appropriate, it was not always possible to include data on psychosocial outcomes as measured by standardised instruments in the meta-analyses. Studies should use well-validated instruments to measure outcome, and should also collect and report categorical and 'count' data, such as days in hospital. Data should be in a form that can easily be incorporated into a systematic review with means and standard deviations (or standard errors) of all continuous outcome variables. In spite of the ethical and practical issues, the ideal evaluation of CTOs would be RCT comparisons of involuntary versus voluntary treatment for each different type of compulsory community treatment given the wide variations across jurisdictions. At present, the only RCT evidence that compares compulsory community treatment with voluntary care concerns court-ordered OPC in the USA. Elsewhere, the OCTET study compared two forms of compulsory community treatment of varying intensity and duration in England, and there is no RCT evidence from any other jurisdiction.

ACKNOWLEDGEMENTS

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies [ordered by study ID]

Burns 2013

Methods	Allocation: randomised (1:1 ratio). Blindess: not blinded: randomisation involved allocation to two different types of legal status. It was therefore both impossible and unlawful to mask research assistants, treating clinicians or patients Duration: 12 months.
Participants	Diagnosis: patients with psychosis discharged from hospital - 84% had schizophrenia - diagnostic criteria not stated N = 336 (ITT analysis). Age: 18 - 65 years. Sex: 225 M, 111 F. History: involuntarily admitted to hospital with psychosis and deemed suitable for supervised outpatient care by the treating clinicians. Exclusion criteria: nil.
Interventions	1. Compulsory community treatment (CCT). 2. Supervised discharge (Section 17 leave): patients allowed to leave hospital for some hours or days, or even exceptionally weeks, while still subject to recall
Outcomes	Service use: readmission to hospital, number of days in psychiatric hospital, number of readmissions, time to admission Mental state: BPRS. Global state: GAF. Unable to use: loss to care, adherence to prescribed. medication, satisfaction with services, engagement with clinical services
Notes	ITT analysis. Both intervention and control groups were subject to some form of compulsory community treatment for at least part of the study

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Consenting participants were randomly assigned (ratio 1:1) by an independent statistician to be discharged from hospital either on CTO or Section 17 leave. Randomisation used random permuted blocks with lengths of two, four, and six, and stratified for sex (male or female), schizophrenic diagnosis (yes or no) and duration of illness (<2 years or ≥2 years). Assignments were enclosed in sequentially numbered,

Burns 2013 *(Continued)*

		opaque, sealed envelopes and stored by a researcher independent to the trial team
Allocation concealment (selection bias)	Unclear risk	The details of the sequence remained unknown to all members of the trial team until recruitment, data collection, and analyses were completed. Randomisation took place after consent was obtained and the baseline interview was done. The envelope was opened on the day of the interview by the independent researcher after recording the participant's trial identification number on the envelope. She then communicated the randomised allocation to the recruiting researcher by telephone
Blinding (performance bias and detection bias) All outcomes	High risk	See above - randomisation involved allocation to two different types of legal status. It was therefore both impossible and unlawful to mask research assistants, treating clinicians or patients
Incomplete outcome data (attrition bias) All outcomes	Low risk	There was no attrition for the primary outcome measure, or health service use; outcome data on psychiatric symptoms and the GAF were only available on 70% of the sample
Selective reporting (reporting bias)	Low risk	
Other bias	High risk	Other potential sources of bias in the study included allowing clinicians to make decisions independent of initial randomisation-whereby 40 patients (25%) allocated to Section 17 were subsequently placed on a CTO during the study and 35 patients randomised to CTOs (22%) did not actually receive the intervention. A sensitivity analysis to remove these protocol violations may, in turn, have left the study underpowered and not removed the possibility that Section 17 patients swapped to a CTO might have been more severely ill than those remaining on Section 17 as per the protocol. There is uncertainty concerning the control condition. Although the length of **initial** compulsory outpatient treatment differed widely between the two groups

Burns 2013   (*Continued*)

		(medians of 183 vs. eight days), the Section 17 group averaged four months on some form of compulsory treatment. A final issue is generalisability. Around 20% of the sample were ineligible or refused to take part. These participants may have been the most unwell or insightless, and therefore the ones most likely to benefit from CTOs

Steadman 2001

Methods	Allocation: randomised, described. Blindness: unclear*. Duration: 11 months.
Participants	Diagnosis: majority had psychosis - diagnostic criteria not stated. N = 152.* Age: over 18 years. Sex: 94 M, 48 F. History: poor compliance with services when discharged. Exclusion criteria: history of violence.
Interventions	1. CCT: enhanced service package + intensive, court-ordered compulsory OPC, including involuntary medication for people thought by court to lack capacity to give informed consent. N = 78. 2. Standard care: enhanced service package with inpatient assessment and comprehensive discharge treatment plan in which patients participated, case management, and oversight by OPC co-ordinating plan. N = 64.**
Outcomes	Service use: number of admissions, compliance with medication. Social functioning: number of arrests, homelessness. Satisfaction with care: perceived coercion (MAES). Unable to use - Service use: hospitalisation - length of stay (no SD), remaining in contact (leaving the study early) (data unusable). Mental state: PANSS (no SD). Global state: GAF (no SD). Quality of life: LBQL (no SD). Adverse effects: various side effects (no SD).
Notes	ITT analysis. * Study did not specifically mention blindness but did use self-report measures for at least some of the outcomes, which are effectively self-blinding *142 completed baseline interview, 10 excluded from all reporting **There was a suggestion that members of the control group and their case managers thought that they were actually on OPC

Risk of bias

Steadman 2001 (*Continued*)

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	The study used a random number list to identify assignment to either the intervention or control group. In this study, a random number list was generated by computer, which then split 200 numbers between one group and the other
Allocation concealment (selection bias)	Unclear risk	Unclear. The printed list was maintained in the research team's office in a locked file. When the treatment team had completed their treatment plan, they called the research team who checked the computer list to see whether the client was to be assigned to the experimental or comparison group
Blinding (performance bias and detection bias) All outcomes	Unclear risk	No specific mention in the study but did use self-report measures for at least some of the outcomes, which are effectively self-blinding
Incomplete outcome data (attrition bias) All outcomes	High risk	Only between 57 and 68 per cent of the participants completed interviews at one, five, and 11 months after hospital discharge. Only some outcomes were assessed by ITT analysis
Selective reporting (reporting bias)	Unclear risk	Unclear from paper
Other bias	Unclear risk	Unclear, no conflict of interests reported.

Swartz 1999

Methods	Allocation: randomised. Blindness: not blinded. Duration: 12 months.
Participants	Diagnosis: schizophrenia, schizoaffective disorder or other major psychotic or affective disorder - diagnostic criteria not stated. N = 264.* Age: over 18 years. Sex: 132 M, 132 F. History: ill > 1 year, significant functional impairment (NCFAS score >/= 90), intensive treatment in past 2 years, awaiting period of court-ordered CCT, only included patients discharged from hospital not those already living in the community. Exclusion criteria: personality disorder, psychoactive substance use disorder, organic brain syndrome in absence of primary psychotic or mood disorder, recent serious act of violence

	involving injury or use of a weapon.*
Interventions	1. CCT: intensive, court-ordered compulsory OPC. N = 129. 2. Standard care: control group were released from OPC by notifying the court. N = 135
Outcomes	Service use: number of admissions,compliance with medication. Social functioning: number of arrests, threatening behaviour, homelessness. Quality of life: victimisation - number of violent or non-violent attacks Satisfaction with care: perceived coercion (MAES). Unable to use - Hospitalisation: length of stay (data unusable). Leaving the study early (data unusable).
Notes	* Data for this review based only on those randomised to treatment groups and only non-violent participants were randomised The RCT was supplemented by a non-random post hoc analysis of the intervention group based on duration of involuntary outpatient treatment. Renewals of CCT were not randomised for patients who no longer met legal criteria ITT analysis.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Stated to be randomised but did not describe process.
Allocation concealment (selection bias)	Unclear risk	Stated to be randomised but did not describe process.
Blinding (performance bias and detection bias) All outcomes	Unclear risk	No specific mention in the study but used self-report measures for at least some of the outcomes, which are effectively self-blinding
Incomplete outcome data (attrition bias) All outcomes	Low risk	Of the identified eligible patients, about 12% refused to participate. Subsequent attrition from the study was 18.2% (N = 48) but bias was minimised by ITT analysis
Selective reporting (reporting bias)	Unclear risk	Unclear from paper.
Other bias	Unclear risk	Unclear, no conflicts of interest reported.

BPRS: Brief Psychiatric Rating Scale
CCT - Compulsory community treatment
CTO: community treatment orders

GAF - Global Assessment of Functioning Scale

ITT - Intent-to-treat
LBQL - Lehman Brief Quality of Life Interview
MAES - MacArthur Modified Admission Experience Survey

NCFAS - North Carolina Functional Assessment Scale
OPC - Outpatient commitment
PANSS - Positive and Negative Syndrome Scale
RCT: randomised controlled trial
SD - Standard deviation
vs: versus

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

Study	Reason for exclusion
Bindman 2002	Allocation: not randomised, review.
Borum 1999	Allocation: not randomised.
Brophy 2006	Allocation: not randomised, no controls.
Burgess 2006	Allocation: not randomised.
Bursten 1986	Allocation: not randomised.
Chaimowitz 2004	Review: no primary data.
Dawson 2006	Review: no primary data.
Fernandez 1990	Allocation: not randomised, no controls.
Frank 2005	Allocation: not randomised.
Geller 1998	Allocation: not randomised.
Gray 2005	Review: no primary data.
Greeman 1985	Allocation: not randomised.
Hiday 1987	Allocation: not randomised.
Hiday 1989	Allocation: not randomised.
Hiday 1999	Allocation: not randomised.
Hunt 2007	Allocation: not randomised.

Jethwa 2008	Allocation: not randomised, review.
Kanter 1995	Allocation: not randomised, review.
Kisely 2004	Allocation: not randomised.
Kisely 2005	Allocation: not randomised.
Kisely 2006	Review: no primary data.
Kisely 2006a	Review: no primary data.
Kisely 2007	Review: no primary data.
Kisely 2007a	Review: no primary data.
Kisely 2013	Allocation: not randomised.
Kisely 2013a	Allocation: not randomised.
Lawton-Smith 2008	Review: no primary data.
Lidz 1999	Allocation: not randomised, review.
Link 2011	Allocation: not randomised.
Miller 1982a	Allocation: not randomised, before and after design.
Miller 1985	Allocation: not randomised, survey of providers.
Muirhead 2006	Allocation: not randomised, retrospective design.
Mullen 2006	Allocation: not randomised, review.
Munetz 1996	Allocation: not randomised, retrospective design.
NASMHPD 2001	Allocation: not randomised, review.
NHPF 2000	Allocation: not randomised, review.
O'Brien 2005	Allocation: not randomised, no controls.
O'Keefe 1997	Allocation: not randomised, no controls.
O'Reilly 2004	Review: no primary data.
O'Reilly 2006	Qualitative evaluation: not randomised.

Patel 2008	Review: no primary data.
Preston 2002	Allocation: not randomised.
Ridgely 2001	Allocation: not randomised.
Rohland 1998	Allocation: not randomised.
Romans 2004	Allocation: not randomised.
Segal 2006	Allocation: not randomised.
Segal 2006a	Allocation: not randomised.
Segal 2006b	Allocation: not randomised.
Segal 2006c	Allocation: not randomised.
Segal 2006d	Allocation: not randomised.
Segal 2006e	Allocation: not randomised.
Segal 2008	Allocation: not randomised.
Segal 2009	Allocation: not randomised.
Sensky 1991	Allocation: not randomised.
Swartz 1997	Allocation: not randomised.
Swartz 2004	Allocation: not randomised.
Swartz 2006a	Allocation: not randomised.
Szmukler 2001	No primary data.
Thornicroft 2013	Not a study of compulsory community treatment.
Van Putten 1988	Allocation: not randomised, no controls.
Vaughan 2000	Allocation: not randomised.
Wagner 2003	Allocation: randomised. Participants: people with schizophrenia, schizoaffective disorder or other major psychotic or affective disorders. Intervention: 1. CCT: intensive court-ordered compulsory outpatient commitment versus 2. Standard care: control group who were released from outpatient commitment by notifying the court. Outcomes: no usable outcomes. Only the number of subsequent out-patient visits were reported, this was considered to be inherent to the process of compulsory community treatment/outpatient commitment and

	not a result of the interventions
Wales 2006	Review: no primary data.
Xiao 2004	Allocation: not randomised.
Zanni 1986	Allocation: not randomised, no controls.

CCT - Compulsory community treatment

DATA AND ANALYSES

Comparison 1.   COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Health service outcomes: 1. Readmission to hospital - by 11-12 months	2	416	Risk Ratio (M-H, Fixed, 95% CI)	0.98 [0.79, 1.21]
2 Health service outcomes: 2. Compliance with medication - by 11-12 months	2	416	Risk Ratio (M-H, Fixed, 95% CI)	0.99 [0.83, 1.19]
3 Patient level outcomes: 1a. Social functioning: trouble with police - by 11-12 months	2		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
3.1 at least one arrest	2	416	Risk Ratio (M-H, Fixed, 95% CI)	0.97 [0.62, 1.52]
3.2 ever arrested/picked up by police for violence against a person	2	416	Risk Ratio (M-H, Fixed, 95% CI)	0.82 [0.56, 1.21]
4 Patient level outcomes: 1b. Social functioning: homeless - by 11-12 months	2	416	Risk Ratio (M-H, Fixed, 95% CI)	0.67 [0.39, 1.15]
5 Patient level outcomes: 2. Quality of life: victimisation - by 11-12 months	1	264	Risk Ratio (M-H, Fixed, 95% CI)	0.50 [0.31, 0.80]
6 Patient level outcomes: 3. Satisfaction with care: perceived coercion - by 11-12 months	2	416	Risk Ratio (M-H, Fixed, 95% CI)	1.36 [0.97, 1.89]

Comparison 2.   COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Health service outcomes: 1. Readmission to hospital - by 12 months	1	333	Risk Ratio (M-H, Fixed, 95% CI)	0.99 [0.74, 1.32]
2 Health service outcomes: 2. Hospital bed-days	1	333	Mean Difference (IV, Fixed, 95% CI)	-8.70 [-30.88, 13.48]
3 Health service outcomes: 3. Number of readmissions by 12 months	1	119	Mean Difference (IV, Fixed, 95% CI)	-0.20 [-0.45, 0.05]
4 Health service outcomes: 4. Number with multiple readmissions by 12 months	1	333	Risk Ratio (M-H, Fixed, 95% CI)	0.56 [0.27, 1.17]

5 Health service outcomes: 5. Days in community till first admission	1	333	Mean Difference (IV, Fixed, 95% CI)	5.0 [-21.74, 31.74]
6 Patient level outcomes: 1. Mental state: BPRS	1	234	Mean Difference (IV, Fixed, 95% CI)	-0.10 [-3.17, 2.97]
7 Patient level outcomes: 2. Global state: GAF	1	237	Mean Difference (IV, Fixed, 95% CI)	-0.70 [-3.91, 2.51]

Analysis 1.1.   Comparison 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE, Outcome 1 Health service outcomes: 1. Readmission to hospital - by 11-12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE

Outcome: 1 Health service outcomes: 1. Readmission to hospital - by 11-12 months

Study or subgroup	Treatment n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Steadman 2001	40/85	27/67		31.9 %	1.17 [0.81, 1.69]
Swartz 1999	56/129	66/135		68.1 %	0.89 [0.68, 1.15]
Total (95% CI)	**214**	**202**		**100.0 %**	**0.98 [0.79, 1.21]**

Total events: 96 (Treatment), 93 (Control)

Heterogeneity: Chi^2 = 1.41, df = 1 (P = 0.23); I^2 =29%

Test for overall effect: Z = 0.21 (P = 0.83)

Test for subgroup differences: Not applicable

0.1 0.2   0.5   1   2   5   10

Favours treatment   Favours control

Analysis 1.2. Comparison I COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE, Outcome 2 Health service outcomes: 2. Compliance with medication - by 11-12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: I COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE

Outcome: 2 Health service outcomes: 2. Compliance with medication - by 11-12 months

Study or subgroup	Treatment n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Swartz 1999	54/129	55/135		50.6 %	1.03 [0.77, 1.37]
Steadman 2001	57/85	47/67		49.4 %	0.96 [0.77, 1.19]
Total (95% CI)	**214**	**202**		**100.0 %**	**0.99 [0.83, 1.19]**

Total events: 111 (Treatment), 102 (Control)

Heterogeneity: Chi² = 0.17, df = 1 (P = 0.68); I² =0.0%

Test for overall effect: Z = 0.09 (P = 0.93)

Test for subgroup differences: Not applicable

0.1 0.2 0.5 1 2 5 10

Favours treatment   Favours control

Analysis 1.3. Comparison 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE, Outcome 3 Patient level outcomes: 1a. Social functioning: trouble with police - by 11-12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE

Outcome: 3 Patient level outcomes: 1a. Social functioning: trouble with police - by 11-12 months

Study or subgroup	Treatment n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
1 at least one arrest					
Swartz 1999	19/129	22/135		65.8 %	0.90 [0.51, 1.59]
Steadman 2001	14/85	10/67		34.2 %	1.10 [0.52, 2.33]
Subtotal (95% CI)	**214**	**202**		**100.0 %**	**0.97 [0.62, 1.52]**
Total events: 33 (Treatment), 32 (Control)					
Heterogeneity: Chi² = 0.18, df = 1 (P = 0.68); I² =0.0%					
Test for overall effect: Z = 0.12 (P = 0.90)					
2 ever arrested/picked up by police for violence against a person					
Swartz 1999	33/129	42/135		100.0 %	0.82 [0.56, 1.21]
Steadman 2001	0/85	0/67			Not estimable
Subtotal (95% CI)	**214**	**202**		**100.0 %**	**0.82 [0.56, 1.21]**
Total events: 33 (Treatment), 42 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.99 (P = 0.32)					

0.1 0.2 0.5 1 2 5 10

Favours treatment   Favours control

Analysis 1.4.   Comparison 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE, Outcome 4 Patient level outcomes: 1b. Social functioning: homeless - by 11-12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE

Outcome: 4 Patient level outcomes: 1b. Social functioning: homeless - by 11-12 months

Study or subgroup	Treatment n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Swartz 1999	8/129	15/135		52.2 %	0.56 [0.24, 1.27]
Steadman 2001	12/85	12/67		47.8 %	0.79 [0.38, 1.64]
Total (95% CI)	**214**	**202**		**100.0 %**	**0.67 [0.39, 1.15]**

Total events: 20 (Treatment), 27 (Control)

Heterogeneity: Chi^2 = 0.38, df = 1 (P = 0.54); I^2 =0.0%

Test for overall effect: Z = 1.44 (P = 0.15)

Test for subgroup differences: Not applicable

```
        0.1  0.2  0.5   1   2   5   10
      Favours treatment    Favours control
```

Analysis 1.5.   Comparison 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE, Outcome 5 Patient level outcomes: 2. Quality of life: victimisation - by 11-12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE

Outcome: 5 Patient level outcomes: 2. Quality of life: victimisation - by 11-12 months

Study or subgroup	Treatment n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Swartz 1999	20/129	42/135		100.0 %	0.50 [0.31, 0.80]
Total (95% CI)	**129**	**135**		**100.0 %**	**0.50 [0.31, 0.80]**

Total events: 20 (Treatment), 42 (Control)

Heterogeneity: not applicable

Test for overall effect: Z = 2.88 (P = 0.0040)

Test for subgroup differences: Not applicable

```
        0.1  0.2  0.5   1   2   5   10
      Favours treatment    Favours control
```
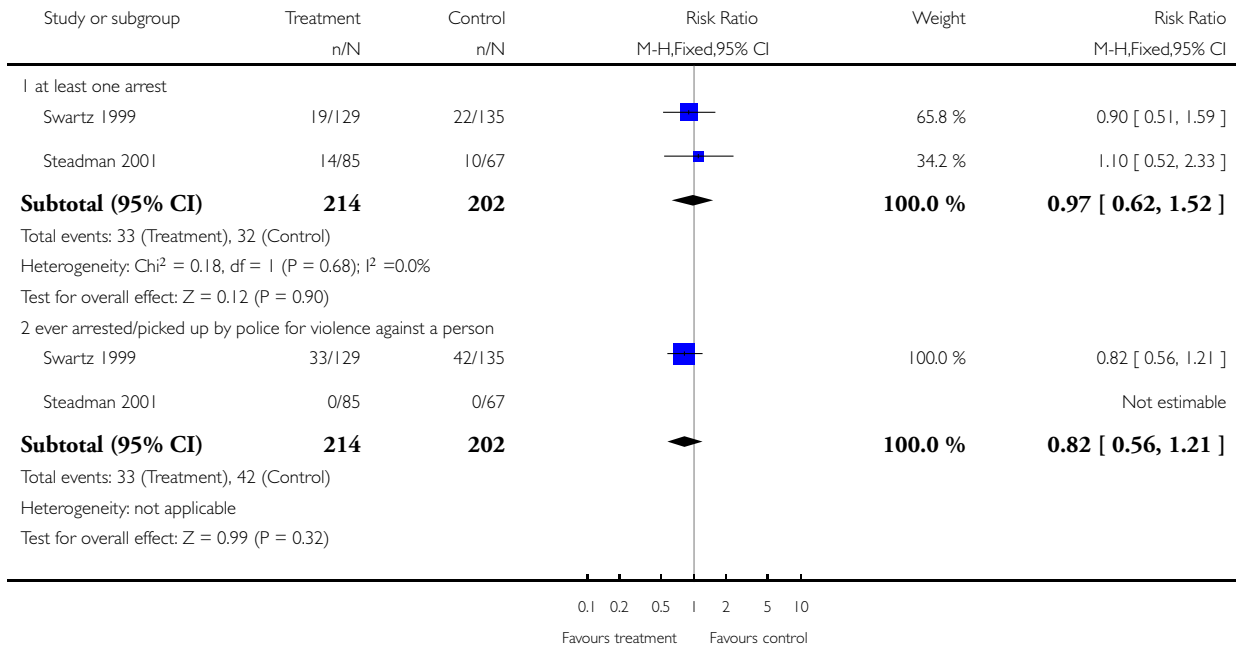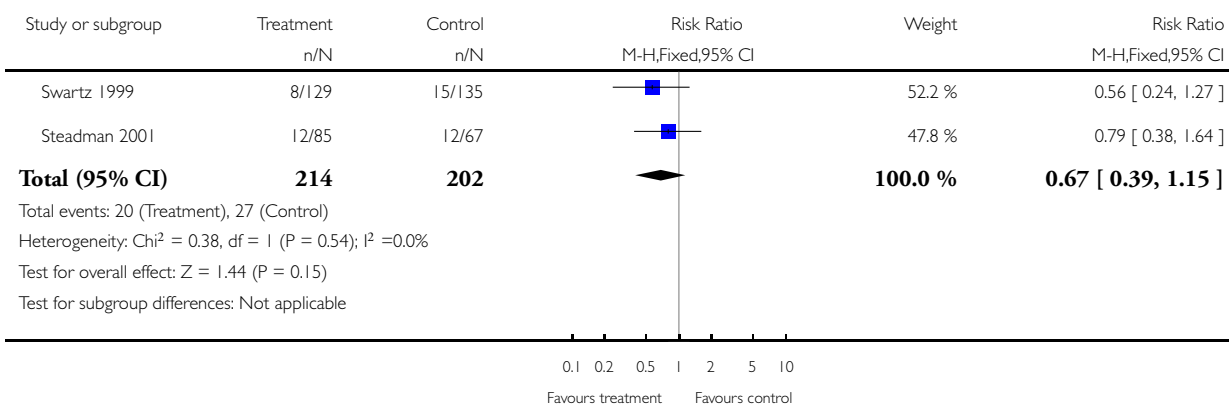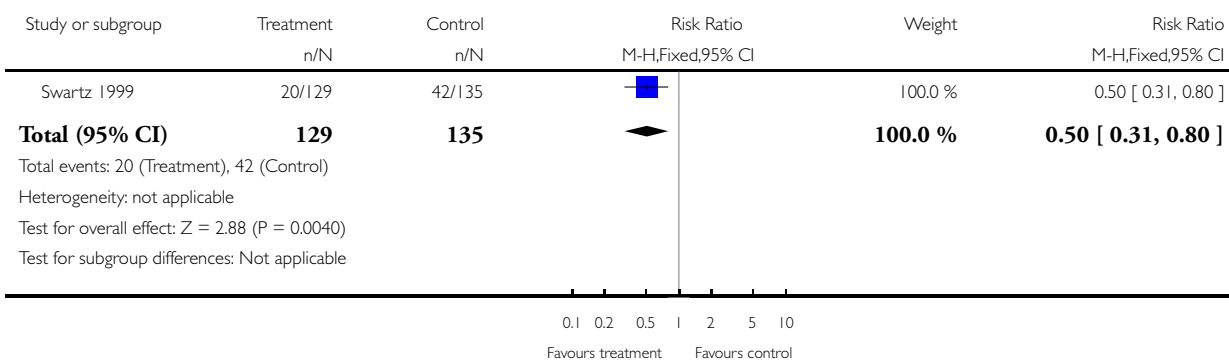
Analysis 1.6. Comparison 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE, Outcome 6 Patient level outcomes: 3. Satisfaction with care: perceived coercion - by 11-12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 1 COMPULSORY COMMUNITY TREATMENT vs STANDARD CARE

Outcome: 6 Patient level outcomes: 3. Satisfaction with care: perceived coercion - by 11-12 months

Study or subgroup	Treatment	Control	Risk Ratio	Weight	Risk Ratio
	n/N	n/N	M-H,Fixed,95% CI		M-H,Fixed,95% CI
Swartz 1999	37/129	27/135		58.1 %	1.43 [0.93, 2.21]
Steadman 2001	27/85	17/67		41.9 %	1.25 [0.75, 2.10]
Total (95% CI)	**214**	**202**		**100.0 %**	**1.36 [0.97, 1.89]**

Total events: 64 (Treatment), 44 (Control)

Heterogeneity: Chi² = 0.16, df = 1 (P = 0.69); I² =0.0%

Test for overall effect: Z = 1.81 (P = 0.071)

Test for subgroup differences: Not applicable

0.1 0.2 0.5 1 2 5 10

Favours treatment   Favours control
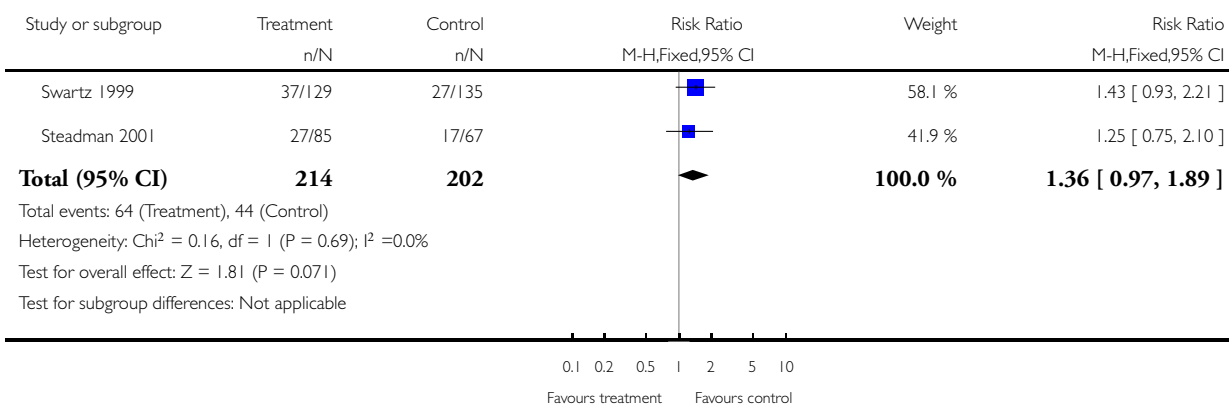
Analysis 2.1. Comparison 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17), Outcome 1 Health service outcomes: 1. Readmission to hospital - by 12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17)

Outcome: 1 Health service outcomes: 1. Readmission to hospital - by 12 months

Study or subgroup	Treatment	Control	Risk Ratio	Weight	Risk Ratio
	n/N	n/N	M-H,Fixed,95% CI		M-H,Fixed,95% CI
Burns 2013	59/166	60/167		100.0 %	0.99 [0.74, 1.32]
Total (95% CI)	**166**	**167**		**100.0 %**	**0.99 [0.74, 1.32]**

Total events: 59 (Treatment), 60 (Control)

Heterogeneity: not applicable

Test for overall effect: Z = 0.07 (P = 0.94)

Test for subgroup differences: Not applicable

0.01 0.1 1 10 100

Favours treatment   Favours control

Analysis 2.2. Comparison 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17), Outcome 2 Health service outcomes: 2. Hospital bed-days.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17)

Outcome: 2 Health service outcomes: 2. Hospital bed-days

Study or subgroup	Treatment		Control		Mean Difference	Weight	Mean Difference
	N	Mean(SD)	N	Mean(SD)	IV,Fixed,95% CI		IV,Fixed,95% CI
Burns 2013	166	82.2 (102)	167	90.9 (104.5)		100.0 %	-8.70 [-30.88, 13.48]
Total (95% CI)	**166**		**167**			**100.0 %**	**-8.70 [-30.88, 13.48]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.77 (P = 0.44)							
Test for subgroup differences: Not applicable							

-200 -100 0 100 200

Favours treatment   Favours control

Analysis 2.3. Comparison 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17), Outcome 3 Health service outcomes: 3. Number of readmissions by 12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17)

Outcome: 3 Health service outcomes: 3. Number of readmissions by 12 months

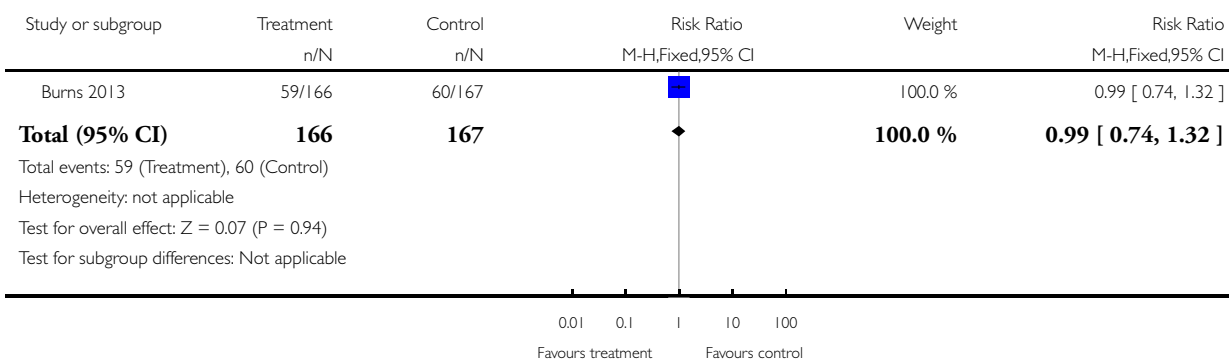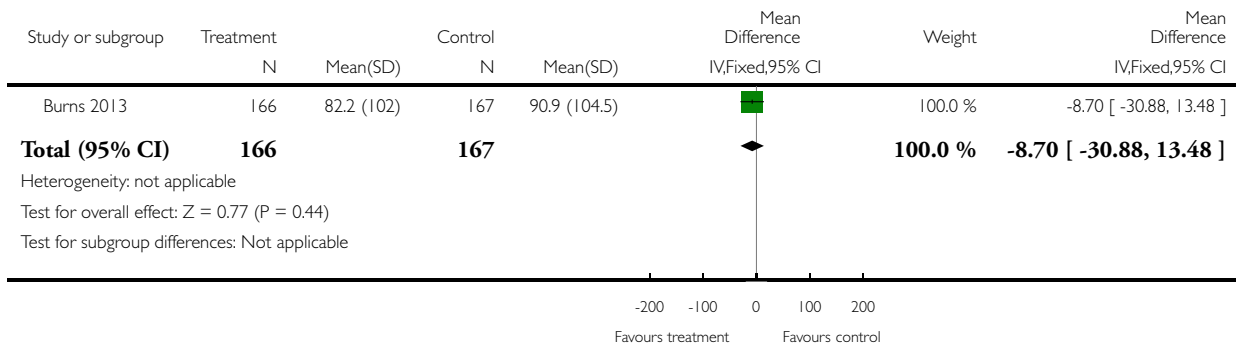| Study or subgroup	Treatment		Control		Mean Difference	Weight	Mean Difference
	N	Mean(SD)	N	Mean(SD)	IV,Fixed,95% CI		IV,Fixed,95% CI
Burns 2013	59	1.2 (0.6)	60	1.4 (0.8)		100.0 %	-0.20 [-0.45, 0.05]
Total (95% CI)	**59**		**60**			**100.0 %**	**-0.20 [-0.45, 0.05]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.54 (P = 0.12)							
Test for subgroup differences: Not applicable							

-2 -1 0 1 2

Favours treatment   Favours control

Analysis 2.4. Comparison 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17), Outcome 4 Health service outcomes: 4. Number with multiple readmissions by 12 months.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17)

Outcome: 4 Health service outcomes: 4. Number with multiple readmissions by 12 months

Study or subgroup	Treatment n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Burns 2013	10/166	18/167		100.0 %	0.56 [0.27, 1.17]
Total (95% CI)	**166**	**167**		**100.0 %**	**0.56 [0.27, 1.17]**

Total events: 10 (Treatment), 18 (Control)
Heterogeneity: not applicable
Test for overall effect: Z = 1.54 (P = 0.12)
Test for subgroup differences: Not applicable

```
      0.01   0.1    1    10   100
    Favours treatment    Favours control
```

Analysis 2.5. Comparison 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17), Outcome 5 Health service outcomes: 5. Days in community till first admission.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17)
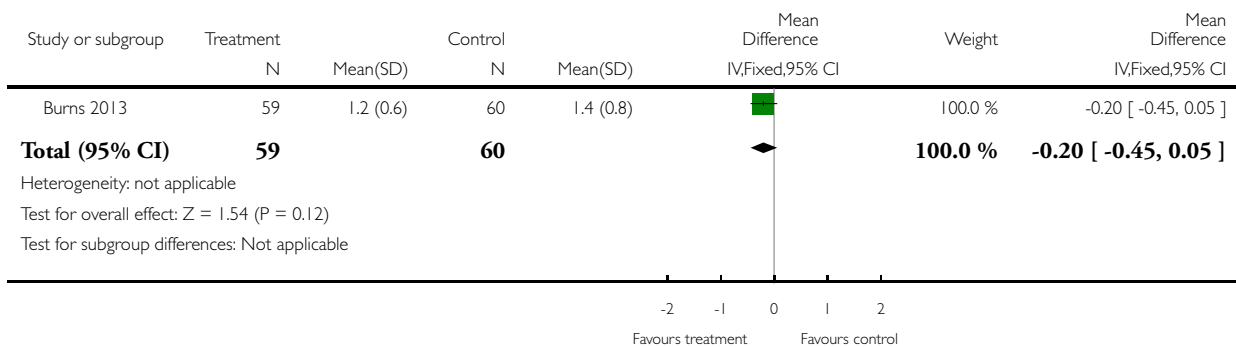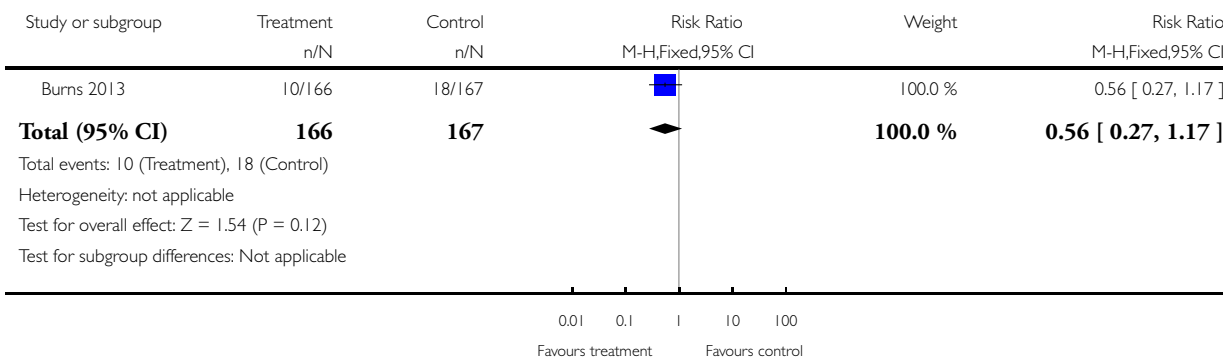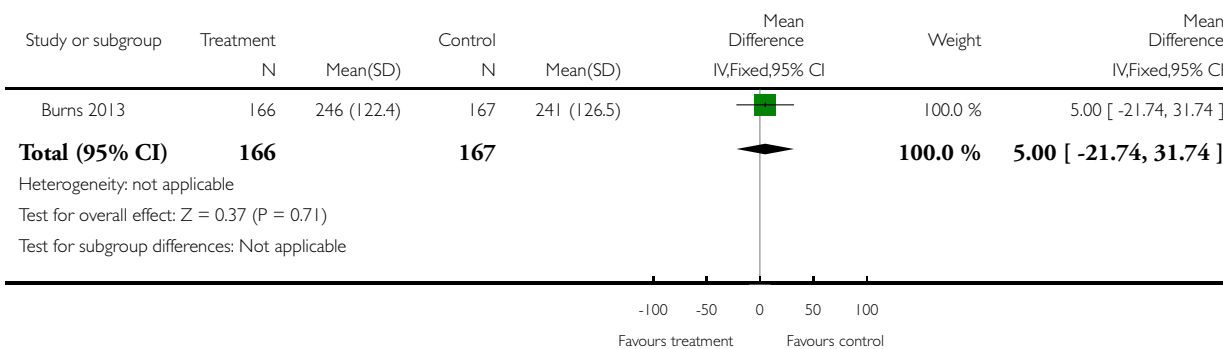
Outcome: 5 Health service outcomes: 5. Days in community till first admission

Study or subgroup	Treatment N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Burns 2013	166	246 (122.4)	167	241 (126.5)		100.0 %	5.00 [-21.74, 31.74]
Total (95% CI)	**166**		**167**			**100.0 %**	**5.00 [-21.74, 31.74]**

Heterogeneity: not applicable
Test for overall effect: Z = 0.37 (P = 0.71)
Test for subgroup differences: Not applicable

```
      -100   -50    0    50   100
    Favours treatment    Favours control
```
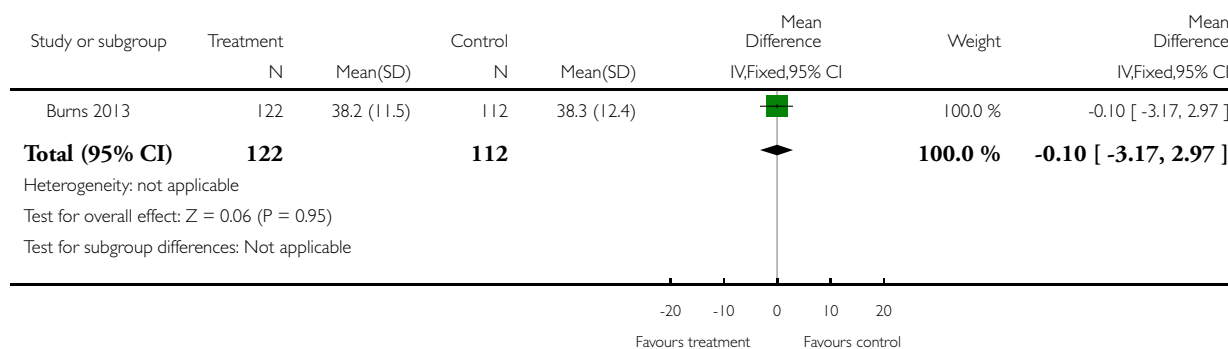
Analysis 2.6. Comparison 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17), Outcome 6 Patient level outcomes: 1. Mental state: BPRS.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17)

Outcome: 6 Patient level outcomes: 1. Mental state: BPRS

Study or subgroup	Treatment		Control		Mean Difference	Weight	Mean Difference
	N	Mean(SD)	N	Mean(SD)	IV,Fixed,95% CI		IV,Fixed,95% CI
Burns 2013	122	38.2 (11.5)	112	38.3 (12.4)		100.0 %	-0.10 [-3.17, 2.97]
Total (95% CI)	**122**		**112**			**100.0 %**	**-0.10 [-3.17, 2.97]**

Heterogeneity: not applicable

Test for overall effect: Z = 0.06 (P = 0.95)

Test for subgroup differences: Not applicable

```
         -20   -10    0    10    20
       Favours treatment   Favours control
```
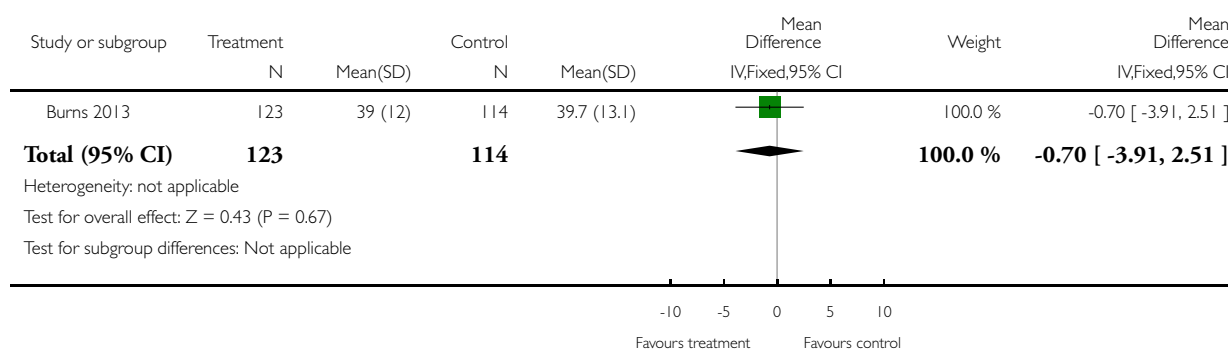
Analysis 2.7. Comparison 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17), Outcome 7 Patient level outcomes: 2. Global state: GAF.

Review: Compulsory community and involuntary outpatient treatment for people with severe mental disorders

Comparison: 2 COMMUNITY TREATMENT ORDERS vs SUPERVISED DISCHARGE (SECTION 17)

Outcome: 7 Patient level outcomes: 2. Global state: GAF

Study or subgroup	Treatment		Control		Mean Difference	Weight	Mean Difference
	N	Mean(SD)	N	Mean(SD)	IV,Fixed,95% CI		IV,Fixed,95% CI
Burns 2013	123	39 (12)	114	39.7 (13.1)		100.0 %	-0.70 [-3.91, 2.51]
Total (95% CI)	**123**		**114**			**100.0 %**	**-0.70 [-3.91, 2.51]**

Heterogeneity: not applicable

Test for overall effect: Z = 0.43 (P = 0.67)

Test for subgroup differences: Not applicable

```
         -10    -5    0    5    10
       Favours treatment   Favours control
```

A P P E N D I C E S

Appendix 1. Search Strategies

2003 Searches

1. Cochrane Schizophrenia Group's Register (May 2003)

We searched using the phrase:

[((community* AND treatment* AND order*) OR (involuntary* AND outpatient* AND treatment*) OR (involuntary* AND outpatient* AND commitment*) OR (extended* AND leave*) in Title or (*community* AND *treatment* AND *order*) OR (*involuntary* AND *outpatient* AND *treatment*) OR (*involuntary* AND *outpatient* AND *commitment*) OR (*extended* AND *leave*) or (*supervised* AND *discharge*) in title, abstract, index terms of REFERENCE] or Involuntary Commitment in intervention of STUDY)]

The Schizophrenia Groups trials register is based on regular searches of BIOSIS Inside; CENTRAL; CINAHL; EMBASE; MEDLINE and PsycINFO; the hand searching of relevant journals and conference proceedings, and searches of several key grey literature sources. A full description is given in the Group's module.

2. Cochrane Library (Issue 2 2003)

We searched using the phrase:
[(exp Commitment of Mentally Ill/ or (community NEAR treatment NEAR order) or (involuntary NEAR outpatient NEAR treatment) or (involuntary NEAR outpatient NEAR commitment) or (extended NEAR leave) or (supervised NEAR discharge)]

3. BIOSIS (1985 to July 2003)

We searched using the phrase:
[(Commitment AND Mentally AND Ill or (extended AND leave) or (community AND treatment AND order) or (involuntary AND outpatient AND treatment) or (involuntary AND outpatient AND commitment) or (extended AND leave) or (supervised AND discharge) or (mandatory AND programs))]

4. CINAHL (1982 to July 2003)

We searched using the Cochrane Schizophrenia Group's phrase for randomised controlled trials combined with:
[(exp Involuntary Commitment/ or exp Hospitalization/ or (extended adj1 leave) or (community adj2 treatment adj2 order) or exp "NONCOMPLIANCE (NANDA)"/ or (involuntary adj3 outpatient adj3 treatment) or (involuntary adj3 outpatient adj3 commitment) or (supervised adj2 discharge) or (mandatory adj3 programs) or (extended adj3 leave))

5. EMBASE (1980 to July 2003)

We searched using the Cochrane Schizophrenia Group's phrase for randomised controlled trials combined with:
[(exp Commitment of Mentally Ill/ or (extended adj1 leave) or (community adj2 treatment adj2 order) or (involuntary adj3 outpatient adj3 treatment) or (involuntary adj3 outpatient adj3 commitment) or (extended adj3 leave) or (supervised adj2 discharge) or (mandatory adj3 programs))

6. MEDLINE (1966 to July 2003)

We searched using the Cochrane Schizophrenia Group's phrase for randomised controlled trials combined with:
[(exp Commitment of Mentally Ill/ or jurisprudence/ or exp mandatory programs/ or (extended adj1 leave) or (community adj2 treatment adj2 order) or (involuntary adj3 outpatient adj3 treatment) or (involuntary adj3 outpatient adj3 commitment) or (extended adj leave) or (extended adj3 leave) or (supervised adj2 discharge))

7. PsycINFO (1872 to July 2003)

We searched using the Cochrane Schizophrenia Group's phrase for randomised controlled trials combined with:
[(exp outpatient commitment/ or exp Legal Processes/ or exp "Commitment (Psychiatric)"/ or exp Psychiatric Hospitalization/ or exp Laws/ or exp Involuntary Treatment/ or (community adj2 treatment adj2 order) or (involuntary adj3 outpatient adj3 treatment) or (involuntary adj3 outpatient adj3 commitment) or (extended adj3 leave) or (supervised adj2 discharge) or (mandatory adj3 programs))]

8. SCISEARCH

Science Citation Index: we sought each of the included studies as a citation on the SCISEARCH database. We then inspected reports of articles that had cited these studies to identify further trials.

9. Google - Internet search engine (July 2003)

We searched the Internet to identify any relevant publications using the following terms:
community treatment order, involuntary outpatient treatment, involuntary outpatient commitment, extended leave, extended release or supervised discharge.

2008 Searches

We searched the Cochrane Schizophrenia Group's register using the phrase:
[((((*treatment* AND * order* within the same field) OR *(involuntar* AND *outpatient* within the same field) OR (*extend* AND *leave* within the same field) OR (*supervis* AND *discharg* within the same field) OR (*compulsor* or *compulsion*) in title abstract or index terms of REFERENCE) OR (*commitment of mentally ill* in index terms of REFERENCE) OR (Involuntary* OR Outpatient* intervention of STUDY)]
This register is compiled by systematic searches of major databases, handsearches, and conference proceedings (see group module).

Additional searches by authors

1. Cochrane Library (Issue 2 2008)

We searched using the phrase:
[(exp Commitment of Mentally Ill/ or (community NEAR treatment NEAR order) or (involuntary NEAR outpatient NEAR treatment) or (involuntary NEAR outpatient NEAR commitment) or (extended NEAR leave) or (supervised NEAR discharge)]

2. BIOSIS (1985 to December 2008)

We searched using the phrase:
[(Commitment AND Mentally AND Ill or (extended AND leave) or (community AND treatment AND order) or (involuntary AND outpatient AND treatment) or (involuntary AND outpatient AND commitment) or (extended AND leave) or (supervised AND discharge) or (mandatory AND programs))]

3. CINAHL (1982 to December 2008)

We searched using the Cochrane Schizophrenia Group's phrase for randomised controlled trials combined with:
[(exp Involuntary Commitment/ or exp Hospitalisation/ or (extended adj1 leave) or (community adj2 treatment adj2 order) or exp "NONCOMPLIANCE (NANDA)"/ or (involuntary adj3 outpatient adj3 treatment) or (involuntary adj3 outpatient adj3 commitment) or (supervised adj2 discharge) or (mandatory adj3 programs) or (extended adj3 leave))

4. EMBASE (1980 to December 2008)

We searched using the Cochrane Schizophrenia Group's phrase for randomised controlled trials combined with:
[(exp Commitment of Mentally Ill/ or (extended adj1 leave) or (community adj2 treatment adj2 order) or (involuntary adj3 outpatient adj3 treatment) or (involuntary adj3 outpatient adj3 commitment) or (extended adj3 leave) or (supervised adj2 discharge) or (mandatory adj3 programs))

5. MEDLINE (1966 to December 2008)

We searched using the Cochrane Schizophrenia Group's phrase for randomised controlled trials combined with:
[(exp Commitment of Mentally Ill/ or jurisprudence/ or exp mandatory programs/ or (extended adj1 leave) or (community adj2 treatment adj2 order) or (involuntary adj3 outpatient adj3 treatment) or (involuntary adj3 outpatient adj3 commitment) or (extended adj leave) or (extended adj3 leave) or (supervised adj2 discharge))

6. PsycINFO (1872 to December 2008)

We searched using the Cochrane Schizophrenia Group's phrase for randomised controlled trials combined with:
[(exp outpatient commitment/ or exp Legal Processes/ or exp "Commitment (Psychiatric)"/ or exp Psychiatric Hospitalization/ or exp Laws/ or exp Involuntary Treatment/ or (community adj2 treatment adj2 order) or (involuntary adj3 outpatient adj3 treatment) or (involuntary adj3 outpatient adj3 commitment) or (extended adj3 leave) or (supervised adj2 discharge) or (mandatory adj3 programs))]

7. SCISEARCH - Science Citation Index

We sought each of the included studies as a citation on the SCISEARCH database. We then inspected reports of articles that had cited these studies to identify further trials.

8. Google - Internet search engine (December 2008)

We searched the Internet to identify any relevant publications using the following terms:
community treatment order, involuntary outpatient treatment, involuntary outpatient commitment, extended leave, extended release or supervised discharge.

Searching other resources

1. Reference searching

We also inspected the references of all identified studies (including those rejected from the review) for more studies.

2. Personal contact

We contacted the first author of each included study and known experts who had published reviews in the field for information regarding unpublished trials and extra data on the published trials.

Appendix 2. Previous data collection and analysis

1. Extraction

Authors SK and LAC independently extracted data from included studies. Again, we discussed any disagreement, documented our decisions and, if necessary, we contacted the authors of studies for clarification. Whenever possible we only extracted data presented in graphs and figures, we only included data if two review authors independently had the same result. We made attempts to contact authors through an open-ended request in order to obtain any missing information or for clarification whenever necessary. Where possible, we extracted data relevant to each component centre of multi-centre studies separately.

2. Management

2.1 Forms

SK and LAC extracted data onto standard, simple forms.

2.2 Data from multi-centre trials

Where possible the authors verified independently calculated centre data against original trial reports.

3. Rating scales

A wide range of instruments are available to measure outcomes in mental and physical health studies. They vary in quality and are often not validated or are created for a particular study. It is accepted generally that measuring instruments should be both reliable and have reasonable validity (Rust 1989). We included continuous data from rating scales only if the measuring instrument had been described in a peer-reviewed journal (Marshall 2000); and not those written or modified by one of the trialists for a particular trial.

4. Endpoint versus change data

We preferred to use scale endpoint data, which typically cannot have negative values and are easier to interpret from a clinical point of view. Change data are often not ordinal and are very problematic to interpret. If endpoint data were unavailable, we used change data.

5. Skewed data

Continuous data on clinical and social outcomes are often not normally distributed. To avoid the pitfall of applying parametric tests to non-parametric data, we aim to apply the following standards to all data before inclusion: (a) standard deviations and means are reported in the paper or obtainable from the authors; (b) when a scale starts from the finite number zero, the standard deviation, when multiplied by two, is less than the mean (as otherwise the mean is unlikely to be an appropriate measure of the centre of the distribution, (Altman 1996); (c) if a scale starts from a positive value (such as PANSS which can have values from 30 to 210) the calculation described above will be modified to take the scale starting point into account. In these cases skew is present if 2SD>(S-S min), where S is the mean score and S min is the minimum score. Endpoint scores on scales often have a finite start and end point and these rules can be applied. When continuous data are presented on a scale which includes a possibility of negative values (such as change data), it is difficult to tell whether data are skewed or not. We entered skewed data from studies of fewer than 200 participants in additional tables rather than into an analysis. Skewed data pose less of a problem when looking at means if the sample size is large, and we entered skewed data from large sample sizes into syntheses.

6. Common measure

To facilitate comparison between trials, we intend to convert variables that can be reported in different metrics, such as days in hospital (mean days per year, per week or per month) to a common metric (e.g. mean days per month).

---

7. Conversion of continuous to binary

Where possible, we made efforts to convert outcome measures to dichotomous data. This could be done by identifying cut-off points on rating scales and dividing participants accordingly into 'clinically improved' or 'not clinically improved'. It was generally assumed that if there had been a 50% reduction in a scale-derived score such as the Brief Psychiatric Rating Scale (BPRS, Overall 1962) or the Positive and Negative Syndrome Scale (Kay 1986; Kay 1987), this could be considered as a clinically significant response (Leucht 2005; Leucht 2005a). If data based on these thresholds were not available, we used the primary cut-off presented by the original authors.

8. Direction of graphs

Where possible, we entered data in such a way that the area to the left of the line of no effect indicates a favourable outcome for the experimental intervention.

Assessment of risk of bias in included studies

Again working independently, two authors (SK, LAC) assessed risk of bias using the tool described in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2008) This tool encourages consideration of how the sequence was generated, how allocation was concealed, the integrity of blinding at outcome, the completeness of outcome data, selective reporting, and other biases. We would have excluded studies where allocation was clearly not concealed.

We removed trials with high risk of bias (defined as at least three out of six domains categorised as 'No') from the 'included' category. If the raters disagreed, we made the final rating by consensus with the involvement of another member of the review group. Where inadequate details of randomisation and other characteristics of trials are provided, we contacted authors of the studies in order to obtain further information. We reported non-concurrence in quality assessment was reported.

Measures of treatment effect

1. Binary data

For binary outcomes we calculated a standard estimation of the fixed-effect risk ratio (RR) and its 95% confidence interval (CI) as well as the number needed to treat/harm statistic (NNT/H). We calculated NNT using the methodology of Cook 1995 for the results that were not significant. If statistically significant we took into account the event rate in the control group (Bandolier 1995). If we found heterogeneity, then we made a decision about whether a quantitative synthesis (meta-analysis) was the appropriate method of summarising this body of research and used a random-effects model.

2. Continuous data

For continuous outcomes we estimated a mean difference (MD) between groups. Again, if we found heterogeneity, then we made a decision about whether a quantitative synthesis (meta-analysis) was the appropriate method of summarising this body of research and used a random-effects model.

Unit of analysis issues

1. Cluster trials

We stated in our protocol (Kisely 2004) that we would account for cluster randomisation in our analysis. However, both studies identified in our review were randomised by subject, not by clinician or practice. Studies increasingly employ 'cluster randomisation' (such as randomisation by clinician or practice) but analysis and pooling of clustered data poses problems. Firstly, authors often fail to account for intra class correlation in clustered studies, leading to a 'unit of analysis' error (Divine 1992) whereby P values are spuriously low, confidence intervals unduly narrow and statistical significance overestimated. This causes type I errors (Bland 1997; Gulliford 1999).

If clustering had not been accounted for in primary studies, we would have presented data in a table, with a (*) symbol to indicate the presence of a probable unit of analysis error. In subsequent versions of this review we would have sought to contact first authors

of studies to obtain intra class correlation co-efficient (ICC) of their clustered data and to adjust for this by using accepted methods (Gulliford 1999). Should clustering have been incorporated into the analysis of primary studies, we would have presented these data as if from a non-cluster randomised study, but adjusted for the clustering effect.

We have sought statistical advice and have been advised that the binary data as presented in a report should be divided by a 'design effect'. This is calculated using the mean number of participants per cluster (m) and the ICC (Design effect=1+(m-1)*ICC) (Donner 2002). If the ICC had not been reported we would have assumed it to be 0.1 (Ukoumunne 1999).

If cluster studies had been appropriately analysed taking into account ICC and relevant data documented in the report, synthesis with other studies would have been possible using the generic inverse variance technique.

2. Cross-over trials

A major concern of cross-over trials is the carry-over effect. It occurs if an effect (e.g. pharmacological, physiological or psychological) of the treatment in the first phase is carried over to the second phase. As a consequence, on entry to the second phase, the participants can differ systematically from their initial state despite a wash-out phase. For the same reason cross-over trials are not appropriate if the condition of interest is unstable (Elbourne 2002). As both effects are very likely in serious mental illness, we would only have used data of the first phase of cross-over studies.

3. Studies with multiple treatment groups

Where a study involved more than two treatment arms, if relevant, we presented the additional treatment arms in comparisons. Where the additional treatment arms were not relevant, we did not reproduce these data.

Dealing with missing data

Reports of trials should give an adequate description of the loss of participants in terms of the number of withdrawals, dropouts, and protocol deviations. We conducted an intention to treat analysis, including all those who were randomised to either compulsory community treatment or control, regardless of subsequent disposition.

1. Overall loss of credibility

At some degree of loss of follow-up, data must lose credibility (Xia 2009). For any particular outcome, should more than 35% of data be unaccounted for, we had stated that we would not reproduce these data or use them within analyses (Kisely 2004). However, the New York study (Steadman 2001) reported attrition rates of approximately 45% for 11-month outcomes. As we were only able to identify two RCTs, we decided to subject this high attrition study to a sensitivity analysis. If we found that inclusion of this data resulted in a substantive change in the estimate of effect, we would not add them to results from Swartz 1999, but present them separately. This is a considerable *post hoc* change from the original protocol (see Differences between protocol and review).

2. Binary

In the case where attrition for a binary outcome is between 0% and 50% and where these data were not clearly described, we presented data on a 'once-randomised-always-analyse' basis (an intention to treat analysis). Those lost to follow-up were all assumed to have the same rates of negative outcome as those who completed, with the exception of the outcome of death. We undertook a sensitivity analysis testing how prone the primary outcomes were to change when 'completed' data only were compared to the intention to treat analysis using the above assumption.

3. Continuous

3.1 Attrition

In the case where attrition for a continuous outcome is between 0% and 50% and completer-only data were reported, we have reproduced these.

3.2 Standard deviations

Where there are missing measures of variance for continuous data but exact standard error and confidence intervals are available for group means, either P values or T values are available for differences in mean, we calculated standard deviation value according to method described in Section 7.7.3 of the *Handbook* (Higgins 2008). If standard deviations were not reported and could not be calculated from available data, we asked authors to supply the data. In the absence of data from authors, we used the mean standard deviation from other studies.

3.3 Last observation carried forward

We anticipated that in some studies the method of last observation carried forward (LOCF) would be employed within the study report. As with all methods of imputation to deal with missing data, LOCF introduces uncertainty about the reliability of the results. Therefore, where LOCF data has been used in the trial, if less than 50% of the data had been assumed, we reproduced these data, and indicated that they are the product of LOCF assumptions.

Assessment of heterogeneity

1. Clinical heterogeneity

To judge clinical heterogeneity, we considered all included studies, initially without seeing comparison data. We simply inspected all studies for clearly outlying situations or people which we had not predicted would arise. Should such situations or participant groups arise we will fully discuss these.

2. Methodological heterogeneity

We considered all included studies initially, without seeing comparison data, to judge methodological heterogeneity. We simply inspected all studies for clearly outlying methods which we had not predicted would arise. Should such methodological outliers arise we will fully discuss these.

3. Statistical

3.1 Visual inspection

We visually inspected graphs to investigate the possibility of statistical heterogeneity.

3.2 Employing the I^2 statistic

We investigated heterogeneity between studies by considering the I^2 method alongside the Chi^2 P value. The I^2 provides an estimate of the percentage of inconsistency thought to be due to chance (Higgins 2003). The importance of the observed value of I^2 depends on i. magnitude and direction of effects and ii. strength of evidence for heterogeneity (e.g. P value from Chi^2 test, or a confidence interval for I^2).

We interpreted I^2 estimate greater than or equal to 75% accompanied by a statistically significant Chi^2 statistic as evidence of substantial levels of heterogeneity (Section 9.5.2 - Higgins 2008) and explored reasons for heterogeneity. If the inconsistency was high and we found clear reasons, we presented data separately.

Assessment of reporting biases

Reporting biases arise when the dissemination of research findings is influenced by the nature and direction of results (Egger 1997). These are described in Section 10 of the *Handbook* (Higgins 2008). We are aware that funnel plots may be useful in investigating reporting biases but are of limited power to detect small-study effects. We did not use funnel plots for outcomes where there were 10 or fewer studies, or where all studies were of similar sizes. In other cases, where funnel plots were possible, we sought statistical advice in their interpretation. Because there were never more than two studies for each outcome, we were unable to use this technique to investigate publication/small study bias.

Data synthesis

We understand that there is no closed argument for preference for use of fixed-effect or random-effects models. The random-effects method incorporates an assumption that the different studies are estimating different, yet related, intervention effects. This often seems to be true to us and the random-effects model takes into account differences between studies even if there is no statistically significant heterogeneity. There is, however, a disadvantage to the random-effects model. It puts added weight onto small studies which often are the most biased ones. Depending on the direction of effect, these studies can either inflate or deflate the effect size. Therefore, we chose the fixed-effect model for all analyses. The reader is, however, able to choose to inspect the data using the random model.

Subgroup analysis and investigation of heterogeneity

1. Subgroups

We had hoped to investigate subgroups including different variations of types of intervention (e.g. community treatment orders, involuntary outpatient treatment, involuntary outpatient commitment or supervised discharge). Because there were never more than two studies for each outcome, and all were of court-ordered compulsory community treatment, we could not undertake such sensitivity analyses as we had hoped.

2. Investigation of heterogeneity

2.1 Unanticipated heterogeneity

Should unanticipated clinical or methodological heterogeneity have been obvious, we would have simply stated hypotheses regarding these for future reviews or versions of this review. We did not anticipate undertaking analyses relating to these.

2.2 Anticipated heterogeneity

We did not anticipate specific reasons for heterogeneity.

Sensitivity analysis

We had anticipated undertaking a sensitivity analysis for primary outcomes should randomisation be implied rather than been stated explicitly. This analysis was not necessary with the two included studies.

W H A T ' S   N E W

Last assessed as up-to-date: 13 November 2013.

Date	Event	Description
29 July 2014	New citation required but conclusions have not changed	Data from one new included study (Burns 2013) have not changed the overall conclusions of this review.
24 July 2014	Amended	TSC checked the references/studies, added CRS IDs.

(Continued)

2 December 2013	New search has been performed	Updated search of 2012. One additional paper identified, which met expanded inclusion criteria. Results of 2012 and 2013 search assessed and added to review

H I S T O R Y

Protocol first published: Issue 4, 2003

Review first published: Issue 3, 2005

Date	Event	Description
27 July 2012	New search has been performed	Update search of Cochrane Schizophrenia Group's Trial Register (see Search methods for identification of studies), 5 studies identified, none of which met our inclusion criteria
24 November 2010	New citation required but conclusions have not changed	Review layout changed substantially to reflect new updated Methods section
2 November 2009	New search has been performed	Results of new search 2008 added, no new studies added, conclusions not changed Risk of bias table and Summary of Findings table added.
22 October 2008	Amended	Converted to new review format.
1 May 2003	New citation required and major changes	First version underway.

C O N T R I B U T I O N S   O F   A U T H O R S

2012 and 2013 search

Steve Kisely (SK) - independently inspected citations from the new electronic search and identified relevant abstracts. SK also inspected full articles of the abstracts meeting inclusion criteria. SK carried out the reliability check of all citations from the new electronic search, updated results and discussion in light of new search.

Original review

Steve Kisely (SK) - formulated the review question, initially developed the search strategy, conducted the analysis, and wrote the first draft of the review. SK also wrote the first draft of the updated review.

Leslie Anne Campbell (LAC) - reviewed and provided comments on the search strategy and review, conducted the analysis. She also commented on the updated review.

DECLARATIONS OF INTEREST

None known.

SOURCES OF SUPPORT

Internal sources

- Health Outcomes Unit, Capital District Health Authority, Halifax, Canada.
- Dalhousie University, Halifax, Canada.
- Fremantle Hospital, Australia.
- University of Western Australia, Australia.
- University of Queensland, Australia.

Salary support of SK

External sources

- No sources of support supplied

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

1. Overall loss of credibility

We now think that the 35% cut-off was inadvisable and prefer 50%. However, this decision was taken only after seeing the data and leaves all subsequent analyses very prone to the inclusion of bias (see Potential biases in the review process). We also added a comparison of compulsory community treatment with supervised discharge.

2. Funnel plot

In our original protocol (Kisely 2004), we stated that data from all included studies would be entered into a funnel graph (trial effect against trial size) in an attempt to investigate the likelihood of overt publication bias (Egger 1997). Because there were never more than two studies for each outcome, we were unable to use this technique to investigate publication bias.

3. Updating of Methods and inclusion of 'Summary of findings' and 'Risk of bias' tables

In our protocol, we stated that we would only compare compulsory community treatment with standard voluntary care. However, the scarcity of RCTs in the assessment of compulsory community treatment meant that we subsequently extended inclusion criteria to studies that compared different types of compulsory treatment in the community.

In addition, we have added 'Risk of bias' and a 'Summary of findings' tables, as well as updated some of the methods and layout of text to reflect the updated structure of Cochrane reviews.

4. Outcome: Global state added

The new trial provided useable data from a Global State scale.

INDEX TERMS

Medical Subject Headings (MeSH)

Ambulatory Care [standards; statistics & numerical data]; Commitment of Mentally Ill [*legislation & jurisprudence]; Community Mental Health Services [*legislation & jurisprudence]; Crime Victims; Length of Stay [statistics & numerical data]; Mental Disorders [*therapy]; Patient Satisfaction; Randomized Controlled Trials as Topic; Treatment Outcome

MeSH check words

Humans