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REVIEW ARTICLE

Randomized and non-randomized evidence for the effect of compulsory community and involuntary out-patient treatment on health service use: systematic review and meta-analysis

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ABSTRACT

Background. There is limited randomized controlled trial (RCT) evidence for compulsory community treatment. Other study methods may clarify their effectiveness. We reviewed RCT and non-RCT evidence for the effect of compulsory community treatment on hospital admissions, bed-days, compliance and out-patient contacts.

Method. A systematic review of RCTs, controlled before-and-after (CBA) studies, and interrupted time series (ITS) analyses. Meta-analysis of RCTs.

Results. Eight papers covering five studies (two RCTs and three CBAs) met inclusion criteria (total $n=1108$). There was no statistical difference in 12-month admission rates between subjects on involuntary out-patient treatment and controls. Survival analyses of time to admission were equivocal. All five studies reported decreases in the number of bed-days following involuntary out-patient treatment but this only reached statistical significance in one situation; patients receiving the intervention were less likely to have admissions of over 100 days. There was no difference in treatment adherence between the intervention and control groups in either RCT or two of the CBA studies. However, the third CBA study reported a statistically significant increase of nearly five visits in the mean number of overall contacts in the involuntary out-patient treatment group.

Conclusions. The evidence for involuntary out-patient treatment in reducing either admissions or bed-days is very limited. It therefore cannot be seen as a less restrictive alternative to admission. Other effects are uncertain. Evaluation of a wide range of outcomes should be included if this type of legislation is introduced.

INTRODUCTION

Compulsory treatment in the community covers interventions under civil law such as Community Treatment Orders (CTOs), Mandatory Out-patient Treatment, Involuntary Out-patient Commitment (OPC) and Supervised Discharge. It does not refer to orders made under criminal law. Compulsory community treatment has been introduced in most jurisdictions in North America, as well as in Scotland, New Zealand, Australia, Israel, Norway, Switzerland, Portugal, Sweden and the Benelux countries (Salize & Dressing, 2002; Dawson,

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2005; Kisely *et al*. 2005*a*; Lawton-Smith, 2005). Non-Resident Orders for treatment in community are also under consideration in England and Wales (Lawton-Smith, 2005).

Studies indicating limited but improved outcomes in terms of readmission to hospital, length of stay, and treatment adherence have often not controlled for selection bias, variations in treatment and differing types or criteria for compulsory treatment in the community. For instance, involuntary out-patient treatment in many American states is court-ordered, while in Scotland, Australasia and Canada it is prescribed by a mental health professional (O'Reilly, 2001). Some studies did not include controls or, when they did, were unable to match on key variables.

Randomized controlled trials (RCTs) address many of these problems but are difficult to conduct where legislative mental health policy is implemented at a state, provincial or national level. We could only identify two RCTs for our Cochrane review (Kisely *et al*. 2005*a*), in which we concluded there was little evidence for any effect of involuntary out-patient treatment on health service use, costs or forensic contacts. However, there are several limitations to reliance on just RCTs in this area. Both RCTs that we reviewed were of court-ordered OPC in the USA and had small numbers of participants. Both RCTs explicitly excluded patients with a history of violence (Swartz *et al*. 1999; Swanson *et al*. 2000; Steadman *et al*. 2001), so limiting their applicability, as recent dangerousness, particularly violence against others, is often the reason for compulsory treatment in hospital or the community (Lansing *et al*. 1997).

In the absence of existing RCT evidence, we conducted a review of other methodologies that might provide clues to the effectiveness of a wider range of involuntary out-patient treatment when applied to a more representative patient group. Such studies include controlled before-and-after (CBA) studies and interrupted time series (ITS) designs. CBA studies incorporate a non-randomized control group; data are collected on the control and intervention groups before and after the introduction of an intervention. ITS designs are multiple observations over time that are 'interrupted' by an intervention or treatment. The effect is measured against the pre-intervention trend (EPOC, 2006).

Although there have been several reviews on the subject, these have been opinion pieces that lacked an explicit search strategy of the literature search with clear inclusion and exclusion criteria. This systematic review and meta-analysis was designed to minimize such reference and interpretation bias.

METHOD

Types of studies

In the absence of existing RCT evidence, a review of other study methodologies might provide clues as to the effectiveness of a wider range of compulsory community treatment when applied to a more representative patient group. Such studies would include controlled CBA and ITS designs. They could also include studies of routine administrative datasets that cover all patients placed on compulsory community treatment to minimize selection or follow-up bias (Bindman, 2002). Most studies on the efficacy of involuntary out-patient treatment have considered their effect on the number of subsequent hospital admissions, days spent in hospital and compliance with treatment such as out-patient visits or adherence to medication. We therefore selected these as the outcomes of interest.

Inclusion criteria

We conducted this review in accordance with guidelines of the Cochrane Effective Practice and Organisational Change (EPOC) group. The EPOC group suggests that non-randomized controlled clinical trials (CCTs), CBA studies and ITS analyses should be considered in the absence of randomized evidence (Bero *et al*. 1998; EPOC, 2006). We included studies of the following: CTOs, Involuntary Out-patient Treatment, Involuntary OPC, and Extended Leave or Supervised Discharge. We excluded compulsory treatment in the community for drug or alcohol dependence (de Miranda, 1989) and community treatments for mentally or behaviourally disordered offenders (Bailey, 2002).

Search strategy

We searched the following electronic databases up to June 2006: CINAHL, Embase, Medline, and PsycINFO. Our search strategy for Medline

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was the following: (exp Commitment of Mentally Ill/or jurisprudence/or exp mandatory programs/or (extended leave) or (community treatment order) or (involuntary outpatient treatment) or (involuntary outpatient commitment) or (supervised discharge)) and combined with the Cochrane Schizophrenia Group's phrase for randomized controlled trials or (interrupted and time and series) or (exp matched-pair analysis) or (controlled clinical trial) or (clinical trial). We also searched the register of the schizophrenia group of the Cochrane Collaboration. This contains randomized and non-randomized clinical trials located by electronic and hand searches of relevant journals and conference proceedings, as well as grey literature sources. Two reviewers collated and independently assessed abstracts. We searched for further trials by scrutinizing the reference lists of initial studies identified and other relevant review papers. We also contacted selected authors and experts.

Data extraction and quality assessment

Two reviewers (L.A.C. and A.S.) independently extracted data; disagreements were resolved by discussion and consultation with the third reviewer (S.K.). Papers were translated into English by a member of staff where required. We assessed methodological quality of studies according to the recommendations of the *Cochrane Collaboration Handbook* (Higgins & Green, 2005) and the EPOC Data Extraction Checklist (Bero *et al.* 1998; EPOC, 2006). EPOC quality criteria for RCTs involve consideration of the unit of allocation and analysis, concealment of allocation, follow-up rates, blinding, comparability of groups at baseline, reliability of outcome assessment and protection against contamination. For inclusion in the review, CBA studies had to have contemporaneous data collection and use appropriate control groups. We also assessed the comparability of intervention and control groups, similarity of baseline measurements in both groups, protection against contamination, follow-up rates and reliability of outcome assessment. For inclusion in the review, ITS studies had to include an intervention delivered at a defined point in time and report three or more data points before and after the intervention.

Statistical analysis

In accordance with EPOC guidelines (Bero *et al.* 1998; EPOC, 2006), we calculated the following: (1) absolute difference (mean or proportion of clinical behaviour in intervention/experimental group minus control); (2) relative percentage difference (absolute difference divided by post-intervention score in the control group); (3) absolute change from baseline (pre- to post-intervention changes in both groups); and (4) difference in absolute change from baseline. In studies without baseline data, only absolute difference and relative percentage differences were calculated. We assessed outcome in terms of any difference between the intervention and control groups in absolute change from baseline. This was expressed as the mean number of admissions, bed-days or treatment contacts as appropriate.

We did not mix randomized and non-randomized evidence, and only included RCTs in our meta-analysis. However, we undertook sensitivity analyses *post hoc* to examine any effect of including non-RCT designs in our meta-analysis, if data were available. We used Review Manager version 4.1 (Cochrane Collaboration, Oxford, UK), a statistical software package for managing and analysing a Cochrane Collaboration systematic review, for our analysis. We calculated odds ratios, and assessed heterogeneity by using the Q statistic. Any heterogeneity in the data was to be noted and cautiously explored by using previously identified characteristics of the studies, particularly assessments of methodological quality, diagnostic category and study length. We used a fixed effects model throughout as we found no significant heterogeneity in the majority of our analyses. We looked *post hoc* to see what difference, if any, using a random effects model would have made. As expected, confidence intervals were slightly wider but were broadly in line with the overall findings when using a fixed effects model.

RESULTS

Study inclusion and characteristics

We found 7356 citations, although this included duplicate entries for some papers. Of these, 106 papers were potentially relevant and subjected to strict quality and eligibility assessment. Of

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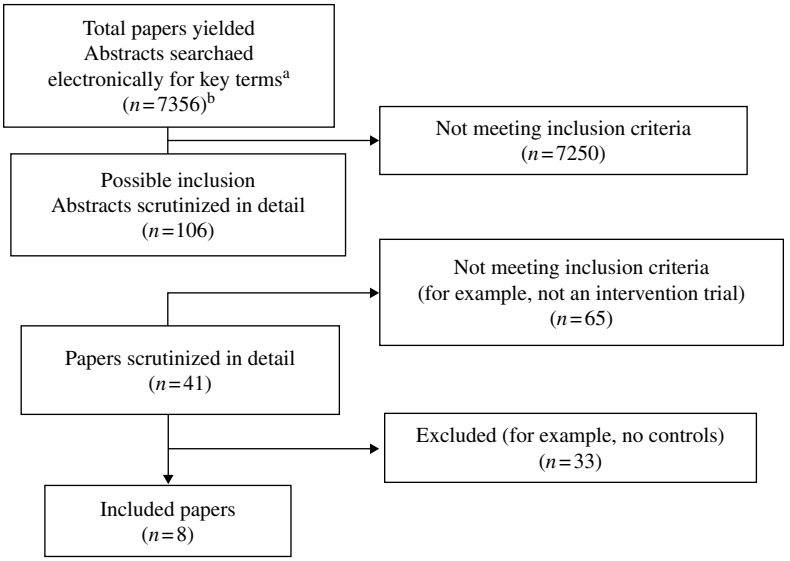

FIG. 1. Number of papers yielded by search strategy in systematic review. [a] Any mention of the following: community treatment order; involuntary out-patient treatment; involuntary out-patient commitment; extended leave; supervised discharge. [b] Included duplicate entries for some papers.

these, we excluded 65 because they did not meet our inclusion criteria (Fig. 1). We excluded a further 33 papers (Table 1) because they were duplicate publications or lacked relevant data. This left eight papers covering five studies (total $n=1108$). The mean age of participants was 39 years (S.D. = 11). Tables 2–4 show details of included studies. All papers reported similar follow-up periods of up to 12 months. As the papers reported different numbers of subjects for the various outcomes, we conducted an intention-to-treat (ITT) analysis in our RCT meta-analysis. Three of the studies were of court-ordered OPC in the USA (Geller *et al.* 1997; Swartz *et al.* 1999; Steadman *et al.* 2001). The other two were of CTOs in Australia (Preston *et al.* 2002; Kisely *et al.* 2005*b*). Two were RCTs (Swartz *et al.* 1999; Steadman *et al.* 2001), and the three others were a CBA design (Geller *et al.* 1997; Preston *et al.* 2002; Kisely *et al.* 2005*b*). Three of the papers described different aspects of a single RCT (Swartz *et al.* 1999, 2001; Wagner *et al.* 2003), and two papers a single CBA study (Preston *et al.* 2002; Kisely *et al.* 2004). We performed a meta-analysis only when both RCTs reported on the same outcome and provided sufficient information. This meant that we could only use meta-analysis for hospital admissions and bed-days. Findings from the three CBA studies were not entered into the main meta-analysis, although sensitivity analyses of any effect of their inclusion were undertaken.

Methodological quality

In terms of the two RCTs, a correct randomization method was described in the New York study (Steadman *et al.* 2001), but a description of the randomization method was not provided in the other (Swartz *et al.* 1999). Both were subject to selection bias as patients with a history of violence were explicitly excluded. The North Carolina papers also included a non-random *post hoc* analysis of the intervention group based on duration of involuntary OPC and follow-up of an additional non-randomized group of patients with a recent history of violence who were placed on OPC. We did not include these in our analysis as such analyses are subject to bias and confounding that randomized trials are designed to minimize (Hotopf *et al.* 1999).

The CBA studies did include patients with a history of violence (Geller *et al.* 1997; Preston *et al.* 2002). The Western Australian study included all patients placed on a CTO within an

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Table 1. *Characteristics of excluded studies*

Study	Reason for exclusion
Atkinson *et al.* (1999)	No controls and not an interrupted time series
Atkinson *et al.* (2002)	No controls and not an interrupted time series
Bar El *et al.* (1998)	No controls and not an interrupted time series
Borum *et al.* (1999)	No controls and not an interrupted time series
Bursten (1986)	Insufficient data
Canvin *et al.* (2002)	Qualitative study
Cavanaugh & Wasyliw (1985)	No controls and not an interrupted time series
Crisanti & Love (2001)	Cross-sectional study
Davies *et al.* (2001)	No controls and not an interrupted time series
Durst *et al.* (1999)	Retrospective survey
Fernandez & Nygard (1990)	No controls and not an interrupted time series
Frank *et al.* (2005)	No controls and not an interrupted time series
Geller *et al.* (1998)	Duplicate data
Hatfield *et al.* (2001)	Predictors of guardianship *versus* supervised discharge
Hiday & Scheid-Cook (1989)	Insufficient data
Hiday & Scheid-Cook (1991)	Insufficient data
Lidz (1998)	Review
Miller & Fiddleman (1984)	No controls and not an interrupted time series
Miller (1985)	Survey
Munetz *et al.* (1996)	No controls and not an interrupted time series
O'Brien & Farrell (2005)	No controls and not an interrupted time series
O'Keefe *et al.* (1997)	Retrospective chart review – no controls
O'Reilly *et al.* (2000)	Survey of psychiatrists
Pinfold *et al.* (1999)	Survey
Pinfold *et al.* (2001)	No controls and not an interrupted time series
Pinfold *et al.* (2002)	Survey of professional attitudes
Rohland *et al.* (2000)	No controls and not an interrupted time series
Segal (2005)	No information collected at baseline
Sensky *et al.* (1991*a*)	Retrospective case-note comparison
Sensky *et al.* (1991*b*)	Intervention was a hypothetical community treatment order (CTO)
Van Putten *et al.* (1988)	No controls and not an interrupted time series
Vaughan *et al.* (2000)	Insufficient data, matched controls differed from intervention group on key variables and analysis not adjusted for confounders
Zanni & DeVeau (1986)	No controls and not an interrupted time series

entire jurisdiction (Preston *et al.* 2002). The paper also presented baseline characteristics of intervention and control groups, and used matching or multivariate analyses to adjust for potential confounders. A further paper extended the study by conducting a survival analysis of time to admission, which included controlling for forensic history from the Offenders' database of Western Australia (Kisely *et al.* 2004). However, matching within a jurisdiction is very difficult to achieve. There may still be some reason why patients were placed on CTOs while the controls were not. These might include social disability, medication type including the use of depot preparations, and characteristics of the primary clinician, treating team or service.

To address the issue of matching within a jurisdiction, the third CBA study (Kisely *et al.* 2005*b*) compared two jurisdictions, one with CTOs (Western Australia), the other without (Nova Scotia), to evaluate their effect on hospital admission rates and lengths of stay. Although they were in different countries, the two jurisdictions had similar health services. In both, the delivery of mental health services was free at the point of delivery, and services have similar characteristics in terms of staffing, as well as the balance of in-patient and out-patient care (Kisely *et al.* 2005*b*). Importantly, neither had jurisdiction-wide assertive community treatment that could act as a confounding variable in assessing health service use. The authors also matched or controlled for most patient characteristics associated with CTO placement that could act as confounders. These included age, gender, diagnosis, rural *versus* metropolitan residence, prior psychiatric service use (admission rates, bed-days and out-patient contacts), and psychiatric co-morbidity including substance use and personality disorder (Kisely *et al.* 2005*b*). However, despite the similarities in psychiatric workforce, bed provision and

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Table 2. *Psychiatric admissions*

Study	Type	*n*	Outcome	Results
Swartz *et al.* (1999)[a]	RCT	Int = 129 Cont = 135	At least one admission at 12 months	Int = 56/129 (43·4%) Cont = 66/135 (48·9%) Absolute difference = −5·5 Relative difference = −11·2%
Steadman *et al.* (2001)	RCT	Int = 85 Cont = 67	At least one admission at 11 months	Int = 40/85 (47·1%) Cont = 27/67 (40·3%) Absolute difference = 6·8 Relative difference = 16·9%
Geller *et al.* (1997)	CBA	Int = 20 Cont = 20	Mean number psychiatric admissions at 1 year	Int Pre μ = 1·6 (S.D. = 1·14) Cont Pre μ = 1·6 (S.D. = 1·1) Int μ = 1·2 (S.D. not provided) Cont μ = 0·75 (S.D. not provided) Absolute difference = 0·45 Relative difference = 60·0% Absolute change from BL: Int = –0·4, Cont = –0·85 Difference in absolute change from BL = 0·45
Preston *et al.* (2002)[b]	CBA	Int = 228 Cont = 228	Mean number psychiatric admissions at 1 year At least one admission at 12 months[d]	Int Pre μ = 1·69 (S.D. = 1·70) Cont Pre μ = 1·81 (S.D. = 1·61) Int μ = 1·21 (S.D. = 1·63) Cont μ = 1·13 (S.D. = 1·84) Absolute difference = 0·08 Relative difference = 7·1% Absolute change from BL: Int = −0·48, Cont = −0·68 Difference in absolute change from BL = 0·2
Kisely *et al.* (2005*b*)	CBA	Int = 196[c] Cont = 196	Mean number psychiatric admissions at 1 year	Int Pre μ = 1·69 (S.D. = 1·59) Cont Pre μ = 1·54 (S.D. = 1·29) Int μ = 1·15 (S.D. = 1·59) Cont μ = 0·90 (S.D. = 1·58) Absolute difference = 0·25 Relative difference = 27·7% Absolute change from BL: Int = −0·54, Cont = −0·64 Difference in absolute change from BL = 0·10

RCT, Randomized controlled trial; CBA, controlled before-and-after; Int, intervention group; Cont, control group; BL, baseline; Pre, pre-intervention; μ, mean; S.D., standard deviation.

[a] Reporting one of several outcomes from the North Carolina RCT.

[b] Kisely *et al.* (2004) reported a survival analysis to admission of the same dataset.

[c] Subset of the intervention sample in Preston *et al.* (2002).

[d] Int = 124/228 (54·4%); Cont = 112/228 (49·1%).

patient characteristics between the two jurisdictions, there remains the possibility that the results were confounded by differences of which the authors were unaware, or for which they failed to match or control.

Effect of CTOs on admission rates

There was no statistically significant reduction in the readmission rate for subjects on OPC compared to the control groups at the 11–12 months follow-up. This finding was not sensitive to the type of model used, with the fixed effect model estimating the relative risk to be 0·98 (0·8–1·2), and the random effects model 0·99 (0·8–1·3). This finding was also mirrored in the three CBA studies, where the effect on admissions was small and not significant (Table 2). Two CBA studies also compared the risk of, and time to, admission using survival analyses such as Cox regression (Kisely *et al.* 2004, 2005*b*). Patients on compulsory community treatment had a shorter time to admission compared to matched controls. One study also compared compulsory community treatment with consecutive controls and found no significant

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Table 3. *Psychiatric bed-days*

Study	Type	*n*	Outcome	Results
Swartz *et al.* (1999)[a]	RCT	Int = 129 Cont = 135	Mean number psychiatric bed-days at 1 year	Int μ = 26·68 (S.D. = 63·4) Cont μ = 27·92 (S.D. = 51·05) Absolute difference = −1·24 Relative difference = −4·4%
Steadman *et al.* (2001)	RCT	Int = 78[b] Cont = 64	Mean number psychiatric bed-days at 11 months	Int μ = 76·20 (S.D. = 77·3) Cont μ = 126·10 (S.D. = 110·10) Absolute difference = −49·90 Relative difference = −39·6%
Geller *et al.* (1997)	CBA	Int = 20 Cont = 20	Mean number psychiatric bed-days at 1 year	Int Pre μ = 122·8 (S.D. = 52·9) Cont Pre μ = 107·2 (S.D. = 62·4) Int μ = 112·6 (S.D. not provided) Cont μ = 123·4 (S.D. not provided) Absolute difference = −10·8 Relative difference = −8·8% Absolute change from BL: Int = −10·2, Cont = 16·2 Difference in absolute change from BL = −26·4
Preston *et al.* (2002)	CBA	Int = 228 Cont = 228	Mean number psychiatric bed-days at 1 year	Int Pre μ = 51·39 (S.D. = 45·61) Cont Pre μ = 48·53 (S.D. = 44·78) Int μ = 24·93 (S.D. = 38·95) Cont μ = 26·95 (S.D. = 59·35) Absolute difference = −2·02 Relative difference = −7·5% Absolute change from BL: Int = −26·46, Cont = −21·58 Difference in absolute change from BL = −4·88
Kisely *et al.* (2005*b*)	CBA	Int = 196[c] Cont = 196	Mean number psychiatric bed-days at 1 year	Int Pre μ = 58·10 (S.D. = 49·98) Cont Pre μ = 62·76 (S.D. = 63·20) Int μ = 22·66 (S.D. = 38·49) Cont μ = 33·72 (S.D. = 59·35) Absolute difference = −11·06 Relative difference = −32·8% Absolute change from BL: Int = −35·44, Cont = −29·04 Difference in absolute change from BL = –6·40

RCT, Randomized controlled trial; CBA, controlled before-and-after; Int, intervention group; Cont, control group; BL, baseline; Pre, pre-intervention; μ, mean; S.D., standard deviation.

[a] Reporting one of several outcomes from the North Carolina RCT.

[b] Data available on *n* = 142.

[c] Subset of the intervention sample in Preston *et al.* (2002).

differences between the two groups (Kisely *et al.* 2004).

Psychiatric bed-days

All five studies reported decreases in the number of bed-days following compulsory community treatment (Table 3). In the case of the two RCTs, when subjects on OPC were compared to the control groups, the weighted mean difference was −23·2 days (−70·7 to 24·2) using the random effects model, and −7·63 days (−19·2 to 3·9) using the fixed effects model. In neither model did this reduction reach statistical significance. In the case of the three CBA studies, the mean reduction in bed-days ranged from 5 to 26 days (Table 3). However, differences only reached statistical significance in one study where a threshold effect was reported; patients on compulsory community treatment were less likely to have long admissions of over 100 days (Kisely *et al.* 2005*b*).

Treatment/contact adherence

In accordance with EPOC guidelines, we combined RCT data on treatment adherence. This showed there was no difference in adherence to

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Table 4. *Psychiatric treatment/contact adherence*

Study	Type	*n*	Outcome	Results
Swartz *et al.* (2001)[a]	RCT	Int = 129 Cont = 135	Proportion adherent to treatment at 1 year	Int = 54/129 (49·1%) Cont = 55/135 (40·7%) Absolute difference = 8·4 Relative difference = 20·6%
Steadman *et al.* (2001)	RCT	Int = 85 Cont = 67	Self-reported treatment compliance at 11 months	Int = 47/85 (55·3%) Cont = 34/67 (56·7%) Absolute difference = −1·4 Relative difference = −2·5%
Wagner *et al.* (2003)[a]	RCT	Int = 129 Cont = 135	Out-patient service use (all services) at 1 year	Int μ = 6·30 (S.D. = 10·2) Cont μ = 5·75 (S.D. = 9·2) Absolute difference = 0·55 Relative difference = 9·6%
Preston *et al.* (2002)	CBA	Int = 228 Cont = 228	Mean number psychiatric contacts at 1 year	Int Pre μ = 59·60 (S.D. = 79·23) Cont Pre μ = 33·30 (S.D. = 47·83) Int μ = 73·28 (S.D. = 89·15) Cont μ = 42·39 (S.D. = 64·09) Absolute difference = 30·89 Relative difference = 72·9% Absolute change from BL: Int = 13·68, Cont = 9·09 Difference in absolute change from BL = 4·59

RCT, Randomized controlled trial; CBA, controlled before-and-after; Int, intervention group; Cont, control group; BL, baseline; Pre, pre-intervention; μ, mean; S.D., standard deviation.

[a] Reporting different outcomes of the North Carolina RCT.

treatment between the two groups irrespective of the type of model used. The fixed effect model estimated the relative risk to be 0·99 (0·8–1·2), while the random effects model estimated it to be 0·98 (0·8–1·2). In terms of out-patient contacts, one of the CBA studies reported an increase in the mean number of overall contacts in the CTO group of nearly five visits (Table 4), this reaching statistical significance (Preston *et al.* 2002). However, no statistical difference was found in the RCT that measured overall out-patient contacts, although the authors reported a statistically significant increase in visits to psychiatrists (Wagner *et al.* 2003).

Heterogeneity

Although the number of studies that reported any given outcome was small, we calculated formal tests of homogeneity. We found no statistically significant heterogeneity in our analyses of admission rates ($\chi^2 = 1{\cdot}41$, df = 1, $p = 0{\cdot}23$) or treatment adherence ($\chi^2 = 0{\cdot}17$, df = 1, $p = 0{\cdot}68$). However, we did find statistically significant heterogeneity in our analysis of bed-days ($\chi^2 = 5{\cdot}75$, df = 1, $p = 0{\cdot}02$), meaning that the results for lengths of stay should be interpreted with caution.

Sensitivity analysis

Because of overlapping or missing data, we were only able to assess the effect of including one CBA study (Preston *et al.* 2002) in our meta-analyses of admissions and bed-days. The sensitivity analysis showed that this made no difference to the results.

Publication bias

We took no formal steps to look for publication bias, such as plotting effect sizes or calculating test statistics, because any formal method would have had little power given the small number of studies.

DISCUSSION

Strengths and limitations of this review

There have been several narrative reviews of compulsory community treatment but these have lacked clearly defined search strategies and transparent analyses, and are subject to interpretation bias. They also did not consider papers from outside the English-speaking world. That is why we undertook a systematic review of the worldwide literature with

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clearly defined search strategies and transparent analyses.

Although RCTs remain the least biased method of evaluating effects of all types of intervention, there are certain situations where they might be inappropriate, difficult or impossible to conduct (Gilbody & Whitty, 2002). It is for this reason that the Cochrane EPOC group accepts the use of other methodologies, such as CBA studies (Bero *et al.* 1998; EPOC, 2006). This paper extends our previous Cochrane review of RCTs of the effectiveness of compulsory community treatment (Kisely *et al.* 2005*a*), which has been criticized for ignoring other possible sources of evidence. We also extended our previous review by considering out-patient contacts.

Because of the difficulties of conducting RCTs in this area, our previous Cochrane review only identified two relatively small RCTs, restricted to court-ordered OPC in the USA. This may limit their generalizability to other jurisdictions where treatment is initiated by a clinician. Involvement of a court in making the order may, in itself, effect compliance with treatment. Both RCTs also explicitly excluded patients with a history of violence.

By including other study designs, we were able to increase the number of subjects from the 414 in our previous review of RCTs to a total of 1108. These extra subjects came from three CBA studies, one of which (Geller *et al.* 1997) was a small study ($n=40$) of court ordered out-patient treatment in the USA. The other two studies were larger and more generalizable to other jurisdictions, as they were of CTOs in Western Australia (Preston *et al.* 2002; Kisely *et al.* 2005*b*). These orders are made by a clinician rather than a judge and are used in Australia, Canada and New Zealand. They are also similar to the type of compulsory community treatment introduced in Scotland and proposed for England and Wales. The other strength of these CBA studies was the inclusion of patients with a past history of dangerousness, so making them more relevant to patients who might be made subject to such an order.

We are aware of a preliminary report of a large controlled study from Australia using the Victorian Case Register, assessing the impact of early-intervention compulsory community treatment in 8979 subjects (Segal, 2005). The intervention was defined as orders following the patients' first hospitalization and/or within 90 days of entry into the mental health system. These subjects were compared to an equal number of matched controls. Early intervention was associated with reduced subsequent health service use. However, we were unable to include these data in our analysis because no information was available on baseline health service use, and so could not be considered a CBA study (Table 1). In addition, the promising findings of this study may have limited generalizability. In jurisdictions such as Canada, compulsory community treatment is restricted by legislation to relatively chronic patients who have had a minimum number of admissions or hospital days in the 24–36 months prior to placement on an order (Gray & O'Reilly, 2001). Similarly, Non-Resident Orders proposed for England focus on patients who frequently relapse, rather than on early intervention.

We did not identify a single ITS analysis, although this would have been a very appropriate design to assess the effectiveness of compulsory community treatment. Neither were we able to include any study from outside the English-speaking world despite our comprehensive search strategy.

Comparison with other work

Even with more studies, our review still does not show strong evidence for the effectiveness of compulsory community treatment. This mirrors the results of our smaller Cochrane review restricted to RCTs (Kisely *et al.* 2005*a*). There were few statistically significant differences between intervention and control groups. Hospital stays over a certain threshold (100 days) were less likely in patients on compulsory community treatment in one CBA study (Kisely *et al.* 2005*b*), and there was an increase of five contacts per year in the average number of out-patient visits in another (Preston *et al.* 2002). This was not confirmed in the two RCTs of court-ordered OPC. However, out-patient contact can be criticized as an outcome measure as it relates to the process of the intervention itself. If patients are compelled to attend out-patient appointments, it is not surprising that out-patient contacts will increase. Data on time to readmission were equivocal (Kisely *et al.* 2004, 2005*b*).

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Our cautious interpretation of the evidence differs from the rather more enthusiastic endorsement of compulsory community treatment of two recent narrative reviews (Swartz & Swanson, 2004; Dawson, 2005). Neither was a systematic review, but a subjective interpretation of data from a wide range of papers including naturalistic uncontrolled studies. The methodological quality was not assessed using explicit guidelines, and data were not extracted or reported in a standardized way. Conflicting interpretations are not new to this literature. In fact, given the limited number of relative studies, it is striking how reviews of the same studies can come to markedly different conclusions (O'Reilly, 2001; Ridgely *et al.* 2001; Bindman, 2002; Swartz & Swanson, 2004; Dawson, 2005). This discrepancy further illustrates the importance of applying standardized methodologies to reviews that include both RCT and non-RCT designs to minimize interpretation bias.

Study implications

Use of compulsory community treatment is becoming more widespread. Involuntary OPC is permitted in most of the USA, and conditional leave or CTOs have also been introduced in Canada, Scotland, New Zealand, Australia, Israel, Norway, Switzerland, Portugal, Sweden and the Benelux countries (Salize & Dressing, 2002; Dawson, 2005; Lawton-Smith, 2005). An initial argument for the introduction of this intervention was that it was less coercive than the alternatives of admission to hospital or arrest. However, research findings have not confirmed that this intervention can reduce either (Kisely *et al.* 2005*a*).

There have been three contradictory responses to this lack of evidence. One has been to undertake *post hoc* analyses of non-random samples whose orders have been extended over sustained periods (>180 days) to demonstrate a reduced admission rate compared to controls. However, such analyses are subject to bias and confounding that randomized trials are designed to minimize. Analysis of subjects who have not been randomly assigned to OPC groups of less than, and more than, 180 days may reflect a bias, where OPC was selectively extended when it seemed to be helping the patient (Szmukler & Hotopf, 2001).

The second is to argue that we should in fact expect compulsory community treatment to increase readmission rates through increased surveillance. Earlier and more frequent admission could then lead to better outcomes in terms of reduced lengths of stay. We found this pattern in only one of the CBA studies we reviewed, where survival analyses showed a shorter time to, and risk of, admission in association with reduced bed-days in some circumstances (Kisely *et al.* 2005*b*). However, this finding was limited to hospital stays over a certain threshold (Kisely *et al.* 2005*b*), and in comparison with controls from a different country. Despite the similarities in psychiatric workforce and bed provision between the two jurisdictions, there always remains the possibility that any difference between the CTO cases and controls was due to differences between jurisdictions other than the existence of CTO legislation. Importantly, this was not found in either of the RCTs or the other CBAs.

The third is to change tack completely and argue that admission rates are not a relevant outcome and that other indicators are more appropriate. Unfortunately, the level of evidence on patient outcomes such as symptomatology, homelessness or quality of life is equally limited (Kisely *et al.* 2005*a*).

A recent paper asked why this type of intervention is so controversial (O'Reilly, 2004). A more appropriate question is why compulsory community treatment is so widespread given the limited evidence. One way to understand this phenomenon is by dividing policymaking into three categories (King, 1998): (1) rationalist-objectivist approaches, where decisions are determined by evidence-based practice; (2) argumentative-subjectivist approaches, where decisions are determined by a dialogue between government, the professions, lobbyists, media and the criminal justice system; (3) integrated-learning, which combines the rationalist-objectivist models for policy evaluation with an argumentative-subjectivist analysis. We would suggest that argumentative-subjectivist approaches have prevailed to date, and that it is time to give a greater emphasis to integrated-learning.

Given the difficulties of conducting RCTs in this area, it is unlikely that other studies will be attempted. Further evaluation will depend on

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quasi-experimental designs, with the analyses of routine databases as one way of minimizing bias (Bindman, 2002). ITS analyses would be particularly appropriate given the difficulties of finding suitable controls. Another option would be the comparison of similar jurisdictions in the same country, before and after legislation in one, with the other jurisdiction acting as the control.

DECLARATION OF INTEREST

None.

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