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Review

Systematic reviews of the effectiveness of day care for people with severe mental disorders: (1) Acute day hospital versus admission; (2) Vocational rehabilitation; (3) Day hospital versus outpatient care

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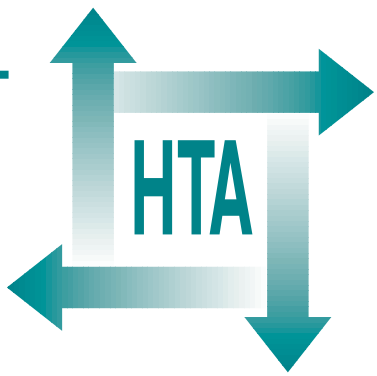
Health Technology Assessment
NHS R&D HTA Programme



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Systematic reviews of the effectiveness of day care for people with severe mental disorders: (1) Acute day hospital versus admission; (2) Vocational rehabilitation; (3) Day hospital versus outpatient care

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The review on vocational rehabilitation has been published: Crowther RE, Marshall M, Bond GR, Huxley P. Helping people with severe mental illness to obtain work: systematic review. *BMJ* 2001;**322**:204–8. It has also been accepted by the Cochrane Library.

NHS R&D HTA Programme

The NHS R&D Health Technology Assessment (HTA) Programme was set up in 1993 to ensure that high-quality research information on the costs, effectiveness and broader impact of health technologies is produced in the most efficient way for those who use, manage and provide care in the NHS.

Initially, six HTA panels (pharmaceuticals, acute sector, primary and community care, diagnostics and imaging, population screening, methodology) helped to set the research priorities for the HTA Programme. However, during the past few years there have been a number of changes in and around NHS R&D, such as the establishment of the National Institute for Clinical Excellence (NICE) and the creation of three new research programmes: Service Delivery and Organisation (SDO); New and Emerging Applications of Technology (NEAT); and the Methodology Programme.

This has meant that the HTA panels can now focus more explicitly on health technologies ('health technologies' are broadly defined to include all interventions used to promote health, prevent and treat disease, and improve rehabilitation and long-term care) rather than settings of care. Therefore the panel structure has been redefined and replaced by three new panels: Pharmaceuticals; Therapeutic Procedures (including devices and operations); and Diagnostic Technologies and Screening.

The HTA Programme will continue to commission both primary and secondary research. The HTA Commissioning Board, supported by the National Coordinating Centre for Health Technology Assessment (NCCHTA), will consider and advise the Programme Director on the best research projects to pursue in order to address the research priorities identified by the three HTA panels.

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The views expressed in this publication are those of the authors and not necessarily those of the HTA Programme or the Department of Health. The editors wish to emphasise that funding and publication of this research by the NHS should not be taken as implicit support for any recommendations made by the authors.

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Reviews in *Health Technology Assessment* are termed 'systematic' when the account of the search, appraisal and synthesis methods (to minimise biases and random errors) would, in theory, permit the replication of the review by others.

The editors and publisher have tried to ensure the accuracy of this report but do not accept liability for damages or losses arising from material published in this report. They would like to thank the referees for their constructive comments on the draft document.

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Contents

Overall summary and introduction

Overall summary

This report contains systematic reviews of: acute day hospital care versus admission to hospital; vocational rehabilitation (VR) versus standard care (without VR); and day hospital care versus outpatient care.

Acute day hospital versus admission

This review assessed the effectiveness of day hospital care versus inpatient care for people with acute psychiatric disorders. Nine randomised controlled trials (RCTs) were identified (1568 patients) and individual patient data were obtained for four (594 patients). Day hospital treatment was feasible for between 23.2% and 37.5% of those admitted to inpatient care. There was no difference in the number of days in hospital (combining day hospital and inpatient days) between day hospital patients and control patients (weighted mean difference (WMD) = –0.38 days/month; 95% confidence interval (CI), –1.32 to 0.55). However, day hospital patients spent more days in day hospital care (WMD = 2.34 days/month; 95% CI, 1.97 to 2.70) and fewer days in inpatient care (WMD = –2.75 days/month; 95% CI, –3.63 to –1.87). Readmission rates were similar for day patients and control groups (relative risk (RR) 0.91; 95% CI, 0.72 to 1.15). Day patients showed a significantly faster improvement in mental state ($n = 407$; $\chi^2 = 9.66$; $p = 0.002$), but not in social functioning ($n = 295$; $\chi^2 = 0.006$; $p = 0.941$). Day hospital care was reported to be cheaper than inpatient care (cost reductions ranging from 20.9% to 36.9%). It was concluded that acute day hospitals can be an attractive alternative when demand for inpatient care is high.

Vocational rehabilitation versus standard care

Prevocational training (PVT) aims to help people with severe mental disorders to obtain work by offering a period of preparation before entering employment. In contrast, Supported Employment (SEm) places people with mental disorders in competitive employment without preparation. The main objective of the review was to assess the effectiveness of PVT and SEm against each other and against standard care. Eighteen RCTs meeting the inclusion criteria were identified. SEm was significantly more effective than PVT in terms of numbers in competitive employment (e.g. at 18 months: SEm 35% employed,: PVT 12% employed; RR = 0.76; 95% CI, 0.69 to 0.84; number-needed-to-treat = 4.45). SEm clients earned more and worked more hours than those in PVT. There was no evidence that PVT was more effective than standard community care in helping clients to obtain employment. It was concluded that people with mental disorders who want to work should be offered the option of SEm.

Day hospital versus outpatient care

Two types of day hospital were covered: "day treatment programmes" and "day care centres". Day treatment programmes are used to enhance the treatment of patients with anxiety/depressive disorders who have failed to respond to outpatient care. Day care centres offer structured support to patients with long-term severe mental disorders. Evidence from two trials suggested that day treatment programmes were superior to outpatient care in improving psychiatric symptoms, but were no better or worse than outpatient care on any other outcome variable (including costs). There was no evidence that day care centres were better or worse than outpatient care on any outcome variable; some data suggested that they could be more expensive. It was concluded that future research should address the cost-effectiveness of day treatment programmes against other alternatives for patients with disorders that are refractory to treatment.

Introduction

Since the 1950s, day care, in some shape or form, has been a key element of most modern psychiatric services.[1] In the USA, the heyday of day care services arose in the early 1970s after the Community Mental Health Act 1963, which dictated that all psychiatric services must provide day care facilities. US developments stimulated similar day care provision in other western democracies, including the UK.

A major problem for researchers and practitioners of day care has been the difficulty in finding a clear and consistent terminology.[2] Consequently, any discussion of day care must begin with an

attempt to define the different types of programmes and to clarify their function. In this report we have classified day care programmes by using the dimensions of programme function and programme structure.

Four main functions can be identified for day care:

- to act as an alternative to admission for patients with acute disorders
- to shorten the duration of admission for patients with acute disorders (transitional care)
- to rehabilitate and maintain patients with long-term disorders
- to enhance treatment in patients (usually with anxiety or depressive disorders) who have not responded to outpatient care.

Three main types of day care structure can be identified:

- day hospitals
- employment programmes
- informal programmes.

For the purposes of this review, day hospitals are defined as multidisciplinary day care facilities offering comprehensive psychiatric care, where: "multidisciplinary" means involving, as a minimum, psychiatrists and nurses; "day care facility" means a building open during working hours on weekdays, although extended and weekend opening is permissible; and "comprehensive psychiatric care" means the diagnostic, medical, psychiatric, psychosocial and occupational treatments that would normally be available to psychiatric inpatients. Employment programmes are directed towards helping patients to obtain work but they do not offer comprehensive psychiatric care and are not necessarily associated with a particular building. Informal programmes are day care facilities that offer a place where patients can meet for support, companionship or daytime activities, but which do not provide comprehensive psychiatric care or specialised employment services.

Table 1 shows how day care may be classified according to the dimensions of programme structure and function. The words in italics indicate the terminology used in this report to refer to particular combinations of programme structure and function. For example, the term "acute day hospital" refers to a day hospital (programme structure 1) used as an alternative to admission (programme function A). Wherever possible the terminology used to refer to combinations of structure and function follows convention in the relevant literature (see the individual reviews for more detailed discussion). Crosses in *Table 1* indicate combinations of programme structure and function that are rarely, if ever, encountered in modern psychiatric services. Not surprisingly, in our literature searches we did not encounter any trials, randomised or otherwise, that fell into any of the "crossed" combinations.

Our original proposal was to carry out four systematic reviews based on the classification summarised in *Table 1*. These reviews were:

1. acute day hospitals as an alternative to admission (combination *1A*)
2. transitional day hospitals as a means of reducing duration of admission (combination *1B*)
3. day care centres, VR programmes and drop-in centres as providers of rehabilitation/maintenance care (combinations *1C*, *2C*, *3C*)
4. day treatment programmes as an alternative to outpatient care (combination *1D*).

We made four modifications to this original plan. First we abandoned review 2 because it was pre-empted by the publication of a satisfactory Cochrane review covering all of the key research in this area.[3] Secondly, we restricted review 3 to VR programmes alone, because it became clear that the complexity of VR research and its focus on employment outcomes meant that it did not sit easily with research on day care centres. Thirdly, we modified review 4 so that it covered both day care centres and day treatment programmes. Finally, we decided

TABLE 1 *Classification of day care*

Programme structure	Programme function			
	A: Alternative to admission	**B: Shortening admission**	**C: Rehabilitation or maintenance**	**D: Enhancing outpatient treatment**
1: Day hospital	*1A: Acute day hospital*	*1B: Transitional day hospital*	*1C: Day care centre*	*1D: Day treatment programme*
2: Employment programme	*2A:* ✗	*2B:* ✗	*2C: VR programme*	*2D:* ✗
3: Informal programme	*3A:* ✗	*3B:* ✗	*3C: Drop-in centre*	*3D:* ✗

not to review the use of drop-in centres because we detected no relevant research in this area.

In summary, therefore, this report consists of three systematic reviews:

1. acute day hospital versus admission for acute psychiatric disorders
2. vocational rehabilitation for people with severe mental disorders
3. day hospital versus outpatient care for patients with psychiatric disorders (containing reviews of the effectiveness of day treatment programmes and day care centres).

Together with the Cochrane review cited above,[3] we believe that this report provides a comprehensive overview of the evidence for the effectiveness of the various forms of day care for people with psychiatric disorders.

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Chapter 1

Acute day hospital versus admission for acute psychiatric disorders

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List of abbreviations

BPRS	Brief Psychiatric Rating Scale*
CIS	Clinical Interview Schedule*
CPN	community psychiatric nurse*
CPRS	Comprehensive Psychopathology Rating Scale*
DH	day hospital*
DP	day patient*
CI	confidence interval
IP	inpatient*
IPD	individual patient data*
ITT	intention-to-treat*
NAppl	not applicable*
NK	not known*
NNT	number-needed-to-treat
PSE	Present State Examination*
RR	relative risk
SAS	Social Adjustment Scale*
SBAS	Social Behaviour Assessment Scale*
SD	standard deviation
SE	standard error*
WMD	weighted mean difference

* Used only in tables and figures

Executive summary

Background

Inpatient treatment is an expensive way of caring for people with acute psychiatric disorders. It has been proposed that many of those currently treated as inpatients could be cared for in acute psychiatric day hospitals.

Objective

The aim of this review was to assess the effectiveness and feasibility of day hospital versus inpatient care for people with acute psychiatric disorders.

Methods

Study selection

Eligible studies were randomised controlled trials of day hospital versus inpatient care for people with acute psychiatric disorders. Studies were excluded if they were primarily concerned with elderly people, children, or patients with a diagnosis of organic brain disease or substance abuse.

Data sources

We searched the Cochrane Controlled Trials Register, MEDLINE, EMBASE, CINAHL, PsycLIT, and the reference lists of articles. Researchers were approached to identify unpublished studies. Trialists were asked to provide individual patient data.

Data extraction

Data were extracted independently by two reviewers and cross-checked.

Data synthesis

Relative risk (RR) and 95% confidence intervals (CI) were calculated for dichotomous data. Weighted or standardised means were calculated for continuous data. Day hospital trials tend to present similar outcomes in slightly different formats, making it difficult to synthesise the data. Individual patient data were therefore sought so that outcomes could be re-analysed using a common format.

Results

Nine trials met the inclusion criteria (involving 1568 randomised patients and 2268 assessed for suitability of day hospital treatment). Individual patient data were obtained for four trials (involving 594 people). A sensitivity analysis of combined data suggested that day hospital treatment was feasible for at worst 23.2% (n = 2268; 95% CI, 21.2 to 25.2) and at best 37.5% (n = 1768; 95% CI, 35.2 to 39.8) of those currently admitted to inpatient care. Individual patient data from three trials showed no difference in the number of days in hospital (combining day hospital days and inpatient days) between day hospital patients and controls (n = 465; weighted mean difference (WMD) = –0.38 days/month; 95% CI, –1.32 to 0.55). However, compared with controls, patients randomised to day hospital care spent significantly more days in day hospital care (n = 265; WMD = 2.34 days/month; 95% CI, 1.97 to 2.70) and significantly fewer days in inpatient care (n = 265; WMD = –2.75 days/month; 95% CI, –3.63 to –1.87). There was no difference between readmission rates for day hospital and control patients (n = 667; RR = 0.91; 95% CI, 0.72 to 1.15). Individual patient data from three trials showed a significant time–treatment interaction, indicating a more rapid improvement in mental state (n = 407; χ^2 = 9.66; p = 0.002), but not social functioning (n = 295; χ^2 = 0.006; p = 0.941) amongst day hospital patients. Four of five trials demonstrated that day hospital care was cheaper than inpatient care (with overall cost reductions ranging from 20.9% to 36.9%).

Conclusions

Acute day hospitals are an attractive option in situations where demand for inpatient care is high and facilities exist that are suitable for conversion. They are a less attractive option when demand for inpatient care is low and where effective alternatives already exist. The interpretation of day hospital research would be enhanced if future trials made use of the common set of outcome measures used in this review. It is important to examine how acute day hospital care can be most effectively integrated into a modern community-based psychiatric service.

Acute day hospital versus admission for acute psychiatric disorders

Background

Despite the growth of community care, many people with acute psychiatric disorders continue to be treated as inpatients.[1] This is an expensive way of caring for such patients[2] and surveys suggest that it is often unnecessary.[3] It has been proposed that many of those currently treated as inpatients could instead be treated in day hospitals.[4]

The acute day hospital has been defined as a day hospital that provides "diagnostic and treatment services for acutely ill patients who would otherwise be treated on traditional psychiatric inpatient units".[5] The acute psychiatric day hospital is to be distinguished from other types of "partial hospitalisation" or "day care" such as: transitional care for patients leaving hospital, more intensive alternatives to outpatient care (day treatment programmes), and support of long-term patients living in the community (day care centres).[5,6]

Psychiatric day hospitals were first described in the Soviet Union in the 1930s, where they arose as a result of bed shortages.[7] The first North American day hospital was opened in Montreal, Quebec, in 1946, also in an attempt to reduce the demand for inpatient beds.[8] In the USA, day hospitals became a popular treatment modality in the 1960s after the 1963 Community Mental Health Center Construction Act, which mandated the setting up of partial hospitalization programmes.[4] Similar developments encouraged the growth of day hospitals in the UK in the 1960s, and in the Netherlands and West Germany in the 1970s.[9] In the 1980s, however, research commissioned by the American Psychiatric Association showed widespread closure of partial hospitalisation programmes and a low rate of growth in the numbers of patients served by them.[10] A number of factors appeared to have contributed to this decline. First, there was a growing awareness of the limited evidence for the effectiveness and cost-effectiveness of day hospitals.[11,12] Secondly, day hospitals faced competition from more radical "non-institutional" alternatives such as assertive community treatment.[6] Thirdly, confusion over the role of day hospitals led to some becoming expensive "day centres", as they became overwhelmed by inappropriately placed long-term patients.[13] Despite these problems, remorseless pressure on inpatient facilities has led to continued interest in acute day hospitals and has inspired the development of new-style day hospitals augmented by outreach services, "crisis beds", and extended hours programmes.[14–17]

Despite 50 years of research, opinion remains divided on the cost-effectiveness of day hospital treatment. Proponents have claimed that it can provide more cost-effective care by promoting quicker recovery,[8] improving social functioning,[9,18] reducing family burden,[4] shortening the duration of hospital care,[19] and reducing relapse rates.[20] However, critics have highlighted the large numbers of patients lost to follow-up in day hospital studies,[21] and have questioned whether day hospital treatment may actually "institution-alise" patients by encouraging them to attend for overlong periods of time.[6]

In part, this lack of consensus reflects the fact that research on acute day hospitals is difficult to interpret because of the range and complexity of the possible outcome variables.[22] For example, one key outcome, "use of hospital care", has been assessed in terms of: days in inpatient care, duration of day patient care, adjusted duration of day care (discounting weekends and days off), duration of index admission, nights out of hospital, actual attendance at day care, readmission to day care, readmission to inpatient care, and so on. The result of this complexity is that, although most acute day hospital trials report similar outcomes, they rarely report these outcomes in the same format. This makes it extremely difficult to make meaningful comparisons across trials. The picture is further complicated because many of the outcome variables are skewed and tend to be presented in forms (such as medians) that cannot be synthesised readily in a meta-analysis. For this review it was therefore considered essential to obtain individual patient data from included trials so that the relevant outcomes could be presented in a common format.

Research question

The objective was to assess the feasibility and effectiveness of admission to an acute day hospital versus admission to inpatient care for people with acute psychiatric disorders. The main hypothesis was that admission to a day hospital would reduce the extent of hospital care and the total costs of care, without any deterioration in follow-up rates or clinical and social functioning. In addition, the review attempted to determine: (1) for what proportion of acutely ill patients day hospital treatment was feasible; (2) whether patients recover more quickly with day hospital treatment (in terms of symptoms and social functioning); and (3) how far clinical and social recovery was affected by personal characteristics such as diagnosis, sex and age. The review was not concerned with the other modes of "partial hospitalisation" listed above (i.e. day treatment programmes and day care centres, see 'Background' section above). The use of partial hospitalisation as a form of transitional care is reviewed elsewhere, in the Cochrane Library.[23]

Methods

Inclusion criteria

Design

Eligible studies were randomised controlled trials that compared admission to an acute psychiatric day hospital with admission to inpatient care. An acute psychiatric day hospital was defined as a unit that provided "diagnostic and treatment services for acutely ill patients who would otherwise be treated on traditional psychiatric inpatient units". The term "day hospital" has been defined in the general introduction to this report.

Participants

Participants were patients with acute psychiatric disorders (all diagnoses) who would have been admitted to inpatient care if the alternative of day hospital admission had not been available. Studies were not eligible if they were largely restricted to patients who were aged under 18 years or over 65 years, or to those with a primary diagnosis of substance abuse and/or organic brain disorder.

Outcome measures

The four main outcome measures were:

- Feasibility and engagement:
  - proportion of patients suitable for day patient care
  - numbers lost to follow-up
- Extent of hospital care:
  - duration of initial admission
  - actual days in inpatient care
  - actual days in day patient care
  - actual days in inpatient or day patient care
  - number readmitted to inpatient or day patient care after discharge
- Clinical and social outcomes:
  - mental state (at various time points)
  - social functioning (at various time points)
  - burden on carers (at various time points)
  - deaths (suicide/homicide/all causes)
  - employed at end of study
  - satisfaction with care (patients and relatives)
- Costs of care:
  - cost of index admission
  - cost of hospital care (mean monthly: comprising cost of index admission plus cost of subsequent admissions)
  - cost of psychiatric care (mean monthly: comprising cost of hospital care plus cost of all ambulatory psychiatric care)
  - cost of all care (mean monthly: comprising cost of psychiatric care plus costs of other medical/social care, but excluding wages, costs to relatives and transfer payments).

Search strategy

The search began by deriving a list of search terms from reading overviews of the field and consulting experts in partial hospitalisation. This led to the following free text search strategy: (DAY HOSP*) or (DAY CARE) or (DAY TREATMENT*) or (DAY CENT*) or (DAY UNIT*) or (PARTIAL HOSP*) or (AMBULATORY TREATMENT) or (AMBULATORY CARE) or (DISPENSARY). This search string was then combined with the MeSH term (MENTAL ILLNESS) and with the Cochrane Collaboration's search string for potential trials and reviews.[24] The combined search string was then run on the following databases: Cochrane Controlled Trials Register (The Cochrane Library, issue 2, 1999); MEDLINE (1966 – December 1998); EMBASE (1980 – December 1998); CINAHL (January 1982 – December 1998); and PsycLIT (1966 – December 1998). The reference lists of all identified trials and reviews were scanned for references to additional trials. Experts in the field were approached to identify unpublished trials.

Selection of trials

The search for trials was performed independently and in parallel by two reviewers (MM and AA). Each reviewer read the abstracts of all publications and discarded those that were irrelevant to create a pool of trials in which day hospital treatment had

been compared against a control treatment. The two pools were then merged and photocopies of the articles describing the trials were obtained. Each reviewer then independently evaluated the trials in the pool to decided which met the inclusion criteria. Of 51 trials identified, nine met the inclusion criteria. A reliability study was performed, which showed complete agreement between raters on which trials met the inclusion criteria.

Quality assessment

Reviewers MM and AA rated the quality of all included trials according to three quality categories described in the Cochrane Collaboration handbook (for details see the "Details of studies included in the review" section below).[24] All trials in categories A, B or C were included.

Extraction and quality of data

Individual patient data were sought from trialists for all patients randomised in eligible trials (published or unpublished). The data requested were: date of birth or age, sex, diagnosis, randomisation status, social functioning at various time points, mental state at various time points, satisfaction with care, days in hospital, days in day hospital, time to discharge, number readmitted, deaths, if employed at the end of the study, and the costs of care. All individual patient data received were verified against the original trial reports to ensure both the accuracy of the meta-analysis database and the quality of randomisation and follow-up. Any queries were resolved by contacting the trialists. The final database entries were verified by the responsible trial investigator or statistician. For trials for which individual patient data were not available, categorical and continuous data were extracted separately from trial reports by two reviewers and cross-checked. Continuous data available only from trial reports were noted in the text but not included in the meta-analysis (this was not an *a priori* exclusion; there were just no instances where these data were presented in a usable form). Data were excluded if they: (1) could not be analysed on an intention-to-treat basis; (2) were collected using unpublished scales (such data are known to be subject to bias[25]); or (3) were available on less that 50% of randomised subjects.

The feasibility of day treatment was defined as the percentage reduction in acute inpatient admissions that could be achieved by diverting patients to an acute day hospital. Feasibility was estimated by a modification of the method suggested by Kluiter,[22] the general formula being: 100 × number engaging in day hospital treatment/(number assessed for eligibility × R), where R is the randomisation ratio for the trial (defined as number of patients randomised to day hospital divided by number of patients randomised). However, estimates of feasibility are profoundly affected by judgements about what is "engagement" in day hospital treatment and how many patients have been "assessed for eligibility". It was therefore decided to perform a sensitivity analysis to give a "best" and "worst" estimate of feasibility for each included trial.

The best estimate of feasibility was based on defining: (1) "engagement in day hospital" as being randomised to day hospital treatment; and (2) "number assessed for eligibility" as those remaining after exclusions for administrative reasons. Patients excluded for administrative reasons were defined as those who: (1) were too well to be randomised to day care; (2) left before they could be assessed; or (3) lived outside the study catchment area.

The worst estimate of feasibility was based on defining: (1) "engagement in day hospital" as: number randomised to day hospital treatment – (number admitted as inpatients in first 4 weeks + number of day patients who did not turn up for day hospital treatment); and (2) "number assessed for eligibility" as the number presenting for admission before any administrative exclusions were made.

A weighted average was derived for the best and worst estimates of feasibility derived in this way. However, for a minority of trials (referred to as "type 2" trials, see "Details of studies included in the review" below), the formula for calculating feasibility could not be applied because all patients were admitted to inpatient care before randomisation to continuing inpatient care or day hospital care. For these trials, a single estimate of feasibility was calculated, based on those patients randomised to day hospital care who experienced only a brief episode of inpatient care before transfer to a day hospital.

The number lost to follow-up was estimated by taking the number who were not re-interviewed at the final follow-up assessment. It was assumed that clients lost to follow-up also dropped out of care.

To facilitate comparisons between trials, continuous variables such as days in hospital were converted to a single common scale (such as mean days in hospital per month). Time spent in the day hospital was adjusted so that "days in day hospital" represented the actual number of attendances at the day hospital (including missed days), rather

than the total time for which the patient was a day hospital patient (except in the case of duration of initial admission). One trial[26] did not distinguish between duration of care and actual number of attendances, so the latter was estimated using the ratio of duration/actual attendances reported in another trial from the same centre[16] (which took place in the same day hospital and inpatient unit). Data concerning the use of hospital care were skewed, but are nonetheless presented as Cochrane plots in this review to facilitate comparison between trials (analysis using non-parametric tests gives the same results as the parametric analyses reported in the Cochrane plots).

For both mental state and social function there was no common outcome measure across the included studies. In order that the datasets could be pooled into a single analysis, outcomes for mental state and social function for each study were standardised to give variables with a zero mean and a standard deviation of 1. The data were then combined in a single longitudinal analysis using a random effects model. A difference in the effect of treatment would manifest itself in a more rapid decline in one treatment group than the other. A multilevel statistical model[27] was used that corresponds to straight lines being fitted to each subject. Random intercepts were considered to allow for individual variation between patients within treatment groups. An initial analysis was carried out to assess whether a random slope effect term needed to be included in the models. The average effect of each intervention over time is expressed as a mean line. The treatment effect can be measured by a time–intervention group interaction in the model. To assess the effect of treatment, a full model with a time–intervention group interaction was compared with a reduced model excluding this term. All analysis was performed using the MLwiN statistical program,[28] which provides a system for the specification and analysis of a range of multilevel models with estimation using iterative generalised least squares. Three covariates common to the included trials (age, diagnosis and sex) were included in the analysis.

Individual patient data on economic variables were not combined across trials because there is no agreed method for overcoming the problems caused by differences in costing methodology between trials and between countries. Instead, these data were presented adjusted to a common format (see types of outcome measure above) in the currencies used in the original trials. Percentage differences in costs between treatment and control conditions were then calculated and, where possible, costs of treatment and control group care were compared using non-parametric tests. For one trial,[26] the costs of hospital care were calculated by using individual patient data, working on the assumption that the relative costs of day hospital and inpatient care were similar to those reported in another trial from the same centre[16] (both trials took place in the same day hospital and inpatient unit).

Missing data

Among the included studies data were not reported on outcomes where less than 50% of those assessed at baseline failed to be reassessed on the same outcome at follow-up.

Subanalysis

There were no subanalyses.

General issues

All data were recorded on Cochrane plots so that the area to the left of the "line of no effect" indicated a "favourable" outcome for the first intervention mentioned in the title of the comparison. For categorical outcomes, the relative risk (RR) and number-needed-to-treat (NNT) were calculated. Categorical data were examined for heterogeneity using the chi-squared test. When heterogeneity was present, the data were re-analysed using a random effects model and efforts were made to identify the main source of the heterogeneity.

Details of studies included in the review

Nine trials (involving 1568 randomised patients and 2268 who were assessed for suitability for day hospital treatment) met inclusion criteria for the review (see *Table 1*). Two trials[29,30] were not carried out on an intention-to-treat basis and so provided data on feasibility only (individual patient data were not sought for these trials). Individual patient data were obtained for four trials involving 594 people.[15–17,26] Of the three remaining trials, contact with the trialists confirmed that individual patient data were no longer available for two;[31,32] trialists for one trial[33] could not be located.

Included trials were found to be of two types (from here on designated as type 1 and type 2). Type 1 trials excluded, before randomisation, any patients who were considered to be ineligible for day hospital treatment (for example, they were too violent or under compulsory detention). Type 2 trials randomised all patients presenting for admission regardless of suitability, but retained on

TABLE 1 *Characteristics of included studies*

Study	Methods	Participants	Outcomes
Creed *et al.*, 1990[26]	Allocation: randomised, sealed envelope Inclusion criteria: (1) presenting for IP admission; (2) not involuntary patient; (3) not too ill for DH; (4) no social factors that made day care impractical Analysis: ITT Follow-up: 3, 12 months Attrition: 31% Intervention: experimental acute DH; 8 nurses, 3 occupational therapists; *n* = 51 Control: routine IP; *n* = 51	*n* = 102; female 44%; ethnic ?%; mean age 42 years; married 39%; unemployed 45%; schizophrenic 23.5%; mood disorder 25.4%; mean previous admissions 1.8	(1) No. lost to follow-up; (2) no. readmitted; (3) duration of index admission (estimated from IPD); (4) IP and DP days/month (IPD); (5) mental state (IPD–PSE[77]); (6) social functioning (IPD–SBAS[80]); (7) burden on relatives (IPD–SBAS); (8) costs of hospital care (estimated from IPD)
Creed *et al.*, 1997[16]	Allocation: randomised, sealed envelope Inclusion criteria: (1) presenting for IP admission; (2) age 18–65 years; (3) not involuntary patient; (4) not too ill for DH; (5) not admission for detoxification; (6) no organic brain disease, personality disorder or mania Analysis: ITT Follow-up: 0.5, 1, 2, 3, 6, 12 months Attrition: 23.5% Intervention: experimental acute DH (CPN out of hours; *n* = 94 Control: routine IP; *n* = 93	*n* = 187; female 45.5%; ethnic 21.5%; mean age 38 years; married 33%; unemployed 41.5%; schizophrenic 38.5%, mood disorder 30%; mean previous admissions 2.6	(1) No. lost to follow-up; (2) no. readmitted; (3) duration of index admission (IPD); (4) IP and DP days/month (IPD); (5) mental state (IPD–CPRS[78]); (6) social functioning (IPD–SBAS[80]); (7) burden on relativess (IPD–SBAS); (8) cost of care (IPD)
Dick *et al.*, 1985[32]	Allocation: randomised – no further details Inclusion criteria: suitable for DH treatment (excluded if too ill, suicidal or impractical) Analysis: ITT Follow-up: 0, 3, 12 and 52 weeks Attrition: 29.6% Intervention: experimental acute DH; 2 trained staff + occupational therapist; patient/staff ratio 12.5:1; *n* = 43 Control: routine IP; *n* = 48	*n* = 91; female 67.6%; ethnic ?%; mean age 35 years; married 50.4%; unemployed 56.6%; schizophrenic ?%; mood disorder 56%; mean previous admissions ?	(1) No. lost to follow-up; (2) no. readmitted; (3) satisfaction with care; (4) duration of index admission; (5) mental state (CIS[82]); (6) cost of index admission
Herz *et al.*, 1971[31]	Allocation: randomised by random numbers table; candidates admitted as IP, then randomly allocated to DH or continuing IP Inclusion criteria: (1) not too ill for DH; (2) not too well for day care; (3) DH not impractical Analysis: ITT Follow-up: 2 weeks, 1, 5, 24 months Attrition: 18.8% Intervention: experimental acute DH (5 days/week, 8.00 am – 4.30 pm); patient/staff ratio not reported; *n* = 45 Control: routine IP; *n* = 45 Staff, setting and activities same for both groups	*n* = 90; female 59%; ethnic 37%; mean age 32 years; married 11%; unemployed ?%; schizophrenic 36%; mood disorder ?%; previous admission 49%	(1) No. lost to follow-up; (2) no. deaths; (3) no. readmitted; (4) duration of index admission
Kris, 1965[33]	Allocation: randomised – no further details Inclusion criterion: previously treated in hospital for psychotic symptoms Analysis: ITT Follow-up: 2 months after discharge Attrition: not clear Intervention: experimental acute DH (5 days/week, 9.00 am – 5.00 pm); patient/staff ratio not reported; *n* = 71 Control: routine IP; *n* = 70	*n* = 141; female ?%; ethnic ?%; mean age ?years; married ?%; unemployed ?%; schizophrenic ?%; mood disorder ?%; mean previous admissions ?	(1) No. employed 2 months after treatment ended

continued

TABLE 1 contd *Characteristics of included studies*

Study	Methods	Participants	Outcomes
Schene *et al.*, 1993[29]	Allocation: randomised – no further details, but 14 withdrawn due to "incorrect randomisation" Inclusion criteria: (1) referred for IP; (2) age under 65 years; (3) no organic brain disease; (4) no substance abuse or mental retardation; (5) no contraindications to DH Analysis: not ITT – 72 patients excluded after randomisation, including day patients transferred to ward for >28 days Follow-up: 6 months after discharge Attrition: not clear Intervention: experimental acute DH; staff/patient ratio 1:12.5 Control: routine IP (Nos. randomised to intervention and control groups not clear)	*n* = 222; demographic composition uncertain given the exclusions post-randomisation	All outcomes other than data relating to feasibility excluded because not an ITT analysis
Sledge *et al.*, 1996[34]	Allocation: randomisation – no further details Inclusion criteria: (1) age >18 years; (2) referred for IP; (3) living locally; (4) not involuntary patient; (5) not too ill for DH; (6) not intoxicated or medically unwell Analysis: ITT Follow-up: discharge, 2, 5, 10 months Attrition: 28.4% Intervention: experimental acute DH/crisis respite programme + "back-up" bed (DH open 9.00 am – 3.00 pm, 5 days/week); *n* = 93 Control: routine IP; *n* = 104	*n* = 197; female 49%; ethnic 32%; mean age 33 years; married 13.7%; unemployed 37%; schizophrenic 39%; mood disorder 52%; previous admissions? but 52% high service users	(1) No. lost to follow-up; (2) no. readmitted; (3) duration of index admission (IPD); (4) IP and DP days/month (IPD); (5) mental state (BPRS[79]); (6) social functioning (SAS[81]); (7) costs of care
Wiersma *et al.*, 1991[17]	Allocation: randomisation – by block using sealed envelope Inclusion criteria: (1) presenting for IP; (2) forensic patients; (3) patients with dementia Analysis: ITT Follow-up: 1, 2 years Attrition: 41% Intervention: experimental acute DH (5 days/week, 8.30am – 4.30 pm); 24-hour on-call line to nurse; *n* = 103 Control: routine IP; *n* = 57	*n* = 160; female 50%; ethnic ?%; mean age 42.4 years; married 37.5%; unemployed 89%; schizophrenic 33.1%; mood disorder 30.1%; previous admissions 61%	(1) No. lost to follow-up; (2) no. deaths; (3) no. readmitted; (4) no. unemployed; (5) days in hospital care (IPD); (6) mental state (IPD–PSE[77]); (7) social functioning (IPD–Groningen Social Disability Scale[83])
Zwerling and Wilder, 1964[30]	Allocation: randomisation by telephone; all patients about to be admitted allocated to DP or IP Inclusion criterion: presenting for IP Analysis: not ITT because patients with organic brain disease randomised but then excluded Follow-up: 2 years Attrition: 8% Intervention: experimental acute DH (5 days/week); *n* = 189 Control: routine IP; *n* = 189	*n* = 378; female ?%; ethnic ?%; mean age ?years; married ?%; unemployed %; schizophrenic ?%; mood disorder ?%; previous admission ?	All outcomes other than data relating to feasibility excluded because not an ITT analysis

BPRS, Brief Psychiatric Rating Scale; CIS, Clinical Interview Schedule; CPN, community psychiatric nurse; CPRS, Comprehensive Psychopathology Rating Scale; DH, day hospital; DP, day patient; IP, inpatient; IPD, individual patient data; ITT, intention-to-treat; PSE, Present State Examination; SAS, Social Adjustment Scale; SBAS, Social Behaviour Assessment Scale

the inpatient ward any "day hospital" patients who were too unwell for immediate day hospital treatment. The methodological differences between type 1 and type 2 trials meant that they had to be analysed in two separate comparisons.

The nine trials were quality assessed on the basis of their allocation concealment (according to the Cochrane handbook).[24] Seven trials were classified as grade A (meaning that allocation concealment was adequate).[15–17,26,30,31,33] One was classified grade B (allocation randomised but method unclear)[32] and one was classified grade C (allocation method inadequate).[29]

There were seven type 1 trials[15,16,26,29,31–33] and two type 2 trials[17,30] One type 1 trial[29] ceased to collect data after randomisation on any patients who had an admission period of less than 28 days or were transferred to a closed ward for more than 28 days. One type 2 trial[30] failed to report data on patients with organic brain disease (who were excluded from day hospital care after randomisation). This meant that data from these trials could not be analysed on an intention-to-treat basis, so all data were excluded other than those on the proportion suitable for day hospital treatment.

It was noted that, in three recent trials contributing individual patient data, day hospital care was augmented by sleep-over facilities[15,17,34] and/or outreach services.[16,17] This suggested that there may be changes in day hospital practice over time that could impact on outcome.

Details of studies excluded from the review

Table 2 gives details of all studies excluded from the review,[35–76] with reasons for exclusion (for further details of excluded studies see the Cochrane Library version of this review[77]).

Results of the review

Table 3 summarises the type 1 trial data on the proportion of patients who were suitable for day hospital treatment. The best estimate of feasibility (see "Methods" section above) was 37.5% (n = 1768; 95% confidence interval (CI), 35.2 to 39.8), while the worst estimate was 23.2% (n = 2268; 95% CI, 21.2 to 25.2). For type 2 trials the estimate of feasibility ranged from 18.4% (from one trial reporting the number of patients averaging more than six or more nights per week away from hospital in the first 15 weeks of the trial[17]) to 39.1% (based on a trial reporting on those treated entirely in a day hospital without readmission[30]). A more detailed analysis of feasibility in type 2 trials can be found in an article by Kluiter and colleagues.[22]

Five type 1 trials[15,16,26,31,32] provided data on the number of patients lost to follow-up, showing no difference between day hospital and control groups (*Figure 1*: n = 667; RR = 0.97; 95% CI, 0.74 to 1.27). However, these data showed evidence of heterogeneity (χ^2 = 8.6; p = 0.07) and analysis by year of publication suggested a time dependent effect, with earlier trials having a higher drop-out rate in the day hospital group and later trials having a higher drop-out rate in the inpatient group. One type 2 trial[17] (also a later trial) provided data on the number lost to follow-up, showing a significant difference in favour of the day hospital group (n = 160; RR = 0.69; 95% CI, 0.48 to 0.99; NNT = 6.3).

Three type 1 trials[15,16,26] provided individual patient data that permitted calculation of the duration of the index admission (defined as time from first admission to discharge to outpatient care). These data showed that patients randomised to day hospital care had a significantly longer index admission (*Figure 2*: n = 465; weighted mean difference (WMD) = 10.9 days; 95% CI, 1.09 to 20.7). However, there was significant heterogeneity on this variable (chi squared = 20.17; df = 2; $p < 0.01$). This heterogeneity was attributable to differences between the two UK trials[16,26] (where the index admission in the day hospital group was significantly longer than the index admission in the inpatient group), and the US trial[15] (where the day hospital index admission was shorter than the inpatient index admission). Two type 1 trials[31,32] also provided data on duration of the index admission in a form that could not be included in the meta-analysis. The first[32] reported a median duration of 34 days for patients in the day hospital group (after adjustment) and 20 days for those in the control group. The second[31] reported a mean duration of 48.5 days for day patients and 138.8 days for the control group, but no statistical tests or standard deviations were given. There were no data on duration of the index admission in type 2 trials.

The use of hospital care was assessed throughout this study using individual patient data from three type 1 trials.[15,16,26] These data showed no difference in the total number of days in hospital (as an inpatient or a day patient) between day hospital patients and controls (*Figure 3*: n = 465;

TABLE 2 *Characteristics of excluded studies*

Study	Reason for exclusion
Austin *et al.,* 1976[35]	Not randomised – survey comparing randomly selected patients from two different DH
Azim *et al.,* 1978[36]	Not randomised – quasi-experimental design, comparing IP, DH patients and non-patient controls
Barkley *et al.,* 1989[37]	Not randomised – retrospective study
Basker and Turel, 1986[38]	Not randomised – before and after design
Beigel and Feder, 1970[39]	Not randomised – quasi-experimental design, comparing patients completing DH programme with drop-outs
Bowman *et al.,* 1983[40]	Not randomised – survey examining differences between patients admitted to DH and IP care
Brook, 1973[41]	Not randomised – survey comparing patients treated in a crisis hostel with those treated in IP care
Comstock *et al.,* 1985[42]	Not randomised – retrospective multivariate analysis
Creed *et al.,* 1991[43]	Randomised (sealed envelope) – trial of DH versus IP admission, but randomisation compromised because patients allocated to DH were much less disabled than those allocated to IP care. Trialists concluded: "the ... study cannot be regarded as an intention-to-treat comparison"
Creed *et al.,* 1989[44]	Not randomised – quasi-experimental design comparing consecutive admissions to DH and IP care
Dick *et al.,* 1991[45]	Randomised – trial of DH versus continuing outpatient care; participants had chronic anxiety/depression
Drake *et al.,* 1994[46]	Not randomised – quasi-experimental design, comparing day centre with supported employment
Ettlinger *et al.,* 1972[47]	Not randomised – case-control study of DH versus IP care
Fink *et al.,* 1978[48]	Not randomised – quasi-experimental study of DH versus IP care
Glick *et al.,* 1986[49]	Randomised (method unclear) – trial of DH versus outpatient care; participants all had severe mental illness and had recently been discharged from hospital
Grad and Sainsbury, 1968[50]	Not randomised – quasi-experimental design comparing community care in two towns
Gudeman *et al.,* 1983[51]	Not randomised – before and after design
Guidry *et al.,* 1979[52]	Not randomised – before and after design
Guillette *et al.,* 1978[53]	Not randomised – survey comparing costs of DH with theoretical costs of IP care
Guy *et al.,* 1969[54]	Randomised (sealed envelope) – trial of DH versus outpatient care; participants suffered from various psychiatric disorders
Herz *et al.,* 1975[55]	Randomised (method unclear) – trial of routine IP care versus brief IP care versus brief IP care plus DH care; participants had acute psychiatric disorders and were about to be admitted to IP care
Hirsch *et al.,* 1979[56]	Randomised (method unclear) – trial of brief IP care (+ some DH care) versus routine IP care; participants had acute psychiatric disorders and were about to be admitted to IP care
Hogarty *et al.,* 1968[57]	Not randomised – survey comparing DH with IP and outpatient care
Hogg and Brooks, 1990[58]	Not randomised – survey comparing long-term IP with long-term day hospital patients
Kecmanovic, 1985[59]	Not randomised – case-control study comparing discharged IP with discharged day hospital patients
Kuldau *et al.,* 1977[60]	Randomised (method unclear) – compared brief IP care with extended transitional care; participants were patients about to be discharged from IP care
Levenson *et al.,* 1977[61]	Randomised (random numbers table) – trial of outpatient care versus IP care for people with acute schizophrenia
Linn *et al.,* 1979[62]	Randomised (sealed envelope) – trial of DH versus outpatient care for patients discharged from hospital
Lystad, 1958[63]	Not randomised – quasi-experimental design
Mathai and Gopinath, 1985[64]	Not randomised – survey of patients in IP, outpatient and DH care
Meltzoff and Blumenthal, 1966[65]	Randomised (sealed envelope) – trial of DH versus outpatient care for patients with a variety of mental disorders
Michaux *et al.,* 1972[66]	Not randomised – quasi-experimental study of DH versus IP care

continued

TABLE 2 contd *Characteristics of excluded studies*

Study	Reason for exclusion
Milne, 1984[67]	Not randomised – quasi-experimental study
Niskanen, 1974[68]	Not randomised – compared patients before and after treatment in DH
Odenheimer, 1965[69]	Not randomised – survey of the relatives of DH patients
Penk *et al.*, 1978[70]	Not randomised – case-control study of DH versus IP care
Piper *et al.*, 1993[71]	Randomised (method unclear) – not an ITT analysis; patients paired then randomised; drop-outs were replaced and matched controls deleted; patients with affective/personality disorders allocated to DH or outpatient care
Platt *et al.*, 1980[72]	Randomised (method unclear) – DH versus IP care but trial abandoned when insufficient patients randomised
Tantam and McGrath, 1989[73]	Not randomised – case-control study of a rehabilitation treatment for long-stay day hospital patients
Tyrer and Remington, 1979[74]	Randomised (sealed envelope) – trial of DH versus outpatient care for patients with anxiety/depression
Washburn *et al.*, 1976[75]	Randomised (method unclear) – trial of continuing IP care versus discharge to DH
Weldon *et al.*, 1979[76]	Randomised (method unclear) – trial of DH versus outpatient care for patients about to be discharged

TABLE 3 *Type 1 trials: proportion of patients with an acute condition who were suitable for DH treatment*

Study	No. eligible (best)	No. eligible (worst)	No. randomised	No. randomised DH	No. randomised and engaged	% feasible (best) (95% CI)	% feasible (worst) (95% CI)
Kris, 1965[33]	?	?	NAppl	NAppl	?	?	? (see text)
Herz *et al.*, 1971[31]	310	424	90	45	35	29.0	16.5
Dick *et al.*, 1985[32]	203	334	75	43	37	36.9	19.3
Creed *et al.*, 1990[26]	175	185	102	51	35	58.3	37.8
Schene *et al.*, 1993[29]	534	534	199	?	?	37.3	? (see text)
Creed *et al.*, 1997[16]	?	?	NAppl	NAppl	?	?	? (see text)
Sledge *et al.*, 1996[15]	546	791	197	93	93	36.1	24.9
All type 1 (95% CI)	1768	2268	663	232	200	37.5 (35.2 to 39.8)	23.2 (21.2 to 25.2)

NAppl, not applicable

WMD = –0.38 days/month; 95% CI, –1.32 to 0.55). However, further analyses of these data showed that, compared with controls, patients randomised to day hospital care spent significantly more days in this type of care (n = 265; WMD = 2.34 days/month; 95% CI, 1.97 to 2.70) and significantly fewer days in inpatient care (n = 265; WMD = –2.75 days/month; 95% CI, –3.63 to –1.87).

Five type 1 trials[15,16,26,31,32] provided data on the number of patients readmitted to hospital care (either inpatient or day hospital) after discharge from the index admission. These data showed no difference between day hospital and control groups (*Figure 4*: n = 667; RR = 0.91; 95% CI, 0.72 to 1.15). One type 2 trial[17] provided data on the extent of hospital care; however, this was in a format that could not be compared easily with that from type 1 trials, even though individual patient data were available. Rather than reporting days in day hospital or in inpatient care, this trial reported "nights in hospital" (defined as the number of nights spent in hospital during follow-up) and "nights out of hospital" (defined for the control group as nights on leave from inpatient care, and for the day hospital group as number of nights spent at home while in day care). This trial then combined these data to give a total length of stay in day/inpatient care. Relative to the data from type 1 trials, the total length of stay as reported by this trial increases the apparent length of day patient care because there is no adjustment for the fact that patients do not attend a day hospital every day of the week. Using this method, the trial found

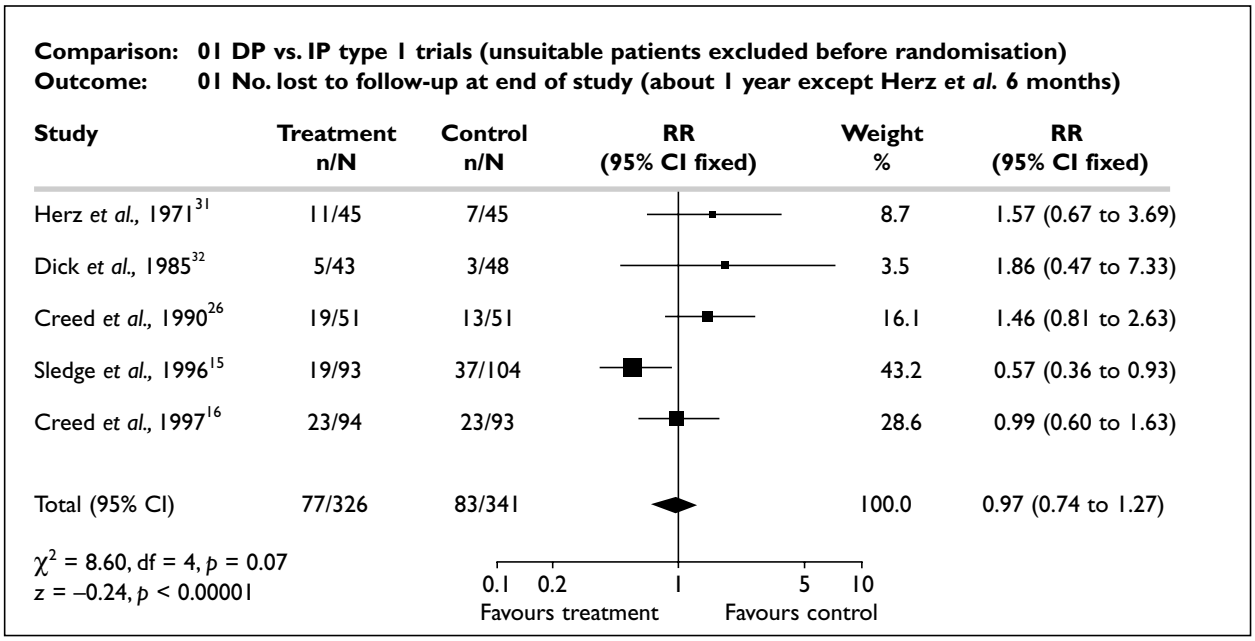

FIGURE 1 *Type 1 trials: number lost to follow-up (Cochrane plot from Cochrane Library version of the review)*

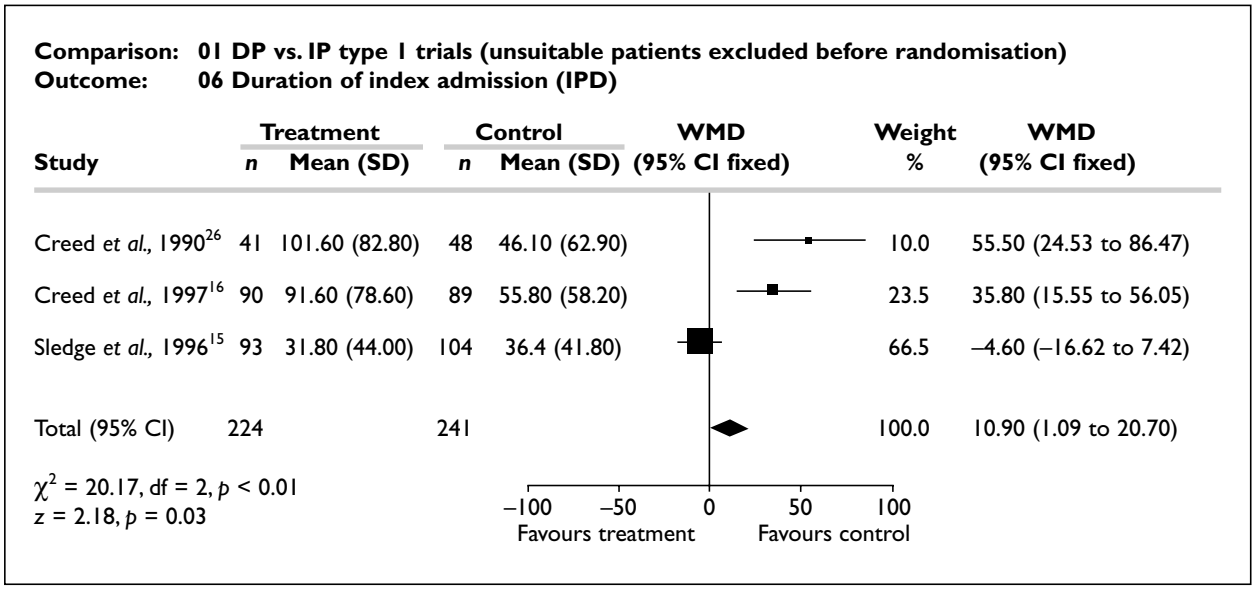

FIGURE 2 *Type 1 trials: duration of index admission (Cochrane plot from Cochrane Library version of the review); numbers in treatment and control groups may vary from* Figures 1 *and* 4 *depending on the number of patients providing continuous data*

no difference in the total number of days in hospital between day hospital patients and controls (n = 160; WMD = 1.1 days/month; 95% CI, –1.57 to 3.77), although experimental patients did spend more nights away from hospital over 2 years than control patients (control median 667 nights away from hospital; day hospital median 699 nights; Mann–Whitney z = 1.78; p = 0.038). These data could not be disaggregated into days in inpatient care and days in a day hospital.

Three type 1 trials (total n = 465) provided individual patient data on mental state and social functioning at various time points,[15,16,26] although there were some differences in the choice of questionnaire instruments and time points for follow-up data collection. Mental state was measured by the Present State Examination,[78] the Comprehensive Psychopathology Rating Scale[79] and the Brief Psychiatric Rating Scale.[80] Social functioning was measured by the Social Behaviour Assessment Scale[81] and the Social Adjustment Scale.[82] *Table 4* gives a breakdown of the demographic characteristics of the patients in these three trials.

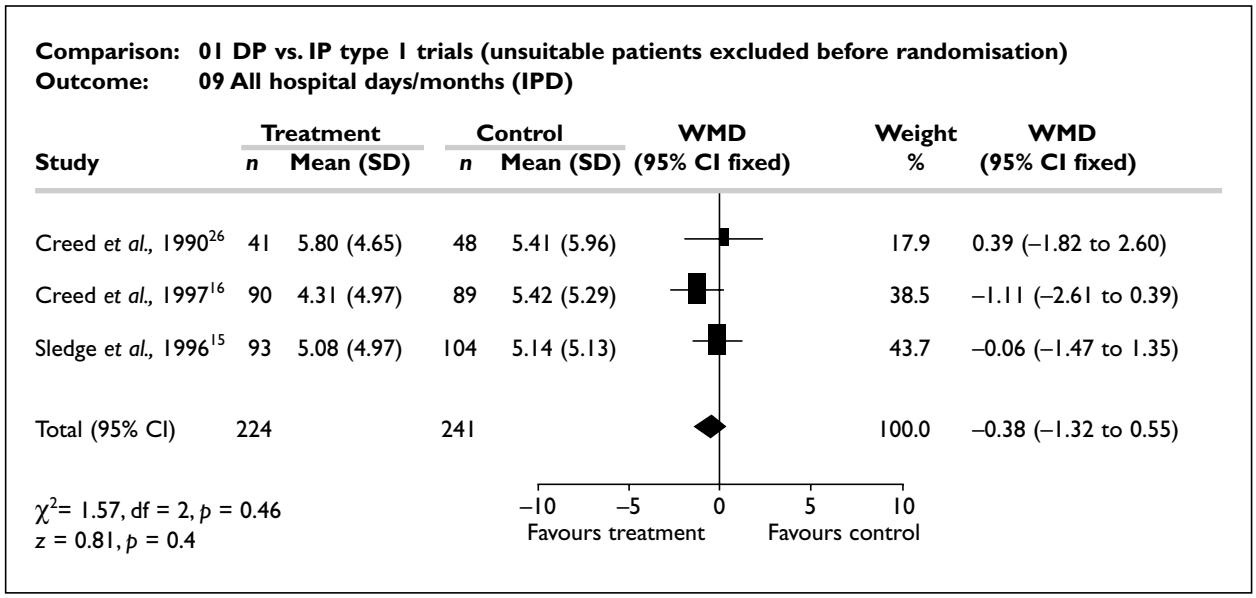

FIGURE 3 *Type 1 trials: all days in hospital (to DP or IP care) (Cochrane plot from Cochrane Library version of the review); numbers in treatment and control groups may vary from* Figures 1 *and* 4 *depending on the number of patients providing continuous data*

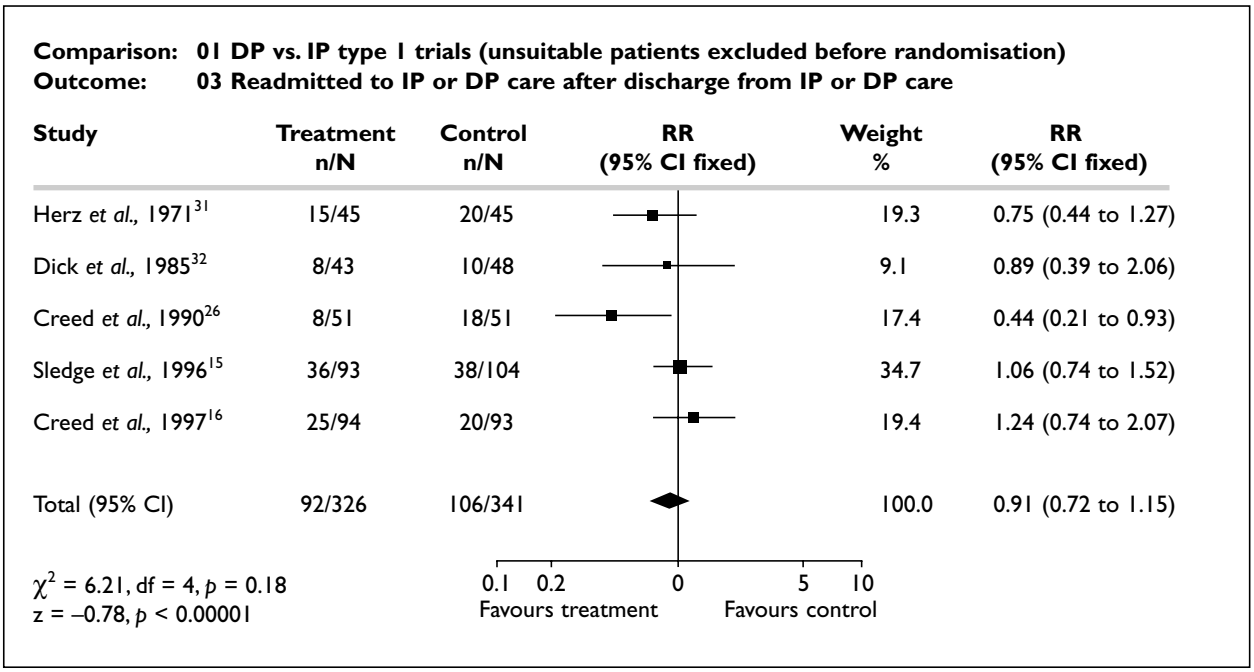

FIGURE 4 *Type 1 trials: no. patients readmitted (to DP or IP care) (Cochrane plot from Cochrane Library version of the review)*

A re-analysis of the data[15,16,26] was carried out, with the intervention types (inpatient, day hospital) concealed from the statistician. Twenty-one (4.5%) patients had to be dropped from the statistical modelling of outcome owing to incomplete covariate data. These appear to be evenly distributed between the intervention groups (*Table 5*). For the mental state re-analysis, 37 patients (8%) could not be included because of the absence of follow-up mental state data. These were as follows: seven from Creed and colleagues[26] (five inpatients and two day patients), seven from Creed and co-workers[16] (five inpatients and two day patients) and 23 from Sledge and co-authors[15] (16 inpatients and seven day patients). There was evidence of curvature of the profiles and positive skewness. A square root transformation was used to linearize the profiles and remove skewness in the patient and time-point level residuals. There was also evidence of both a significant random intercept ($\chi^2 = 180.25$; $p < 0.001$) and a significant random slope effect ($\chi^2 = 25.46$; $p < 0.001$) measured by change in log-likelihood, so both these terms were included in the statistical modelling.

Comparison of a full model including time–treatment interaction with a reduced model without the interaction suggested that there was evidence of a significant time–treatment interaction measured by change in log-likelihood ($\chi^2 = 9.66$; $p = 0.002$). The difference in slope was –0.007 (95% CI, –0.011 to –0.002) with the negative coefficient representing increased improvement in the day hospital patients (*Table 5*). The intervention group had a significant effect ($\chi^2 = 4.58$; $p = 0.032$), indicating a difference in baseline levels for the two groups. The difference was 0.144 (95% CI, 0.009 to 0.278), representing a higher baseline for the day hospital group. An analysis with a model assuming a common baseline did not modify the conclusion concerning the treatment effect. None of the other covariates had a significant effect. Unfortunately it is not possible to estimate the extent of the difference in improve-ment rates because back transformation of square-root transformed data is not easily interpreted.

One trial (with no individual patient data)[32] also measured mental state, by using the Clinical Interview Schedule[83] at 0.75, 4 and 12 months. No standard deviations were provided. A significant difference in favour of day hospital treatment was reported at 0.75 months, but not at the other time points (decrease in score: 0.75 months – day patients 13.6, inpatients 9.6, $p < 0.001$ *t*-test; 4 months – day patients 16.2, inpatients 11.6, *p*-value not significant; 12 months – day patients 20, inpatients 14.1, *p*-value not significant).

For the social functioning re-analysis, 149 patients (34%) could not be included owing to the absence of follow-up social functioning data. These were divided between the studies as follows: 15 from Creed and co-workers[26] (nine inpatients and six day patients), 83 from Creed and colleagues[16] (43 inpatients and 40 day patients), and 51 from Sledge and co-authors[15] (32 inpatients and 19 day patients). There was evidence of a significant random intercept ($\chi^2 = 62.58$; $p < 0.001$) but no significant random slope effect ($\chi^2 = 0.80$; $p = 0.67$) measured by change in log-likelihood, so only the random intercept was included in the statistical modelling. When a full model including time–treatment interaction was compared with a reduced model without the interaction there was no evidence of a time–treatment interaction measured by change in log-likelihood ($\chi^2 = 0.006$; $p = 0.941$; *Table 6*). There was a significant age ($\chi^2 = 7.82$; $p = 0.005$) and a significant gender effect ($\chi^2 = 21.95$; $p < 0.001$), with increased age having a positive effect on improvement, but males improving less.

One type 2 trial[17] also reported data on mental state (Present State Examination total score[78]) and social functioning (Groningen Social Disabilities Schedule[84]) at 0, 12 and 24 months. No significant differences were found between treatment and control groups on either variable at any time point (for example: mental state at 12 months – day patients 12.4 (standard deviation (SD) 12.9), inpatients 10.6 (SD 8.6); social functioning at 12 months – day patients 2.21 (SD 0.86), inpatients 2.46 (SD 0.84); a high score indicates a worse performance).

Two trials reported data on the burden on carers using the Social Behaviour Assessment Scale Burden Scale[81] at 0, 3 and 12 months[26] and at 0, 0.5, 1, 2, 3, 6 and 12 months.[16] However, data on burden from the latter trial at 6 and 12 months could not be included because it was available on less than 50% of the randomised patients. The available data showed no difference in carer burden between day hospital and control groups at 0.5, 1, 2, 3 and 12 months, although data were limited for all time points except 3 months (where WMD = –0.59; 95% CI, –1.62 to 0.44; i.e. not significant but favouring day hospital treatment).

Although deaths among participants were acknowledged in some type 1 trials, with one exception[31] (one death in the control group) these data were not reported in relation to randomisation group, nor was it possible to derive this information from individual patient data. One type 2 trial[17] showed no difference in death rates between day hospital and control groups ($n = 160$; RR = 0.74; 95% CI, 0.17 to 3.18). There were two suicides in the day hospital group, although these did not occur while the patients were actually attending the day hospital.

Two type 1 trials[16,33] reported on the number of patients who were unemployed. One[33] provided these data at 2 months after discharge from day or inpatient care (number unemployed: day patients 43/71; inpatients 57/70; RR = 0.74; 95% CI, 0.60 to 0.93), but the duration of the index admission was not specified. The other trial[16] demonstrated no difference in the number unemployed at 12 months ($n = 179$; RR = 0.88; 95% CI, 0.66 to 1.19). One type 2 trial[17] showed no difference in the number who were unemployed at 24 months ($n = 160$; RR = 0.95; 95% CI, 0.87 to 1.04). In one type 1 trial,[32] data on the number of patients who were not satisfied with care were presented; these data showed a significant difference in favour of day hospital care (n = 91; RR = 0.46; 95% CI, 0.27 to 0.79; NNT 3.07).

TABLE 4 *Type 1 trials: summary of covariates used in the individual patient analysis (no. (%))*

Covariate	Creed *et al.*, 1990[26] (IP)	Creed *et al.*, 1990[26] (DP)	Creed *et al.*, 1997[16] (IP)	Creed *et al.*, 1997[16] (DP)	Sledge *et al.*, 1996[15] (IP)	Sledge *et al.*, 1996[15] (DP)
No. randomised	51	51	94	93	104	93
No. after exclusion for incomplete baseline data	47	40	84	84	98	91
Sex						
Male	26 (55)	23 (58)	45 (54)	46 (55)	56 (57)	42 (46)
Female	21 (45)	17 (42)	39 (46)	38 (45)	42 (43)	49 (54)
Age (years)						
≤24	2 (4)	4 (10)	14 (17)	14 (17)	15 (15)	15 (16)
25–34	20 (43)	10 (25)	23 (27)	24 (29)	43 (44)	42 (46)
35–44	7 (15)	6 (15)	25 (30)	17 (20)	23 (23)	20 (22)
45–54	8 (17)	7 (18)	13 (15)	9 (11)	10 (10)	11 (12)
≥55	10 (21)	13 (32)	9 (11)	20 (24)	7 (7)	3 (3)
Bipolar or schizophrenic	18 (38)	14 (35)	40 (48)	31 (37)	56 (57)	46 (51)
Other diagnosis	29 (62)	26 (65)	44 (52)	53 (63)	42 (43)	45 (49)

TABLE 5 *Type 1 trials: model coefficients for standardised mental state score*

Parameter	Model coefficient (SE)	95% CI	*p*-value
Fixed effects			
Time–intervention interaction (months)	–0.007 (0.0022)	–0.011 to –0.002	0.002
Time (months)	–0.073 (0.0067)	–0.086 to –0.059	
Gender (0 = female; 1 = male)	0.018 (0.0642)	–0.110 to 0.147	0.777
Diagnosis (0 = other; 1 = schizophrenic or bipolar disorder)	0.054 (0.0648)	–0.076 to 0.184	0.406
Age	0.019 (0.1124)	–0.206 to 0.244	0.862
Creed *et al.*, 1997[16]	–0.046 (0.0899)	–0.225 to 0.134	
Sledge *et al.*, 1996[15]	0.084 (0.0948)	–0.106 to 0.273	0.189
Intervention group	0.144 (0.0671)	0.009 to 0.278	0.032
Constant	0.229 (0.1303)	–0.026 to 0.485	
Random effects (patient level)			
Constant (intercept)	0.211 (0.0324)	NAppl	
Constant × time (weeks)	0.001 (0.0007)	NAppl	
Time gradient (weeks)	0.00008 (0.00003)	NAppl	
Random effects (time level)			
Constant (error)	0.508 (0.0225)	NAppl	

SE, standard error

TABLE 6 *Type 1 trials: model coefficients for standardised social functioning score*

Parameter	Model coefficient (SE)	95% CI	*p*-value
Fixed effects			
Time–intervention interaction (months)	–0.001 (0.0121)	–0.025 to 0.023	0.941
Time (months)	–0.052 (0.0087)	–0.069 to –0.034	
Gender	0.404 (0.0862)	0.231 to 0.576	0.001
Diagnosis	0.087 (0.0854)	–0.084 to 0.257	0.310
Age	–0.100 (0.0356)	–0.171 to –0.028	0.005
Creed *et al.*, 1997[16]	–0.010 (0.1158)	–0.241 to 0.222	
Sledge *et al.*, 1996[15]	–0.010 (0.1094)	–0.229 to 0.209	0.995
Intervention group	–0.041 (0.1098)	–0.261 to 0.179	0.708
Constant	0.344 (0.1698)	0.011 to 0.677	
Random effects			
Patient level (constant – intercept)	0.313 (0.0440)	NAppl	
Time level (constant – error)	0.565 (0.0343)	NAppl	

Data on costs of care were reported by four type 1 trials[15,16,26,32] (three provided individual patient data; *Tables 7* and *8*) and by one type 2 trial[17] (individual patient data provided). The four type 1 trials showed that day hospital care was cheaper than hospital care (with eight of eight comparisons across a range of cost indices favouring day hospital care, six significantly; *Table 8*). Reductions in costs ranged from 33.5% to 49.6% for the index admission to 20.9 to 36.9% for the costs of all psychiatric care (including hospital care). The type 2 trial found no significant difference between day and inpatient care in two comparisons, although the trend favoured inpatient care.

Discussion

This review has suggested that psychiatric inpatient admissions could be reduced by at least 23% if patients were diverted to an acute day hospital. This substantial reduction could be achieved without increasing the loss to follow-up or the burden on relatives, although there are insufficient data to judge whether it would have any effect (positive or negative) on mortality rates. The review has also shown that, although patients who are diverted to acute day hospitals spend the same number of "days" in "hospital" as inpatients, they spend significantly more of these days in day hospital care. This means that the use of acute day hospitals leads to substantial cost savings because day hospital care is cheaper than inpatient care. The review has also provided some evidence that patients benefit clinically from diversion to day hospital care, in that they show more rapid improvement in their mental state and are more satisfied. However, contrary to the suggestions of some experts, day hospital care does not reduce readmission rates or lead to improvements in social functioning.

The evidence on feasibility of day hospital care is strong, being based on a large dataset from a number of trials. The evidence for clinical effectiveness is reasonable for improvements in mental state but is limited by the fact that the use of a square-root transformation has made it impossible to estimate the effect size on this variable. The finding of no effect on mental state from Wiersma and colleagues[17] does not contradict the positive finding from type 1 trials because: (1) the increased rate of improvement appears to occur before 1 year (in Wiersma and colleagues' trial the first follow-up was at 1 year); and (2) this was a type 2 trial, which meant that any effect of day care would be diluted by the large proportion of "day patients" who actually received inpatient care. The evidence is reasonably strong that social functioning is unaffected by day care because these data come from more than one trial and involve

TABLE 7 *Costs of care (raw data) in type 1 and type 2 trials*

Trial	DP: index admission (range)	IP: index admission (range)	DP: hospital care (range)	IP: hospital care (range)	DP: all psychiatric care (range)	IP: all psychiatric care (range)	DP: total cost (range)	IP: total cost (range)
Type 1 trials								
Dick *et al.*, 1985[32]	£307.3	£610.0	NK	NK	NK	NK	NK	NK
Creed *et al.*, 1990[26]	NK	NK	£4847 (3310–6384)	£6396 (4277–8515)	NK	NK	NK	NK
Creed *et al.*, 1997[16]	NK	NK	£4101 (2852–5351)	£6809 (5388–8231)	£4653 (3339–5966)	£7379 (5886–8872)	£5695 (2483–8907)	£7487 (5339–9636)
Sledge *et al.*, 1996[15]	$13,239 (9189–17,288)	$19,903 (15,906–23,899)	$24,376 (18,567–30,186)	$30,747 (24,904–36,590)	$26,819 (20,933–32,705)	$33,916 (27,940–39,893)	NK	NK
Type 2 trials								
Wiersma *et al.*, 1991[17]	NK	NK	Dfl 43,928 (33,535–54,319)	Dfl 35,990 (23,375–48,604)	Dfl 48,377 (38,005–58,748)	Dfl 38,252 (25,684–50,821)	NK	NK

DP, day patient; IP, inpatient; NK, not known

TABLE 8 *Type 1 trials: percentage differences in costs and significance of differences (Mann–Whitney test)*

Trial	Index admission	Hospital care	All psychiatric care	All costs care
Type 1 trials				
Dick *et al.*, 1985[32]	–49.6 (no test)	NK	NK	NK
Creed *et al.*, 1990[26]	NK	–24.2 ($p = 0.675$)	NK	NK
Creed *et al.*, 1997[16]	NK	–39.8 ($p < 0.001$)	–36.9 ($p < 0.001$)	–23.9 ($p = 0.014$)
Sledge *et al.*, 1996[15]	–33.5 ($p < 0.001$)	–20.7 ($p = 0.012$)	–20.9 ($p = 0.009$)	NK
Type 2 trials				
Wiersma *et al.*, 1991[17]	NK	+22.0 ($p = 0.175$)	+26.4 ($p = 0.057$)	NK

reasonably large data sets. There are also fairly sound data to suggest that the burden on carers is unaffected by day care, although beyond 3 months' follow-up these data are from only one trial. The evidence for improved satisfaction is weak, being based on data from one trial only. There are limitations on the interpretation of the cost data caused by the fact that the trials were conducted at different times, in different countries, and using different costing methodologies and pricing. Moreover, the costing data calculated for one trial[26] is an estimate (see "Method" above) based on prices derived from a similar trial conducted in the same setting.[16] However, since all type 1 trials achieved a shift of similar magnitude from inpatient care to day hospital care, it seems likely that all would have reduced costs, the exact magnitude of this reduction being determined by the relative pricing of inpatient and day patient care adopted by the trial. The finding of no difference in costs in the type 2 trial providing individual patient data[17] does not contradict the cost findings from type 1 trials. This trial set the price of a day in day treatment as equivalent to the price of 24 hours in inpatient treatment as part of an agreement between the participating hospital and the insurance companies involved.

A limitation on the applicability of the review is that, although substantial amounts of individual patient data were obtained, they were derived from trials conducted in only three centres (Manchester,[16,26] New Haven, CT,[15] and Groningen[17]). Although non-individual patient data from the other included trials generally support the individual patient data, those from a wider range of centres would enhance confidence in applicability. Questions about applicability are also raised by the reduction in loss to follow-up in the day hospital arms of later trials. This probably reflects a trend in psychiatric practice towards more persistent follow-up of patients who default from care. This is unlikely to affect the applicability of the findings based on individual patient data (all derived from more recent trials), but it may explain why some earlier reviewers concluded that day hospital care reduced readmission rates[20] (perhaps relapsing patients were lost to follow-up). A further observation with implications for applicability is an apparent difference in practice between US and UK day hospitals. Data on the duration of the index admission (both individual patient data and other aggregate data) suggest that US acute day hospitals are geared towards intensive treatment and rapid discharge, whereas UK day hospitals allow a more gradual "tailing off" of day care. It is unclear how far this difference has implications for effectiveness or cost. Differences in inclusion criteria between trials do not appear to be an important limitation on the applicability of the review. Generally, type 1 trials used similar inclusion criteria (that exclude involuntary, suicidal or dangerous patients), with the exception of the trials reported by Kris (which contributed few data to the meta-analysis)[33] and by Creed and colleagues[16] (which excluded patients with mania).

Conclusions

Implications for practice

Acute day hospitals are a means of providing intensive psychiatric care without the high overheads and restrictions on liberty that are associated with inpatient care. This review has shown that day hospitals can achieve substantial reductions in inpatient care while improving patient outcome, so it is curious that they are not more popular. In part this may be due to the difficulties in interpreting day hospital trials or to the growing interest in more radical community alternatives. On the other hand there are three disadvantages of day hospital treatment that need to be considered.

The first is that day hospital treatment does not appear to be as effective in reducing admission rates as more radical crisis intervention approaches. For example, assertive community treatment, when used to divert patients from hospital, can achieve a 55% reduction in admissions compared with the 23% achieved by day hospitals.[85] However, this has to be set against the fact that acute day hospitals do not involve radical, and perhaps unsustainable, alternations in psychiatric practice.[85]

The second disadvantage is that the cost savings achieved by day hospital care are modest. For example, compared with savings of up to 65% reported in studies of crisis intervention,[86] acute day hospital care (taking a pessimistic estimate) can be expected to achieve a saving of 4.8% of the costs of acute psychiatric care (calculated as: cost savings in patients diverted multiplied by the proportion of patients diverted; i.e. 20.9×0.232, assuming no inpatient beds were closed). Moreover, the cost equation would look more unfavourable still if it were necessary to build a new day hospital, rather than change practice in an existing non-acute day hospital. On the other hand, so far it has proved to be difficult to quantify reliably exactly how much is saved by crisis intervention approaches.[85] Moreover, if acute day hospitals

proved to be more sustainable than crisis intervention alternatives, this may mean that in-patient beds could actually be closed, thus shifting the cost equation in favour of day hospital care.

The third disadvantage is that, although recent trials[15,16] have enhanced day hospital care with respite or outreach services, it remains unclear how it should be integrated with other types of community care, such as assertive community treatment. It may be, for example, that day hospitals are less effective in situations where other alternatives to admission are available.

In summary, therefore, the decision to establish an acute day hospital must be made after careful consideration of local problems and resources. Acute day hospitals are an attractive option in situations where demand for inpatient care is high and facilities exist that are suitable for conversion. They are a less attractive option in situations where the demand for inpatient care is low and where effective alternatives are already in operation.

Implications for research

This review has suggested four avenues for further research on acute day hospitals, as listed below in order of priority:

1. There is a need for a multicentre randomised controlled trial to show how far the findings from the present small number of centres can be more widely replicated across a range of services and settings (a recommendation previously made by other experts[22]). Future trials should make use of the common set of outcome measures used in this review and should also take care to report data on mortality and other untoward events.
2. It is important to examine how acute day hospital care can be most effectively integrated into a modern community-based psychiatric service (for example working in combination with crisis teams). It may be of interest to compare the two approaches to acute day hospital care that are found in type 1 and type 2 trials.
3. It would be of interest to explore the relative cost-effectiveness of the US and UK approaches to acute day hospital care (rapid discharge versus gradual discharge).
4. It would also be useful to examine why patients' psychiatric symptoms appear to resolve more rapidly when they are receiving day care (for example, does hospital admission actually worsen symptoms of depression or anxiety?).

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Chapter 2

Vocational rehabilitation for people with severe mental disorders

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List of abbreviations

BPRS	Brief Psychiatric Rating Scale
CE	competitive employment*
CHIRP	community-based hospital industrial rehabilitation placement*
CI	confidence interval
IPS	individual placement and support
NNT	number-needed-to-treat
NS	not significant*
PVT	prevocational training
RCT	randomised controlled trial
RR	relative risk
SD	standard deviation
SEm	supported employment
TE	transitional employment
VR	vocational rehabilitation

* Used only in tables

Executive summary

Background

People who are disabled by severe mental disorders experience high rates of unemployment, but most want to work. Prevocational training (PVT) is the traditional approach to helping such people to return to work. PVT assumes that a period of preparation is required before those with a severe mental disorder can enter into competitive employment. Supported Employment (SEm) is a new approach that places clients in competitive employment without extended preparation. Both PVT and SEm are widely practised, but it is unclear which is the most effective.

Objectives

The overall objective of this review was to assess the effectiveness of PVT and SEm relative to each other and to standard care (in hospital or the community) for people with severe mental disorders. In addition, the review examined the effectiveness of: (1) special types of PVT ("clubhouse" model) and SEm (individual placement and support model); and (2) modifications for enhancing PVT (e.g. payment or psychological interventions).

Methods

Study selection

Eligible studies were randomised controlled trials (RCTs) examining the effectiveness of vocational rehabilitation approaches (PVT and SEm or modifications) for people of working age and suffering from a severe mental disorder.

Data sources

Relevant trials were identified from searches of the Cochrane Schizophrenia Group's specialised register, MEDLINE, EMBASE, CINAHL and PsycLIT, and the reference lists of all identified studies and review articles. Researchers who were active in the field were approached in order to identify unpublished studies.

Data extraction

All data were extracted independently by two reviewers and cross-checked. Continuous data were excluded if they were collected by using an unpublished scale or were based on a subset of items from a scale.

Data synthesis

For all comparisons, the primary outcome was the number of clients who were in competitive employment at various time points. Secondary outcomes were: other employment outcomes, clinical outcome and costs. The relative risk (RR) and number-needed-to-treat (NNT) were calculated for the relevant categorical outcomes. Continuous data were either presented as in the original trial reports or, where possible, combined across trials as a standardised mean difference score.

Results

Eighteen RCTs of reasonable quality were identified: PVT versus hospital controls, three RCTs, $n = 172$; PVT versus community controls, five RCTs, $n = 1204$; modified PVT, four RCTs, $n = 423$; SEm versus community controls, one RCT, $n = 256$; and SEm versus PVT, five RCTs, $n = 491$). The main finding was that, on the primary outcome (number in competitive employment), SEm was significantly more effective than PVT at all time points (e.g. at 12 months, SEm 34% employed, PVT 12% employed; RR of not being in competitive employment = 0.76, 95% confidence interval 0.69 to 0.84, NNT = 4.5). Clients in SEm also earned more and worked more hours per month than those in PVT.

Conclusions

The main finding was that SEm was more effective than PVT for patients suffering from a severe mental disorder who wanted to work. There was no evidence that PVT was more effective than standard community care or hospital care. The implication of these findings is that people suffering from mental disorders who want to work should be offered the option of SEm. Commissioning agencies would be justified in encouraging vocational rehabilitation (VR) providers to develop more SEm schemes.

From a research perspective, the cost-effectiveness of SEm should be examined in larger multicentre trials, both within and outside the USA. There is a case for countries outside the USA to survey their existing VR services to determine the extent to which the most effective interventions are being offered.

Vocational rehabilitation for people with severe mental disorders

Background

People who suffer from a severe mental disorder experience high rates of unemployment. In the USA unemployment rates amongst such people are estimated at 75–85%,[1,2] while in the UK rates of 61–73% have been reported.[3,4] These high percentages reflect the disability caused by severe mental illness, but they also reflect discrimination (unemployment rates are higher than in other disabled groups[5]) and the low priority given to employment by psychiatric services.[6] Despite high unemployment rates amongst those who are severely mentally ill, surveys have consistently shown that most of these people want to work.[1,7,8]

There are compelling ethical, social and clinical reasons for helping severely mentally ill people to work. From an ethical standpoint, the right to work is enshrined in the Universal Declaration of Human Rights 1948 and has been incorporated into national disability acts in Europe and the USA. From a social standpoint, high unemployment rates are an index of the social exclusion of severely mentally ill people, which many governments, including that of the UK, are committed to reducing.[9] Finally, from a clinical standpoint, employment may lead to improvements in the outcome of severe mental illness through increasing self-esteem, alleviating psychiatric symptoms, and reducing dependency and relapse.[1]

Helping mentally ill people to work is not a new idea. The value of therapeutic work was recognised by the pioneers of the asylum movement and, in their latter days, many large asylums depended on the labour of their inmates on farms, in workshops and in work crews.[10] As asylums closed down, work experience played an important role in the preparation of patients for discharge. Patients who performed well on graded tasks within the hospital were gradually reintroduced to working in the community, often through special arrangements with local employers. As community care developed, these arrangements evolved into enterprises or workshops providing "sheltered" employment within a segregated work setting.[11] Such sheltered workshops aimed to place clients in normal employment after a period of training, but follow-up studies showed a success rate of only 5–10%.[12,13]

The "clubhouse" movement arose in the 1950s as an alternative to traditional "sheltered employment".[14] This proposed that better employment outcomes could be achieved by fostering patient autonomy in a non-psychiatric setting known as a "clubhouse", which is a building run by clients and staff along egalitarian lines, where clients meet for social activity, mutual support and graded work experience. Like the traditional approach, the clubhouse approach involves a period of preparation (prevocational training (PVT)) before clients attempt to return to competitive employment. This period of preparation essentially consists of two stages: "the work ordered day" and "transitional employment" (TE).[15] The work-ordered day refers to a process whereby clients (working side by side with staff) take responsibility for managing and maintaining the clubhouse as a means of preparing for TE, which refers to the placement of clients in a series of paid but temporary jobs controlled by the clubhouse, in order to help them to develop the skills and confidence required to cope with competitive employment.[16] Although there are no rigid guidelines for the length of time spent on work crews, clients are discouraged from seeking competitive employment until they have achieved success in TE. They are free to return to work crews at any time.[17] Cross-fertilisation between the clubhouse and traditional methods led to a number of hybrid approaches (or stepwise eclectic approaches), combining, for example, TE with pre-employment training.[16]

In the mid-1980s a new approach to vocational rehabilitation (VR) emerged, known as supported employment (SEm). Originally developed for people with learning disabilities, SEm has been defined as paid work that takes place in normal work settings, with provision for ongoing support services.[16,18] Proponents of SEm had two objections to PVT.[17,19] First, they argued that it promotes dependency and thus deters clients from finding competitive employment. Secondly, they maintained that it was not effective in developing work skills. Instead of PVT, they proposed trying to

place clients as quickly as possible in competitive employment positions, where they would receive intensive on-the-job support and training from personnel known as "job coaches".[20]

Individual placement and support (IPS) is a carefully specified variant of SEm, distinguished by six key principles:

- The goal is competitive employment in work settings integrated into a community's economy.
- Clients are expected to obtain jobs directly, rather than following lengthy pre-employment training (rapid job search).
- Rehabilitation is an integral component of mental health treatment rather than a separate service.
- Services are based on clients' preferences and choices.
- Assessment is continuous and based on real work experiences.
- Follow-on support is continued indefinitely.

Adherence to IPS guidelines may be measured by using a fidelity scale.[21]

All three approaches to VR (traditional, clubhouse and SEm) are widespread, both in a pure form and in combination with other approaches. For example, in the USA there are: around 3000 "psychiatric rehabilitation providers" (most of which offer a traditional approach); about 230 clubhouses; and about 36,000 people with mental disorders in SEm positions.[16,22] In the UK there are 135 organisations offering traditional sheltered employment and 77 offering SEm.[23] There is no consensus on how far PVT approaches (such as sheltered workshops or clubhouses) and SEm schemes are effective at helping people with severe mental disorders to obtain employment.

Research question

The main objective was to determine how far PVT and SEm were effective in helping people with severe mental disorders to obtain competitive (i.e. open) employment. The review also examined how far PVT and SEm affected other work and clinical outcomes. The main comparisons in the review were as follows:

- PVT versus standard hospital care
- PVT in addition to standard community care versus standard community care alone
- SEm in addition to standard community care versus standard community care alone
- SEm versus PVT.

The reviewers also examined the effectiveness of modifications designed to enhance approaches to VR (i.e. payment or psychological interventions, and rapid entry into TE) and the effectiveness of well-characterised subtypes of PVT and SEm (clubhouse and IPS models respectively). The reviewers did not consider the effectiveness of assertive community treatment and case management in improving employment outcomes because these general approaches to enhancing community care have been reviewed elsewhere.[24,25]

Methods

Inclusion criteria

Design

Eligible studies were randomised controlled trials (RCTs) that compared a VR approach (PVT or SEm) with: standard hospital or community care; another VR approach; or a VR approach enhanced by some modification (such as payment or psycho-logical interventions). Trials were excluded if they failed to provide outcome data on 50% of the randomised participants or if they failed to provide data that could be analysed on an intention-to-treat basis.

Participants

VR services are not designed as an intervention for specific diagnostic groups, nor are they applied in a diagnostic-specific way in everyday practice. Therefore, for the purpose of this review, the main requirements of participants were that they were similar to those who typically present to VR services (i.e. that they were suffering from a severe mental disorder and were of working age). Specific inclusion criteria were that a majority of the clients in the trial were: (1) aged 18 to 65 years; and (2) suffering from a severe mental disorder (defined as: schizophrenia or schizophrenia-like disorder; bipolar disorder; or depression with psychotic features). Substance abuse was not considered to be a severe mental disorder in its own right, but participants were eligible if they had a problem with substance abuse in addition to a mental disorder. Learning disability was not considered as a severe mental disorder and trials were excluded where the majority of clients were suffering from a learning disability.

Types of intervention

Four interventions were defined:

- PVT, defined as any approach to VR in which participants were expected to undergo a period of preparation before being encouraged to seek competitive employment. This involved either

work in a sheltered environment (such as a workshop or work unit) or some form of pre-employment training or TE. Both the traditional (e.g. sheltered workshop) and clubhouse approaches were classified as PVT.
- SEm, defined as any approach to VR that attempted to place clients immediately in competitive employment. It was acceptable for SEm to begin with a short period of preparation, but this had to be of less than 1 month's duration and not involve: employment training; work placement in a sheltered setting; or TE. IPS was defined as SEm that adhered to the six principles outlined in the "Background" section above.
- Modified VH programmes, defined as either PVT or SEm, which had been enhanced by some technique to increase participants' motivation. Typically, such techniques consisted of payment or some form of psychological intervention.
- Standard care, defined as the usual psychiatric care for patients in the trial, without any specific vocational component.

Outcome measures

The primary outcome was the number of clients in competitive employment at various time points (defined as a full- or part-time position held by the client in an ordinary work setting, for which they were receiving payment at the market rate). Secondary outcome measures were grouped into three main categories:

- Other employment outcomes:
  - number in any form of employment (defined as competitive employment, TE, SEm or voluntary work)
  - number in any form of employment or education (defined as above but including places on training courses or in full- or part-time education)
  - mean hours per month in competitive employment
  - mean monthly earnings
- Clinical outcomes:
  - number lost to follow-up (for trials with community or hospital controls only) or number not participating in a programme (for trials comparing different VR approaches)
  - number admitted to hospital (for trials with a community control) or number living in the community at the end of the study (if a hospital control)
  - other clinical outcomes (e.g. symptoms, quality of life and social functioning).
- Costs
  - mean monthly programme costs (direct costs of experimental programme versus direct costs of control programme)
  - mean monthly healthcare costs (including costs of all psychiatric/medical care and programme costs)

Search strategy

The search began by deriving a list of search terms from reading overviews of the field and consulting experts in VR. This led to the following free-text search string:
(SUPP* EMPLOY*) or (EMPLOYMENT*) or (PSYCHOSOCIAL* REHAB*) or (PSYCHIATRIC* REHAB*) or (OCCUPATIONAL* REHAB*) or (SOC* REHAB*) or (WORK* REHAB*) or (JOB* REHAB*) or (SHELTERED* WORK*) or (TRANSITIONAL* EMP*) or (REHABILITATION* COUNSELLING) or (VOCATION*) or (FOUNTAIN HOUSE) or (FOUNTAIN-HOUSE) or (CLUBHOUSE*) or (CLUB-HOUSE*).

This search string was then combined with the MeSH term (MENTAL ILLNESS) and with the Cochrane Collaboration's search string for potential trials and reviews. The combined search was then run on the following databases: MEDLINE (1966 – December 1998); PsycLIT (1887 – December 1998); EMBASE (1980 – December 1998) and CINAHL (January 1982 – December 1998). This identified 40 confirmed trials and 13 review articles.

The sensitivity of the search strategy was examined by comparing the search results with the reference lists of the identified reviews to determine how many trials cited in the reviews had not been detected. Of three undetected trials cited in the reviews, two were not listed on any of the databases, while the third was indexed under the term "DELIVERY OF HEALTH CARE/INTEGRATED". This term was then added to the search strategy and the search was re-run, and, finally, the results of the search were compared against the bibliographies of two unpublished PhD theses,[26,27] but no further trials were detected.

Selection of trials

The search for trials was performed by one reviewer (RC). The list of publications identified by the above search strategy was independently examined by two reviewers (MM and RC). Each reviewer discarded irrelevant publications and retained only those relating to trials in which some form of VR had been compared against a control treatment. There were no disagreements between the raters on which trials should be discarded.

The reviewers then obtained copies of all articles relating to relevant trials. Once obtained, they were read independently by the two reviewers, who decided whether or not individual trials were eligible for the study. They were then allocated to one of five relevant comparisons:

- PVT versus standard hospital care
- PVT versus standard community care
- modifications of VR programmes
- SEm versus standard community care
- SEm versus PVT.

Inter-rater agreement was assessed for overall eligibility and for the allocation of trials to comparisons. The inter-rater reliability for the inclusion of trials in the review (based on a sample of 20 trials) was 0.89. There was complete inter-rater agreement on the allocation of trials to the five comparisons.

Quality assessment

Quality was assessed independently by two reviewers. Trials were classified according to three categories of allocation concealment:[28] A, adequate; B, method of concealment unclear; C, inadequate. Disagreements were resolved by discussion, or, failing this, by seeking further information from the trialists. Trials in all three categories of allocation concealment were included in the review, but, when there was a significant finding, a sensitivity analysis was performed to exclude trials in which the quality of the random allocation was considered to be inadequate (category C). Blinding of patients and treating clinicians is not possible in a trial of VR. It is also difficult to see how persons who are evaluating outcome could remain blind to group allocation, given that the primary data they have to collect concerns days in different types of employment, which would normally disclose group allocation. However, it is possible for those evaluating outcome to be independent of those providing the treatment. Trials were therefore rated on the independence of evaluators (non-independent evaluators being defined as being also involved in the treatment of trial patients) and, if non-independent, whether the information collected was objective (based on client records) or subjective (based on interview or clinical judgement). In the event of a significant finding, a sensitivity analysis was performed to exclude trials conducted by non-independent evaluators.

Extraction and quality of data

Categorical data and continuous data were extracted individually by the two reviewers and then cross-checked. When further clarification was needed, the authors of trials were contacted to provide missing data. Data were excluded from studies in which more than 50% of the participants in any group were lost to follow-up (except for the outcome of "leaving the study early"). The impact of including studies with high attrition rates (25–50%) was analysed in a sensitivity analysis. Data were excluded if they were collected by using an unpublished scale or based on a subset of items from a scale.

Data synthesis

For categorical outcomes, a standard estimation of the risk ratio and its 95% confidence interval (CI) was calculated. The relative risk (RR) was chosen over the Peto odds ratio because the latter tends to overestimate effect size when event rates are high.[29] The number-needed-to-treat statistic (NNT) was also calculated. For continuous outcomes a standardised mean difference between groups was estimated. Continuous data presented in the trial reports without summary statistics (i.e. mean, standard deviation (SD)/standard error or non-parametric equivalent) were not considered valid, although the existence of such data was noted in the text.

A chi-squared test was used, as well as the visual inspection of graphs, to investigate the possibility of heterogeneity. A significance level of less than 0.10 was interpreted as evidence of heterogeneity, in which case the data were re-analysed using a random effects model. If this made a substantial difference, the studies responsible for heterogeneity were presented separately from the main body of homogeneous trials and the reasons for heterogeneity were investigated.

Data have been reported as presented in the original studies, with two exceptions. First, continuous variables such as costs or days in employment were converted to a single common scale (such as mean days in employment per month or mean monthly costs) in order to facilitate comparisons. Secondly, the number of clients not participating was estimated by taking the number of clients who were not re-interviewed at the final follow-up assessment, or by taking actual non-participation rates (when these were given in the trial report and were greater than the number not re-interviewed). It was assumed that clients lost to follow-up remained unemployed.

Two subtypes of PVT and SEm (the clubhouse and IPS models respectively) have been sufficiently specified to be regarded as approaches in their

own right (see above for details). Data from trials using these approaches were included in the main PVT or SEm comparisons, but were also analysed separately in subanalyses.

Details of studies included in the review

Eighteen RCTs were included in the trial (*Tables 1–5*). The trials were distributed across the comparisons as follows:

- PVT versus standard hospital care (*Table 1*), three RCTs, $n = 172$[30–32]
- PVT versus standard community care (*Table 2*), five RCTs, $n = 1204$[33–37]
- modified PVT (*Table 3*), four RCTs, $n = 423$[38–41]
- SEm versus standard community care (*Table 4*), one RCT, $n = 256$[42]
- SEm versus PVT (*Table 5*), five RCTs, $n = 491$.[11,43–46]

Randomisation

There were three trials in allocation concealment category A (PVT versus hospital,[31,32] SEm versus PVT[43]), 14 in category B,[11,30,34–42,44–46] and one in category C.[33]

Independence of raters

In six trials the raters of outcome were either not independent or their independence was unclear (PVT versus hospital;[30,31] PVT versus community;[33–35] PVT versus SEm[45]).

Follow-up

Only one trial[41] had a follow-up rate of less that 75% (this was 63%).

Details of studies excluded from the review

There were 22 excluded studies,[42,47–67] as listed in *Table 6* with reasons for their exclusion.

Results of the review

For all comparisons the primary outcome was the number of clients in competitive employment at various time points. Secondary outcomes were: (1) other employment outcomes; (2) clinical outcomes; and (3) costs. Primary and secondary outcomes will be considered in turn for each of the comparisons and subanalyses. When an outcome is not referred to under a comparison or subanalysis (e.g. mental state), this indicates that no data were available on this outcome for that comparison.

Prevocational training versus standard hospital care (*Table 1*)

Three trials provided data for this comparison.[30–32] Few data were available on the primary outcome (number in competitive employment). One small trial[30] reported data at 8-month follow-up, which showed a non-significant trend in favour of people in the PVT group ($n = 50$; RR = 0.79; 95% CI, 0.63 to 1.00).

Other employment outcomes

Becker[30] reported that, at 8 months, significantly more clients in the PVT group had obtained some form of employment ($n = 50$; RR = 0.42; 95% CI, 0.26 to 0.68; NNT = 1.8). Walker and colleagues,[32] however, reported no difference in hours/month in competitive employment ($n = 28$; mean PVT = 36.8, control mean = 31.6; $p = 0.92$ Mann–Whitney). Kuldau and co-workers[31] reported that PVT clients earned signifi-cantly more dollars per month than those in the control group (PVT mean = $176.2, control mean = $97.3; p <0.01). Data from two trials[30,32] showed a non-significant trend towards better participation amongst PVT clients (n = 78; RR = 0.5; 95% CI, 0.05 to 5.25).

Clinical outcomes

Clients in the PVT group were not more likely to be discharged from hospital ($n = 50$; RR = 0.95; 95% CI, 0.76 to 1.19).[30]

Prevocational training versus standard community care (*Table 2*)

PVT in this context refers to all types, including the clubhouse approach. Five trials provided data for this comparison.[33–37] Some limited data were available on the primary outcome (number in competitive employment) at 18 and 24 months.[33,35] These showed no difference between PVT and control groups (18 months: $n = 28$; RR = 1.18; 95% CI, 0.87 to 1.61;[35] 24 months: $n = 215$; RR = 0.95; 95% CI, 0.77 to 1.17[33]).

Other employment outcomes

Three trials[33,34,36] reported data on numbers of participants in any form of employment. These showed no difference between PVT and control groups at 3, 6, 9, 12 and 18 months.

Clinical outcomes

Data from two trials[34,36] showed no difference in the number of clients participating in the programme ($n = 284$; RR = 0.97; 95% CI, 0.73 to 1.30) between PVT and control groups. Data from three trials[33,34,37]

TABLE 1 *Details of included studies (PVT versus standard hospital care)*

Study	Intervention	Main outcome measures (data suitable for analysis)	Design
Becker, 1967[30]	PVT: specialised rehabilitation ward with sheltered workshop and transitional work opportunities in the community Control: continuing treatment on inpatient rehabilitation wards	No. in competitive employment at 8 months; no. in any employment at 8 months; no. discharged from hospital at 8 months; no. lost to follow-up at 8 months	Randomisation: "randomly assigned" but no details given (category B) PVT group 25; control group 25; follow-up period 8 months; follow-up rate 100%; raters probably not independent
Kuldau and Dirks, 1977[31]	PVT: care in rehabilitation ward/transitional day hospital, help with finding supported or sheltered work, eventually graduating to discharge to community Control: hospital "rapid discharge" programme followed by standard community aftercare	Mean income at 18 months	Randomisation: sealed envelope (category A) PVT 44; hospital control 50; follow-up period 18 months; follow-up rate 95%; unclear if raters independent
Walker *et al.*, 1969[32]	PVT: CHIRP – clients placed in an industrial setting off hospital grounds Control: same hospital and community treatment as PVT group, but could not attend CHIRP	No. not participating in programme; mean income	Randomisation: random numbers table (category A) PVT 14; control 14; follow-up period 6 months; follow-up rate 100%; raters independent

CHIRP, community-based hospital industrial rehabilitation placement

TABLE 2 *Details of included studies (PVT versus standard community care)*

Study	Intervention	Main outcome measures (data suitable for analysis)	Design
Beard *et al.*, 1963[33]	PVT: clubhouse with work-ordered day followed by TE and placement in "real" job with outreach and supported accommodation Control: community care from usual services	No. in any form of employment at follow-up; non-attendance at 6 months; no. readmitted to hospital at 1 year	Randomisation: by day of application (category C) PVT group 274; control group 78; follow-up period every 3 months for 24 months; follow-up rate 86%; raters probably not independent
Bond *et al.*, 1984[34]	PVT: "Thresholds" – a privately operated VR programme that provides PVT Control: community care from usual services, with additional 6 hours/week of supportive psychotherapy	No. not participating in programme; no. not in any kind of employment at 9 months; no. admitted to hospital in first year of study; mean cost of hospital care; mean cost of total care	Randomisation: random assignment (category B) PVT 66; control 66; follow-up period 9 months; follow-up rate 63%; raters probably not independent; collected objective data only
Griffiths, 1974[35]	PVT: rehabilitation programme at the Maudsley Hospital involving industrial workshops Control: community care from usual services	No. not in CE at 18 months	Randomisation: random allocation (category B) PVT 14; control 14; follow-up period 18 months; follow-up rate 100%; unclear if raters independent
Okpaku *et al.*, 1997[36]	PVT: employment orientated case management from a multidisciplinary team of rehabilitation specialists (gradual approach involving sheltered placements) Control: standard case management services	No. not participating in programme; no. in any form of employment at 18 months	Randomisation: random assignment (category B) PVT 73; control 79; follow-up period 3 months; follow-up rate 100%; raters independent
Wolkon *et al.*, 1971[37]	PVT: treatment in non-residential, transitional, social rehabilitation centre for clients recently discharged from hospital; treatment included group work, counselling and transitional work Control: usual community aftercare	No. readmitted to hospital in first year of study	Randomisation: random assignment (category B) PVT 333; control 207; follow-up period 12, 18, 24, 30 months; follow-up rate 92%; raters independent

CE, competitive employment

TABLE 3 *Details of included studies (modified approaches to PVT)*

Study	Intervention	Main outcome measures (data suitable for analysis)	Design
Modification 1: PVT + payment versus PVT alone			
Bell *et al.*, 1996[38]	Experimental group: participated in TE programme on a general hospital site; paid for work up to 20 hours/week Control group: participated in the same programme but were unpaid	No. in any type of employment at 5 months; mean hours/month in employment; mean earnings/month; drop-out rate; no. readmitted to hospital; symptoms	Randomisation: "randomised" method not specified (category B) Paid PVT 80; unpaid PVT 70; follow-up period 5 months; follow-up rate 96%; raters independent

continued

TABLE 3 contd *Details of included studies (modified approaches to PVT)*

Study	Intervention	Main outcome measures (data suitable for analysis)	Design
Modification 2: PVT + psychological interventions versus PVT alone			
Blankertz and Robinson, 1996[39]	Experimental group: received counselling from two employment specialists who used social learning techniques, group sessions and rewards for passing up a "ladder" of success Control group: usual community care	No. in CE; no. not participating in programme; no. in any form of employment or education	Randomisation: random allocation (category B) PVT + counselling 61; PVT control 61; follow-up period 9 months; follow-up rate 100%; raters independent
Kline and Hoisington, 1981[40]	Experimental group: attended employment group that met for 1.5 hours/week for 12 weeks to discuss work values; VR counsellors were group facilitators Control group: received usual VR service (PVT)	No. obtaining CE; no. not participating in programme	Randomisation: random assignment (category B) PVT + counselling 10; control 10; follow-up period 18 months; follow-up rate 95%; raters not independent
Modification 3: accelerated entry into TE versus gradual entry into TE			
Bond and Dincin, 1986[41]	Experimental group: immediate placement in paid TE (minimum 2 days/week) Control group: remained in unpaid prevocational work crew for a minimum of 4 months, followed by placement in TE	No. in CE; no. in any employment; monthly earnings; no. not participating in programme; no. rehospitalised; no. in any form of employment or education	Randomisation: random assignment (category B) Immediate placement 64; gradual approach 67; follow-up period 4, 9, 15 months; follow-up rate 82%; raters independent

TABLE 4 *Details of included study (SEm versus standard community care)*

Study	Intervention	Main outcome measures (data suitable for analysis)	Design
Chandler *et al.*, 1996[42]	SEm: care from integrated services agency that included: (1) assertive community treatment; (2) employment programme based at central site (with immediate entry into employment opportunities (cafe, store, catering service, client bank, janitor service)); (3) two staff to develop competitive jobs and support clients (SEm). Finding employment was the key value of programme Control: usual community services including limited case management	No. not participating in programme (at about 1 year); no. not in CE at 1, 2 and 3 years; no. not in any form of employment at 1 year; no. admitted to hospital during study; mean income/month	Randomisation: random allocation (category B) SEm 127; control 129; follow-up period 12, 24, 36 months; follow-up rate 79% at 12 months, 71% at 36 months; raters independent

TABLE 5 *Details of included studies (SEm versus PVT)*

Study	Intervention	Main outcome measures (data suitable for analysis)	Design
Drake *et al.*, 1994[43]	SEm: IPS programme, which included employment specialists attached directly to clinical teams, who helped clients to find jobs immediately and provided on-job training and supportive follow-up Control: pre-employment preparation group involving: (1) discussions about skills needed to obtain and keep jobs; (2) practising these skills; (3) exploration of work-related values and clients' strengths and weaknesses as workers; (4) interview skills meetings; (5) discussion of job leads and interviews (meetings 2/week) In addition: both groups received usual mental health services	No. not participating in programme; no. in CE at 3, 6, 9, 12, 15, 18 months; mean income	Randomisation: randomly assigned (category B) SEm 74; PVT 69; follow-up monthly for 24 months; follow-up rate 98%; raters independent
Drake *et al.*, 1999[44]	SEm: IPS programme emphasising rapid job search and on-job support once job is secured; involved 3 employment specialists with a caseload of 25 each Control: received PVT and paid work adjustment training in a sheltered workshop In addition: both groups received usual mental health services	No. not in CE at 4, 6, 9, 12, 15, 18 months	Randomisation: random numbers table (category A) SEm 76; PVT 76; follow-up period 6, 12, 18 months; follow-up rate 95% at 18 months; raters independent

continued

TABLE 5 contd *Details of included studies (SEm versus PVT)*

Study	Intervention	Main outcome measures (data suitable for analysis)	Design
Bond *et al.,* 1995[45]	SEm: rapid job search with job coaching and follow-on support from employment specialists based in a rural and an urban community mental health centre Control: at least 4 months' preparation in work-readiness training	No. not participating in programme (at about 1 year); number in CE at 1, 2, 4 years; mean earnings/month	Randomisation: random assignment (category B) SEm 43; PVT 43; follow-up period 12, 24, 48 months; follow-up rate 86% at 12 months; raters were not independent, although data collected concerned objective outcomes
Gervey and Bedell, 1994[11]	SEm: received immediate placement in CE, with support provided by either job coaches or a family/peer support group Control: employment training in a sheltered workshop setting with weekly individual, family and group therapy	No. not in CE at 12 months	Randomisation: random assignment (category B) SEM 22; PVT 12; follow-up period 12 months; follow-up rate 100%; raters independent
McFarlane *et al.,* 2000[46]	SEm: received family-aided assertive community treatment, which included vocational specialists who provided help with job searching and support once in employment Control: referral to traditional state VR service practising PVT and making use of placements in sheltered workshops	No. obtaining CE; no. obtaining any form of employment; no. not participating in programme; monthly earnings	Randomisation: random assignment (category B) SEm 37; PVT 32; follow-up period 3-monthly for 18 months; follow-up rate 100%; raters independent

TABLE 6 *Excluded studies*

Study	Reason for exclusion
Adams-Shollenberger and Mitchell, 1996[47]	Case-control study concerned only with clients with learning difficulties
Azrin and Philip, 1980[48]	RCT of the job club method of finding employment; subjects had a mix of intellectual and physical handicaps
Becker *et al.,* 1999[49]	Not an RCT
Bell and Ryan, 1984[50]	Quasi-experimental comparison of a hospital-based VR programme with 2 other intensive treatment units
Bond *et al.,* 1990[51]	RCT of assertive community treatment, which is not a specific approach to VR; effect on unemployment state is summarised in the systematic review of assertive community treatment published elsewhere (Cochrane Library[25])
Briggs and Yater, 1966[52]	Appears to be an RCT of PVT versus standard community care; however it is difficult to follow. The number of patients randomised appears to be fewer than the number followed up. Two different figures are given for the number recruited. Pending clarification, this study was excluded. As presently reported, it does not contain any usable data
Chandler *et al.,* 1996[42]	RCT of assertive community treatment trial, described separately in the article by Chandler *et al.,* 1996[42], which was included because assertive community treatment was combined with a specific employment intervention (SEm)
Clark *et al.,* 1996[53]	Quasi-experimental comparison of a day care centre with a former day care centre converted to an SEm programme
Fabian, 1992[54]	Case-control study comparing two groups of patients with severe mental disorder, the first in SEm, the second in other community programmes
Faulkner *et al.,* 1986[55]	Case-control comparison of outcome for patients undergoing VR versus patients not receiving such services
Huxley *et al.,* 1999[56]	Case-control study (matched pairs) of clubhouse users versus similar patients in a neighbouring area
Kauffman, 1995[57]	RCT of a PVT approach (self-help employment centre) versus other PVT approaches: the numbers randomised to treatment and control groups were not specified and the control condition was unclear; all controls were referred to other VR services, but it is unclear how many (if any) actually engaged
Keith *et al.,* 1977[58]	RCT of a psychological approach for enhancing the effectiveness of VR; not all the participants were mentally ill
Kregel *et al.,* 1989[59]	Not an RCT
Luo and Yu, 1994[60]	Retrospective case-control study of patients with schizophrenia attending a sheltered workshop versus outpatient clinic
McFarlane *et al.,* 1996[61]	RCT of two different types of assertive community treatment; no specific VR component in either intervention
Noble, 1991[62]	Quasi-experimental study

continued

TABLE 6 contd *Excluded studies*

Study	Reason for exclusion
Otero and Rebolledo, 1993[63]	Before and after study comparing standard outpatient care at a mental health centre with a rehabilitation programme
Proudfoot *et al.*, 1997[64]	RCT of an occupational training programme (incorporating cognitive behavioural therapy) versus a programme that emphasised social support; participants were not mentally ill
Sauter and Nevid, 1991[65]	RCT of work skills training for patients with chronic schizophrenia who were attending a sheltered workshop; concerned only with increasing productivity rates, not with employment outcomes
Stein and Test, 1980[66]	RCT of assertive community treatment versus hospital admission; no specific vocational rehabilitation component
Velasquez and Cubbin, 1980[67]	RCT of residential milieu therapy; no specific VR intervention

showed that significantly fewer patients were admitted to hospital amongst those receiving PVT (n = 887; RR = 0.79; 95% CI, 0.65 to 0.95; NNT = 35.2). However, a sensitivity analysis excluding the trial reported by Beard and colleagues[33] (randomisation category C, and non-independent raters) and that by Bond and Dincin[34] (follow-up rate less than 75% and non-independent raters) found no significant difference on this variable. Griffiths[35] reported no difference in self-esteem (Self-confidence Scale[68]) between PVT and control groups (n = 28; PVT mean = 25.5, PVT SD = 6.6; control mean = 23.3; control SD = 7.3).

Costs

One trial[34] reported mean monthly total healthcare costs of $417.90 for PVT and $651.50 for the control group, but no statistical analysis was presented.

Subanalysis: clubhouse approach versus standard community care

Only one trial[33] provided data for this subanalysis. On the primary outcome (number in competitive employment) at 24 months there was no clear difference between the clubhouse approach and the control group (n = 215; RR = 0.95; 95% CI, 0.77 to 1.17).

Other employment outcomes

Beard and co-workers[33] demonstrated no difference between the clubhouse and control groups in numbers obtaining any form of employment at 3, 6 and 12 months.

Clinical outcomes

Beard and colleagues[33] noted significantly fewer admissions to hospital among clients in the clubhouse group compared with controls (n = 215; RR = 0.69; 95% CI, 0.46 to 0.96; NNT = 6.1).

Modification 1: Prevocational training plus payment versus prevocational training alone (*Table 3*)

One trial[38] provided data for this comparison; no data were available on the primary outcome.

Other employment outcomes

At 6-month follow-up, significantly more clients in the payment group were in some form of employment (n = 150; RR = 0.40; 95% CI, 0.28 to 0.57; NNT = 2.2). Clients in the payment group earned significantly more per month (payment group mean = $192; non-payment group mean = $32.03; t = 7.56; $p < 0.0001$).

Clinical outcomes

Significantly more clients from the payment group participated in the programme (n = 150; RR = 0.53; 95% CI, 0.39 to 0.71; NNT = 2.8). There were also significantly fewer admissions to hospital in the payment group (RR = 0.55; 95% CI, 0.31 to 0.96; NNT = 6.4) and they showed significantly better total symptom scores (Positive and Negative Syndrome Scale for schizophrenia[69]: payment group mean = 66.2, payment group SD = 15.1; non-payment group mean = 72.6, non-payment group SD = 15.0; $p < 0.02$).

Modification 2: Prevocational training plus psychological intervention versus prevocational training alone (*Table 3*)

Two trials[39,40] provided data for this comparison. On the primary outcome (number in competitive employment) at 6 months, Kline and Hoisington[40] found no difference between clients receiving the psychological intervention and the control group (n = 20; RR = 0.56; 95% CI, 0.29 to 1.07), whereas Blankertz and Robinson,[39] at 9-month follow-up, found a significant difference in favour of clients receiving psychological interventions (n = 122; RR = 0.90; 95% CI, 0.83 to 0.98; NNT = 10).

Other employment outcomes

One trial[39] reported that clients who were receiving psychological interventions were significantly more likely to be in some form of employment (n = 122; RR = 0.89; 95% CI, 0.81 to 0.97; NNT = 8.7) or in some form of employment, training or education at the end of the study (n = 122; RR = 0.63; 95% CI, 0.52 to 0.77; NNT = 2.8).

Clinical outcomes

Both these trials reported data on numbers not participating in the programme, but found no significant difference between the treatment and control groups (n = 142; RR = 0.85; 95% CI, 0.33 to 2.18).

Modification 3: Accelerated entry versus gradual entry to transitional employment (*Table 3*)

One trial[41] provided data for this comparison. For the primary outcome (number in competitive employment) there was no difference between groups at 9 and 15 months (although there was a trend in favour of accelerated placement that fell just sort of significance at 15 months: n = 131; RR = 0.88; 95% CI, 0.78 to 1.0).

Other employment outcomes

Clients in the accelerated entry to TE group were not more likely to be in any form of employment at 15 months (n = 131; RR = 0.96; 95% CI, 0.69 to 1.33), but they earned more per month (accelerated group mean = $115.3; control group mean = $38.9; no statistical analysis).

Clinical outcomes

There was no difference in participation rates between the two groups at 9 or 15 months.

Supported employment versus standard community care (*Table 4*)

Only one trial[42] provided data for this comparison. On the primary outcome (number in competitive employment) there was no difference between SEm and controls at 12 months (n =256; RR = 1.01; 95% CI, 0.93 to 1.09) but there was a significant difference favouring SEm at 24 months (n = 256; RR = 0.92; 95% CI, 0.85 to 0.99; NNT = 12.6) and 36 months (n = 256; RR = 0.88; 95% CI, 0.82 to 0.96; NNT = 9).

Other employment outcomes

SEm clients were significantly more likely to be in some form of employment at 12 months (n = 256; RR = 0.79; 95% CI, 0.70 to 0.90; NNT = 5.5) and also earned significantly more per month (SEm mean = $60.5; control mean = $26.9; $p < 0.05$).

Clinical outcomes

SEm clients were not significantly less likely to drop out of the programme (n = 256; RR = 0.74; 95% CI, 0.55 to 1.01). There was no difference in the number of hospital admissions between SEm and control clients (n = 256; RR = 0.83; 95% CI, 0.63 to 1.10).

Costs

The mean monthly healthcare costs were significantly higher for clients in the SEm group (SEm mean = $1599; control mean = $527.30), but this finding was difficult to interpret because SEm clients were also receiving assertive community treatment.

Supported employment versus prevocational training (*Table 5*)

Five trials[11,43–46] provided data for this comparison. On the primary outcome (number in competitive employment) there was a difference in favour of SEm at 4, 6, 9, 12, 15 and 18 months (*Figure 1*). For example: at 6 months, n = 364, RR = 0.74; 95% CI, 0.67 to 0.82; and at 12 months, n = 484; RR = 0.76 (95% CI, 0.69 to 0.84); NNT = 4.5 (95% CI, 4.48 to 4.63). At 12 months 34% of the SEm group were employed and 12% of the PVT group. There was no significant heterogeneity on this variable at any time point. In a sensitivity analysis, significant differences remained at all time points up to 18 months after the exclusion of one trial[45] (non-independent raters).

Secondary employment outcomes

It was noted in three trial reports[11,43,44] that clients in SEm spent significantly more hours per month in competitive employment than those receiving PVT (*Table 7*). Three[43,45,46] of four[43–46] trials also revealed that clients in SEm had higher mean monthly earnings that those in the PVT group (*Table 8*).

Clinical outcomes

People were no more likely to participate in SEm programmes than PVT programmes at 6, 12, and 18 months (12-month data analysed using a random effects model because of heterogeneity). Drake and colleagues[43] reported no difference in overall functioning (General Assessment Scale[70]), self-esteem (Rosenberg Scale[71]) or mental state (Brief Psychiatric Rating Scale (BPRS)[72]), but did not present any raw data. Drake's group[44] also reported no significant differences in self-esteem (Rosenberg Scale), quality of life (Lehman's Scale[73]) or psychiatric symptoms (BPRS) at 6, 12 and 18 months.

Costs

Bond and co-workers[45] reported that the programme costs of SEm were greater than for PVT, but that other healthcare costs were reduced (no statistical analysis), so that the overall healthcare costs were less for SEm. Drake and colleagues[43] found no significant difference in programme costs or overall healthcare costs between SEm and PVT (*Table 9*).

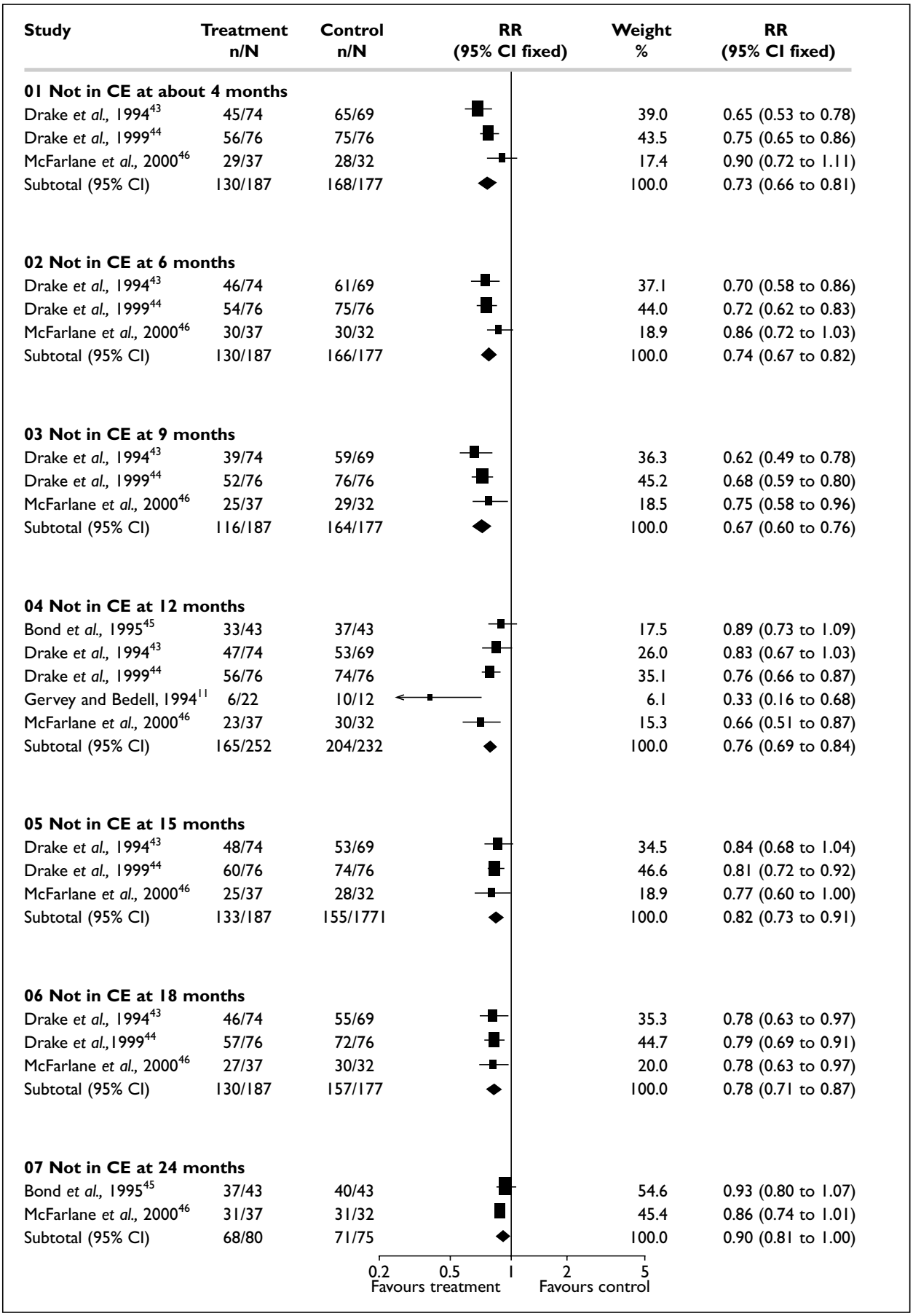

FIGURE 1 *SEm versus PVT: number in CE*

Subanalysis: Individual placement and support versus prevocational training

Two trials[43,44] provided data for this comparison, in which IPS was considered as a type of SEm. On the primary outcome (number in competitive employment) there was a difference in favour of IPS clients at 4, 6, 9, 12, 15 and 18 months (*Figure 1*). For example: at 4 months, $n = 295$; RR = 0.71; 95% CI, 0.63 to 0.79; and at 12 months, $n = 295$; RR = 0.79; 95% CI, 0.70 to 0.89; NNT = 5.5. At 12 months 30% of the people allocated to IPS were employed compared with 12% in the PVT group.

Secondary employment outcomes

Both trials[43,44] reported that IPS clients spent significantly more hours per month in competitive employment (*Table 7*). One trial[43] demonstrated significantly higher mean monthly earnings, but the other[44] indicated no difference (although the IPS group earned more from competitive employment; *Table 8*).

Clinical outcomes

IPS clients were not significantly more likely to participate ($n = 295$; RR = 0.52; 95% CI, 0.15 to 1.85, random effects model). There were no significant differences between groups on self-esteem, mental state, overall functioning, or quality of life at any time point.

Costs

Drake and colleagues[43] found no significant difference in programme costs or overall healthcare costs between IPS and PVT (*Table 9*).

Discussion

The main finding of this review was that, for patients with severe mental disorder who wanted to work, SEm is more effective than PVT. The evidence to support this was strong: five randomised trials ($n = 484$) showed that people in SEm were significantly more likely to be in competitive employment at six time points across 18 months. There was no evidence of heterogeneity at any time point, nor was the finding substantially altered by sensitivity analysis. The main finding was supported by the data from secondary employment outcomes, which showed that clients in SEm worked more hours and had higher monthly earnings. On the basis of the limited data available, SEm also appears to be superior to standard community care on the primary outcome (finding competitive employment), again supporting the main finding.

The data on the characteristics of participants in SEm versus PVT trials are summarised in *Table 10* (see the Cochrane Library version of this review for full data on trial participants[74]). These show that trials of SEm versus PVT have shown good recruitment of women, ethnic minorities and people suffering from schizophrenia. This suggests that the main finding of the review can be applied with confidence to the general population of people with severe mental disorders. The generalisability of the main finding is, however, limited by the fact that all relevant trials were conducted in the USA. This limitation makes it uncertain how far the findings can be generalised to countries with less dynamic economies, different welfare structures, and dissimilar cultural attitudes to work.

This review revealed no evidence to suggest that PVT was more effective on the primary outcome (number in competitive employment) than standard community care or hospital care. It was of interest that clients on PVT programmes were significantly less likely to be admitted to hospital than those receiving standard community care. However, this finding was not robust to a sensitivity analysis excluding poorer quality trials. Moreover, all trials in this comparison involved interventions that offered comprehensive psychosocial rehabilitation in addition to PVT, so that it cannot be assumed that it was PVT *per se* that was responsible for the reduction in hospitalisation. The data on reduced admission rates should therefore be treated with caution.

There was little evidence in this review that either SEm or PVT improved symptoms, quality of life or social functioning. However, because only a minority of participants in VR trials actually find competitive employment (about one-third in the most effective SEm trials), a very large sample would be required to detect clinically significant improvements. There was some indication of symptomatic improvement amongst those clients who actually worked. For example, Bell and colleagues[38] identified a significant improvement in symptoms after financial inducements had ensured a high participation rate in the treatment group, although Drake and co-workers[43] presented a subanalysis of mental state data showing a significant improvement in clients who obtained competitive work.

There was also some evidence that payment improved engagement in PVT programmes and enhanced their effectiveness. The effect of psychological interventions to enhance motivation was less clear, although there were some promising

TABLE 7 *SEm versus PVT – mean hours per month in CE*

Study	Intervention	Mean hours in CE	t- or F-value	p-value
Drake *et al.*, 1994[43 a]	SEm PVT	33.7 11.4	$t = 3.7$	<0.0001
Drake *et al.*, 1999[44 a]	SEm PVT	17.9 1.5	$t = 4.4$	<0.001
Gervey and Bedell, 1994[11]	SEm PVT	69.0 9.9	$F = 3.7$	0.03

[a] *IPS trials*

TABLE 8 *SEm versus PVT – mean earnings per month ($)*

Study	Intervention	Mean monthly earnings ($)	t-value	p-value
Bond *et al.*, 1995[45]	SEm PVT	99.9 60.7	2.75	<0.01
McFarlane *et al.* 2000[46]	SEm PVT	41.9 11.8	2.35	0.019
Drake *et al.*, 1994[43 a]	SEm PVT	188.5 59.9	3.34	<0.001
Drake *et al.*, 1999[44 a]	SEm PVT	111.1 111.4	4.29	NS

[a] *IPS trials*
NS, not significant

TABLE 9 *Costs of care (mean monthly cost (US$) per patient)*

Study	Group	Programme costs	Other health costs	Overall cost
Bond *et al.*, 1995[45]	SEm PVT	251.6 132.0	263.0 586.5	514.6 718.5
Drake *et al.*, 1994[43 a]	SEm PVT	313.1 307.3	801.6 928.5	1114.7 1235.8

[a] *IPS trial*

TABLE 10 *Characteristics of clients in SEm versus PVT trials*

Study	Age (years)	Female (%)	Ethnic minority (%)	Schizophrenic (%)	Ever married (%)
Bond *et al.*, 1995[45]	24.5	31	25	55	52
McFarlane *et al.*, 2000[46]	32.9	30.4	7	65.1	26
Drake *et al.*, 1994[43 a]	37	51.7	5	47	49.7
Drake *et al.*, 1999[44 a]	39.4	61.2	82.9	67	34.2
Gervey and Bedell, 1994[11]	19	33	83	?[b]	?

[a] *IPS trials*
[b] *All suffered from "severe mental disorder"*

indications. There was insufficient evidence to judge whether clubhouses were more effective than other approaches to PVT. Although IPS appeared to be an effective form of SEm, there were insufficient data to say whether it was superior to other less carefully specified forms of SEm.

Conclusions

Implications for practice

The evidence suggests that SEm is the most effective way to help severely mentally ill people to obtain competitive employment. This finding would seem to imply that people with mental disorders who want to work should be offered the option of SEm. Based on the evidence, it would be justified for commissioning agencies to encourage VR providers to develop more SEm schemes.

Implications for research

This review has identified five avenues for further research in VR. These are listed in order of priority below:

1. SEm needs to be developed and evaluated outside the USA, particularly in countries with high rates of unemployment or more extensive welfare systems (where the effectiveness of SEm may be affected by such factors as the "poverty trap").
2. The cost-effectiveness of SEm needs to be examined in larger, multicentre trials, both within and outside the USA. Researchers who are planning future trials of SEm should consider standardising the intervention by adhering to the carefully specified IPS model.
3. There is a case to be made for surveying existing VR agencies to determine the extent to which the most effective interventions are being offered.
4. Research is required to determine how far mental state and social outcome may be improved by working. Methodological considerations may mean that such research may have to take place outside the framework of RCTs.
5. Research is also required to determine how far PVT (including the clubhouse approach) affects readmission rates under modern conditions.

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Chapter 3

Day hospital versus outpatient care for patients with psychiatric disorders

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List of abbreviations

CAS	Community Adaptation Scale
CI	confidence interval
DCC	day care centre*
DTC	day treatment centre*
DTP	day treatment programme*
ITT	intention-to-treat*
RCT	randomised controlled trial
RR	risk ratio
SCL	Symptom Check List
SD	standard deviation
SMD	standardised mean difference

* Used only in tables

Executive summary

Background

This review considers the use of day hospitals as an alternative to outpatient care. Two types of day hospital provision are covered: "day treatment programmes" and "day care centres". Day treatment programmes are day hospitals that are used to enhance the treatment of patients with anxiety or depressive disorders who have failed to respond to outpatient care. Day care centres are day hospitals that offer structured support to patients with long-term severe mental disorders who would otherwise be treated in an outpatient clinic.

Objectives

There were two objectives: first, to assess the effectiveness of day treatment programmes versus outpatient care for people with non-psychotic disorders; and, secondly, to assess the effectiveness of day care centres versus outpatient care for people with severe long-term disorders.

Methods

Study selection

Eligible studies were randomised controlled trials comparing day hospital care (either a day treatment programme or a day care centre) with outpatient care. Studies were ineligible if they were largely restricted to patients who were aged under 18 or over 65 years or who had a primary diagnosis of substance abuse or organic brain disorder.

Data sources

Relevant trials were identified from searches of the Cochrane Controlled Trials Register, MEDLINE, EMBASE, CINAHL, PsycLIT, and the reference lists of all identified studies and review articles. Researchers were approached to identify unpublished studies. Trialists were asked to provide individual patient data.

Data extraction

All data were extracted independently by two reviewers and cross-checked.

Data synthesis

Relative risks and 95% confidence intervals were calculated for dichotomous data. Standardised mean differences were calculated for continuous data.

Results

There was evidence from two of the five trials identified suggesting that day treatment programmes were superior to continuing outpatient care in terms of improving psychiatric symptoms. There was no evidence to suggest that day treatment programmes were better or worse than outpatient care on any other clinical or social outcome variable or on costs. There was no evidence that day care centres were better or worse than outpatient care on any clinical or social outcome variable. There were some inconclusive data on costs suggesting that day care centres could be more expensive than outpatient care.

Conclusions

There was some limited evidence to support the use of day treatment programmes for patients with anxiety or depression who have not responded to standard outpatient treatment. Future research should address the feasibility of day treatment programmes and how far they are cost-effective against other alternatives, such as outpatient cognitive behavioural therapy. There was no evidence to support the use of day hospitals as day care centres.

Day hospital versus outpatient care for patients with psychiatric disorders

Background

Psychiatric day hospitals were originally developed as an alternative to inpatient care,[1] but they are also commonly used as an alternative to outpatient care for two groups of patients.

The first group comprises those whose symptoms have failed to respond to outpatient treatment. It has been proposed that such patients (who usually suffer from depression or anxiety disorders) may experience greater symptomatic improvement with the more intensive input offered by day hospitals.[2,3] The term "day treatment programme" is usually applied to day hospitals used in this way.[4] Critics of day treatment programmes have argued that patients with neurotic disorders find them "neither congenial nor especially helpful".[5]

Patients in the second group are those with severe long-term disorders (usually schizophrenia).[6,7] It has been proposed that such patients could experience closer engagement,[8] improved clinical outcome,[8] and a reduced readmission rate,[2,9] when given access to the structured support and range of treatments offered by a day hospital. The term "day care centre" is usually applied to a day hospital that is used in this way.[4] Critics of day care centres have argued that they "institutionalise" patients, and fail to provide focused, effective treatment.[7,10,11]

Research question

This review had two main objectives. The first was to assess the effectiveness of day treatment programmes as an alternative to continuing outpatient care for people with non-psychotic disorders. The main hypothesis was that admission to a day treatment programme would result in a better clinical outcome (mental state, social functioning or quality of life) without increasing the costs of care. In addition, the review considered patients' satisfaction with care, their engagement in treatment and their use of inpatient care.

The second objective was to assess the effectiveness of day care centres as an alternative to outpatient care for people with severe long-term mental disorders. The main hypothesis was that day treatment centres would: (1) increase the numbers remaining in contact; (2) reduce the number and duration of admissions to hospital; (3) improve clinical outcome; and (4) reduce the costs of care. In addition, the review considered patients' satisfaction with care.

Methods

Inclusion criteria

Design

Eligible studies were randomised controlled trials (RCTs) comparing day hospital care (either day treatment programme or day care centre) versus outpatient care. For a study to be eligible it had to contain data that could be analysed on an intention-to-treat basis.

Participants

For studies of day treatment programmes, participants were patients with non-psychotic disorders (all diagnoses) who would have been treated by outpatient care had day hospital care not been available. Studies were not eligible if they were restricted to, or included a majority of, patients who were aged under 18 or over 65 years, or had a primary diagnosis of substance abuse or organic brain disorder. It was not necessary for participants to be "refractory to outpatient treatment" because there is no generally agreed definition of this term. However, the reviewers recorded the entry criteria for each day treatment programme and took these into consideration in the analysis of results.

For studies of day care centres, participants were patients with severe long-term disorders (predominantly schizophrenia and other psychoses) who would have been followed up by outpatient care had day hospital care not been available. Studies were not eligible if they were restricted to, or included a majority of, patients who were aged under 18 or over 65, or had a

primary diagnosis of substance abuse or organic brain disorder.

Types of intervention

Day treatment programmes were defined as: "psychiatric day hospitals offering intensive input to patients with non-psychotic disorders". As in the introduction to this report, the term "day hospital" was defined as a "multidisciplinary day care facility offering comprehensive psychiatric care, where: (1) "multidisciplinary" means involving, as a minimum, psychiatrists and nurses; (2) "day care facility" means a building that is open during working hours on weekdays; and (3) "comprehensive psychiatric care" means the diagnostic, medical, psychiatric, psychosocial and occupational treatments that would normally be available to psychiatric inpatients. Day care centres were defined as "psychiatric day hospitals offering continuing care to patients with severe mental disorders".

Outcome measures

For the comparison of day treatment programmes against outpatient care for patients with non-psychotic disorders, the main outcome measures were:

- Clinical and social outcomes:
  - mental state
  - social functioning
  - quality of life
  - death (all causes)
  - burden on relatives
- Costs of care:
  - mean monthly cost of psychiatric care (comprising cost of hospital care plus cost of all ambulatory psychiatric care)
  - mean monthly cost of all care (comprising cost of psychiatric care plus cost of other medical/social care, minus benefits such as wages)
- Other secondary outcome measures were:
  - number of patients refusing to enter a trial because they were unwilling/unable to attend a day hospital
  - number lost to follow-up
  - number admitted to inpatient care
  - mean number of days in inpatient care
  - satisfaction with care.

For the comparison of day care centres against outpatient care for patients with severe long-term disorders, the main outcome measures were:

- Engagement with treatment:
  - number lost to follow-up
- Readmission to hospital:
  - number admitted to inpatient care
  - mean days in inpatient care
- Clinical outcomes:
  - mental state
  - social functioning
  - quality of life
  - death
  - burden on relatives
- Cost of care:
  - mean monthly cost of psychiatric care (comprising cost of hospital care plus cost of all ambulatory psychiatric care)
- Other secondary outcome measure:
  - satisfaction with care.

Search strategy

The search began by deriving a list of search terms from reading overviews of the field and consulting experts in day hospital care. This led to the following free-text search strategy: (DAY HOSP*) or (DAY CARE) or (DAY TREATMENT*) or (DAY CENT*) or (DAY UNIT*) or (PARTIAL HOSP*) or (AMBULATORY TREATMENT) or (AMBULATORY CARE) or (DISPENSARY).

This search string was then combined with the MeSH term (MENTAL ILLNESS) and with the Cochrane Collaboration's search string for potential trials and reviews.[12] The combined search string was then run on the following databases: Cochrane Controlled Trials Register;[13] MEDLINE (1966 – December 1998); PsycLIT (1966 – December 1998); EMBASE (1980 – December 1998) and CINAHL (January 1982 – December 1998). The reference lists of all identified trials and reviews were scanned for references to additional trials. Experts in the field were approached to identify unpublished trials.

Selection of trials

The search for trials was performed by two reviewers (MM and AA). Inspection of the citations identified in the search outlined above was performed independently by the same two reviewers. Potentially relevant abstracts were identified and full articles ordered. Trials meeting the inclusion criteria were rated for methodological quality. A reliability study found complete agreement on which trials met the inclusion criteria.

Quality assessment

Quality was assessed independently by two reviewers. Each reviewer allocated the included trials to one of three categories of allocation concealment, as described in the Cochrane Collaboration handbook.[12] Disagreements were resolved by discussion, or, failing this, by seeking further information from the trialists. Only trials in category A or B were included in the review

(i.e. randomised trials where the method of allocation concealment was either adequate or unclear). Trials were also rated on the degree of blindness. The blinding of patients and treating clinicians is not possible in trials of day hospital treatment, but trials were rated on independence and blinding of evaluators (non-independent evaluators being defined as being also involved in the treatment of trial patients). A sensitivity analysis was performed with the exclusion of trials conducted by non-independent or non-blind evaluators.

Data extraction

Data were extracted independently by three reviewers (MM, AA and RC) and cross-checked. Where further clarification was needed the authors of trials were contacted and asked to provide missing data. Data from studies in which more than 50% of the participants in any group were lost to follow-up (except for the outcome of "lost to follow-up") were excluded. The impact of including studies with high attrition rates (25–50%) was analysed in a sensitivity analysis. If the inclusion of data from this latter group resulted in a substantive change to the estimate of effect, these data were presented separately, rather than being added to those of trials with lesser attrition rates. Individual patient data were not generally sought for this review; however, one author (PT) provided such data.

Unpublished scales are known to be subject to bias in trials of treatments for schizophrenia.[14] Therefore, continuous data from rating scales were included only if the measuring instrument had been described in a peer-reviewed journal and the instrument was either a self-report or completed by an independent rater or relative (not the therapist).

Data synthesis

For binary outcomes, a standard estimation of the relative risk (RR) and its 95% confidence interval (CI) was calculated. The number-needed-to-treat statistic was also calculated. If heterogeneity was found, a random effects model was used. For continuous outcomes, a standardised mean difference (SMD) between groups was estimated. Continuous data presented without summary statistics (i.e. mean, standard deviation (SD)/standard error (SE) or non-parametric equivalent) were not considered valid, although the existence of such data was noted in the text.

A chi-squared test was used, as well as visual inspection of graphs, to investigate the possibility of heterogeneity. A significance level of less than 0.10 was interpreted as evidence of heterogeneity. If heterogeneity was identified, the data were re-analysed using a random effects model. If this made a substantial difference, the studies responsible for heterogeneity were presented separately from the main body of homogeneous trials and the reasons for heterogeneity were investigated. There were insufficient data available to address the question of publication bias. If this had not been so, they would have been entered into a funnel graph (trial effect against trial size) in an attempt to investigate the likelihood of overt publication bias.[15]

Details of studies included in the review

Five trials were included in the review.[16–20] Two were of day treatment programmes versus outpatient care[16,19] (n = 202) and three were of day care centres versus outpatient care[17,18,20] (n = 272). Details of the included trials are given in *Tables 1* and *2* (for further details see the Cochrane Library version of the review[21]). One trial[18] provided data only on number lost to follow-up. The remaining data from this trial were excluded because they were not collected on an intention-to-treat basis (patients who failed to engage in treatment were excluded from follow-up, as were those who were in hospital at the time of follow-up).

Randomisation

Both day treatment programme trials[16,19] were randomised by sealed envelope (allocation concealment quality A). Two of three day care centre trials were in allocation concealment category A, the first randomised by sealed envelope[17] and the second by a random numbers table.[18] In the third day care centre trial[20] the randomisation method was not specified (allocation concealment quality B).

Blinding to interventions and outcomes

Blinding of patients and clinicians is not possible in trials of day hospital care, although it is possible to use evaluators who are independent of the treating clinicians and blind to group allocation. In both day treatment programme trials, evaluators were independent and blind to group allocation.[16,19] In one day care centre trial, evaluators were independent and blind to treatment allocation,[18] while in the remaining two trials[17,20] it was unclear whether evaluators were independent or blind.

Follow-up

Day treatment programmes

Dick and colleagues[16] achieved a good follow-up

TABLE 1 *Day treatment programmes: details of included studies*

Study	Intervention	Outcomes	Design
Dick *et al.*, 1991[16]	DTP: day hospital specialising in treatment of patients with severe neurotic disorders; offering problem-orientated approach, time structuring and behavioural programmes; staffing ratio of 1:12 Control: outpatient care; seen monthly for medication and anxiety management	No. lost to follow-up; no. of hospital admissions; mental state; satisfaction with care	Randomisation: sealed envelope (category A) DTP 46; control 50; follow-up 0, 6 months; drop-out rate 4% at 6 months; ITT analysis; evaluator independent of treating clinician and blind to group allocation (blindness not evaluated)
Tyrer *et al.*, 1979[19]	DTP: two day hospitals, one specialising in treatment of neurotic disorders (well staffed with psychotherapeutic orientation), the other providing standard day hospital treatment (data from the two groups combined for the purpose of this review) Control: routine outpatient care	No. lost to follow-up; no. admitted to hospital (at 8, 24 months); deaths; social functioning; satisfaction with care	Randomisation: sealed envelope (category A) DTP 48; control 58; follow-up 4, 8, 24 months; drop-out rate at 24 months 26%; ITT analysis; evaluators independent and blind to group allocation (not tested); data analysed blind to group allocation (information from trialist)

DTP, day treatment programme; ITT, intention-to-treat

TABLE 2 *Day care centres: details of included studies*

Study	Intervention	Outcomes	Design
Linn *et al.*, 1979[17]	DCC: 10 Veteran's Affairs day hospitals that aimed to enhance social functioning in chronically ill patients by offering a place to socialise and engage in productive activities; employed social workers and physicians, and offered: recreational activities, group therapy, counselling, occupational therapy and medication Control: outpatient drug management from same physicians who were offering medication follow-up in DTC; no other aftercare	No. admitted to hospital; mean no. of days in hospital; social functioning; mental state; costs of care	Randomisation: sealed envelope (category A) DCC 80; control 82; follow-up at 6, 12, 18, 24 months; drop-out rate 15% at 24 months; unclear if evaluator was independent of DTC, or blind to allocation
Meltzoff and Blumenthal 1966[18 a]	DCC: day hospital veterans with neuropsychiatric disabilities who had spent time in hospital; offered individual and group psychotherapy and medication follow-up Control: standard outpatient care	No. lost to follow-up	Randomisation: random numbers table (category A) DCC 40; control 40; follow-up at 0, 3, 6, 9, 12, 15, 18 months; drop-out rate 13.7%; not an ITT analysis (patients were excluded from further follow-up if they dropped out early; only data on no. lost to follow-up was useable; evaluators independent of treating clinicians
Weldon *et al.*, 1979[20]	DCC: day hospital where patients attended 5 days/week for group and goal directed therapy; patient to staff ratio 2:5 Control: outpatient care; psychotherapy orientated	No. lost to follow-up; no. admitted to hospital; social functioning	Randomisation: "randomly assigned" but method unclear (category B) DCC 15; control 15; follow-up at 3 months; drop-out rate 0%; ITT analysis; unclear if evaluators were independent or blind to group allocation

[a]Meltzoff and Blumenthal[18] provided data only on no. lost to follow-up. The remaining data from this trial were excluded because they were not collected on an ITT basis (patients who failed to engage in treatment were excluded from follow-up, as were those who were in hospital at the time of follow-up)
DCC, day care centre; DTC, day treatment centre

rate of 96% at 6 months. Tyrer and co-workers[19] achieved a follow-up rate of 84% at 8 months and 74% at 2 years. No randomised patients were excluded from the analysis. The reasons why patients were lost to follow-up were clearly reported in one of these articles,[19] but not in the other.[16]

Day care centres

Linn and colleagues[17] achieved a follow-up rate of 85% at 24 months. Meltzoff and Blumenthal achieved an 86.3% follow-up rate at 18 months (which might have been greater if an attempt had been made to follow up patients who failed to attend for treatment). Weldon and co-workers[20] achieved a follow-up rate of 100% at 3 months. The reasons why patients were lost to follow-up were clearly reported by Linn and colleagues,[17] but only partially reported by Meltzoff and Blumenthal.[18]

Details of studies excluded from the review

Table 3 shows all the studies that were excluded from the review, with their reasons for exclusion. Forty-five studies were excluded;[9,10,22–64] 26 were

non-randomised studies and 19 were RCTs. The non-randomised studies consisted of: two surveys (without comparison groups); 11 cross-sectional comparisons; four uncontrolled "before and after" comparisons; and nine quasi-experimental designs (i.e. comparative trials where no attempt was made to randomise). The excluded RCTs consisted of: one trial of admission to hospital versus outpatient care; 11 trials of acute day hospital care versus admission; five trials of transitional day hospital care (to reduce the duration of admission); and two trials of day treatment versus outpatient care for which data on an intention-to-treat basis was not collected.[42,58] In the first of these last two trials,[42] data were not collected on an intention-to-treat basis because patients who did not attend for treatment at the day hospital or in the outpatient group were automatically excluded from follow-up. Moreover, in this study, the follow-up period was variable, because it began only after patients had "completed treatment". In the second trial,[58] data were not collected on an intention-to-treat basis because patients were paired before randomisation and when a patient dropped out of treatment the matching control was also excluded.

Results of the review

Day treatment programmes versus outpatient care for patients with non-psychotic disorders

No usable data were available on quality of life, burden on relatives, costs of care, or mean number of days in inpatient care.

These two trials[16,19] both reported improvements in mental state scores favouring the day treatment group. In one,[16] this difference was statistically significant (day treatment median at baseline = 35, at 6 months = 21; control at baseline = 36, at 6 months = 32; $p < 0.001$ Mann–Whitney U-test), whereas, in the other[19] it was not (change in scores at 4 months' day treatment = –13.03, SD = 11.45; control = –9.30, SD = 12.42; SMD = –0.31; 95% CI, –0.73 to 0.11; change in scores at 8 months' day treatment = –14.56, SD = 13.85; control = –11.85, SD = 12.53; SMD = –0.20; 95% CI, –0.63 to 0.22).

One trial[19] revealed no significant difference in social functioning (change in scores from baseline), although the direction of the effect favoured the day treatment group (change in scores at 4 months' day treatment = –10.62, SD = 12.12; control = –7.38, SD = 10.87; SMD = –0.28; 95% CI, –0.70 to 0.14; change in scores at 8 months' day treatment = –13.42, SD = 12.12; control = –9.04, SD = 11.03; SMD = –0.34; 95% CI, –0.76 to 0.08).

The data from one trial[19] showed a non-significant increase in mortality in the day treatment groups, but CIs were very wide (RR = 2.42, 95% CI, 0.23 to 25.85).

Dick and colleagues[16] assessed 124 referred patients and randomised 96 (77.4% suitable for day treatment). Eight patients specifically refused to attend day treatment (6.5%). Tyrer and co-workers[19] assessed 264 patients and randomised 106 (40% suitable), but it was unclear how many of them specifically refused to attend day treatment. Data from both trials showed no significant difference in numbers lost to follow-up at 6–8 months, however CIs were wide (RR = 1.08; 95% CI, 0.49 to 2.38). Data from one trial[19] again showed no difference in follow-up rates at 24 months (RR = 1.61; 95% CI, 0.85 to 3.07).

Data from both trials showed no significant difference in the number admitted to hospital at 6–8 months; however, there was evidence of heterogeneity on this variable. Re-analysis using a random effects model found no significant difference, but CIs were very wide (RR = 1.23; 95% CI, 0.06 to 25.5). At 24 months, one trial[19] indicated no significant difference in admission rates; although the direction of effect favoured the control treatment, CIs were wide (RR = 1.81; 95% CI, 0.54 to 6.05).

Data from both these trials showed no significant differences in satisfaction with care at 4–6 months (whether or not those dropping out were counted as dissatisfied). However, there was statistically significant heterogeneity on this variable. One trial[16] indicated that patients were significantly more satisfied with day treatment, while the other[19] demonstrated that they were significantly less satisfied. Analysis using a random effects model showed no significant difference overall, but the value of summating these data is in doubt.

Day care centres versus outpatient care for patients with severe mental disorders

No usable data were available for quality of life, burden on relatives, or satisfaction with care.

Data from one trial[20] showed no significant difference in follow-up rates at 3 months (no patients were lost to follow-up), but CIs were wide (RR = 1.0; 95% CI, 0.02 to 47.38). Another trial indicated no significant difference in follow-up

TABLE 3 *Details of excluded studies*

Study	Reason for exclusion
Austin *et al.,* 1976[22]	Not randomised – survey comparing randomly selected participants from two different day hospitals
Azim *et al.,* 1978[23]	Not randomised – quasi-experimental design comparing patients in a day treatment programme for non-psychotic patients with non-patient controls
Barkley *et al.,* 1989[24]	Not randomised – retrospective study of admission rates at three day care centres
Basker and Turel, 1986[25]	Not randomised – before and after design examining outcome in a multipurpose day hospital in those who dropped out
Beigel and Feder, 1970[26]	Not randomised – survey comparing patients who completed treatment in a multipurpose day hospital with those who dropped out
Bowman *et al.,* 1983[27]	Not randomised – survey examining differences between patients admitted to acute day hospital and inpatient care
Brook, 1973[28]	Not randomised – survey comparing patients treated in a crisis hostel with those treated in inpatient care
Comstock *et al.,* 1985[29]	Not randomised – retrospective multivariate analysis of attenders at a day treatment programme
Creed *et al.,* 1991[30]	Randomised by sealed envelope (however the trialists judged that the randomisation procedure had been compromised) – trial comparing acute day hospital with inpatient care
Creed *et al.,*1989[31]	Not randomised – quasi-experimental design comparing consecutive admissions to acute day hospital and inpatient care
Creed *et al.,* 1990[32]	Randomised – trial of acute day hospital versus inpatient care, not outpatient care
Creed *et al.,* 1997[33]	Randomised – trial of acute day hospital versus inpatient care, not outpatient care
Dick *et al.,* 1985[34]	Randomised – trial of acute day hospital versus inpatient care, not outpatient care
Drake *et al.,* 1994[35]	Not randomised – quasi-experimental design comparing outcome in long-term patients attending a DCC with those attending a Supported Employment programme (previously a DCC)
Ettlinger *et al.,* 1972[36]	Not randomised – retrospective case-control study comparing patients allocated to transitional day hospital care with those remaining in inpatient care
Fink *et al.,* 1978[37]	Not randomised – quasi-experimental study of inpatient care versus acute day hospital care
Glick *et al.,* 1986[38]	Randomised, although method not clear – transitional day hospital care, which is not reviewed here
Grad and Sainsbury, 1968[39]	Not randomised – quasi-experimental design comparing outcome of community care in two towns
Gudeman *et al.,* 1983[40]	Not randomised – before and after design examining outcome for patients with severe long-term mental disorder referred to a DCC
Guidry *et al.,* 1979[9]	Not randomised – before and after design examining outcome for patients with severe long-term mental disorder referred to a DCC
Guillette *et al.,* 1978[41]	Not randomised – cross-sectional study comparing costs of acute day hospital care with costs of inpatient care
Guy *et al.,* 1969[42]	Randomised by sealed envelope, but no ITT analysis – intervention was day hospital treatment versus outpatient care for patients with a variety of disorders referred from unspecified sources
Herz *et al.,* 1971[43]	Randomised by random numbers table – trial of acute day hospital care versus admission, not day hospital versus outpatient care
Herz *et al.,* 1975[44]	Randomised but method not specified – trial of routine inpatient care versus brief inpatient care plus day care, not a comparison of day hospital versus outpatient care
Hirsch *et al.,* 1979[45]	Randomised but method not specified – trial of routine inpatient care versus brief inpatient care versus brief inpatient plus day care, not a comparison of day hospital versus outpatient care
Hogg and Brooks, 1990[46]	Not randomised – survey comparing long-term inpatients with long-term day patients
Kecmanovic, 1985[47]	Not randomised – cross-sectional case-control study comparing discharged inpatients with discharged day patients
Kris, 1965[48]	Randomised – trial of acute day hospital versus inpatient care, not a trial of day hospital versus outpatient care
Kuldau *et al.,* 1977[49]	Randomised – trial comparing rapid discharge from inpatient care versus community transitional system, not a comparison of day hospital versus outpatient care

continued

TABLE 3 contd *Details of excluded studies*

Study	Reason for exclusion
Levenson *et al.,* 1977[50]	Randomised by random numbers tables – trial of treatment in an outpatient clinic versus hospital admission; outpatient clinic did not meet criteria for a day hospital
Lystad, 1958[51]	Not randomised – quasi-experimental design comparing acute day hospital care with inpatient care
Mathai and Gopinath, 1985[52]	Not randomised – survey of patients in inpatient, outpatient and day hospital care
Michaux *et al.,* 1972[53]	Not randomised – quasi-experimental design comparing acute day hospital care with inpatient care
Milne, 1984[54]	Not randomised – quasi-experimental design comparing a day hospital offering behavioural treatment with one offering social milieux therapy
Niskanen, 1974[55]	Not randomised – before and after design examining outcome for patients treated in a day hospital; patient characteristics unclear
Odenheimer, 1965[56]	Not randomised – survey of the relatives of day hospital patients
Penk *et al.,* 1978[57]	Not randomised – quasi-experimental study (using matched controls) of day hospital versus inpatient care for patients with acute psychiatric disorders
Piper *et al.,* 1993[58]	Randomised but not an ITT analysis – intervention was outpatient treatment versus day hospital care for patients with affective and personality disorders
Platt *et al.,* 1980[59]	Randomised – trial of acute day hospital versus inpatient care, not a trial of day hospital versus outpatient care
Schene *et al.,* 1993[60]	Randomised – trial of acute day hospital care versus inpatient care, not a trial of day hospital versus outpatient care
Sledge *et al.,* 1996[61]	Randomised by sealed envelope – trial of acute day hospital with crisis residence versus inpatient care, not a trial of day hospital versus outpatient care
Tantam and McGrath, 1989[10]	Not randomised – quasi-experimental design (using matched controls) comparing a rehabilitation team with a day treatment centre
Washburn *et al.,* 1976[62]	Randomised but method not specified – trial comparing continuing inpatient admission versus discharge to day patient care, not a trial of day hospital versus outpatient care
Wiersma *et al.,* 1995[63]	Randomised – trial of acute day hospital care versus inpatient care, not a trial of day hospital versus outpatient care
Wilder *et al.,* 1966[64]	Randomised – trial of acute day hospital care versus inpatient care, not a trial of day hospital versus outpatient care

rates at 14 months;[18] although the direction of effect favoured the control group, CIs were wide (RR = 1.75; 95% CI, 0.56 to 5.51).

One trial[20] demonstrated no significant difference in admission rates at 3 months (none admitted), but CIs were wide (RR = 1.0; 95% CI, 0.02 to 47.38). Another[17] showed no significant difference in admission rates at 12 or 24 months, although the direction of effect favoured the day centre group (12 months: RR = 0.86; 95% CI, 0.61 to 1.23, 24 months: RR = 0.82, 95% CI, 0.64 to 1.05).

In one trial[17] day centre patients spent significantly fewer days in inpatient care over a 24-month period (day centre 77.9 days, outpatient 95.9 days), however these data were difficult to evaluate because no *p*-value, SD or CIs were reported and the numbers of patients in each group were unclear.

Data from one trial[20] demonstrated no significant difference in mental state at 3 months by using the Symptom Check List (SCL-90);[65] although the effect favoured the control group, CIs were wide (day centre SCL-90 mean score = 1.09, SD = 0.73; control mean = 0.78, SD = 0.70; SMD = 0.42; 95% CI, –0.30 to 1.15). Another trial[17] revealed a significant time by group interaction (analysis of variance) in favour of day centre patients (at 6 months: day centre SCL-90 mean score = 37.4, control mean = 38.1; at 12 months: day centre mean = 36.0, control mean = 36.6; at 18 months: day centre mean = 35.4, control mean = 36.3; at 24 months: day centre mean = 31.3, control mean = 38.4). In point by point comparisons, only the final difference at 24 months was significant ($p < 0.01$, $F = 8.08$). However, these data could not be added to the meta-analysis because the numbers of patients at each follow-up point were not stated and no SDs were reported.

One trial[20] demonstrated no significant difference in social functioning at 3 months (Community Adaptation Scale (CAS)[66]), but CIs were very wide (day centre CAS mean score = 3.58, SD = 0.42; control mean = 3.62, SD = 0.39; SMD = –0.08; 95% CI, –0.81 to 0.62). In another trial,[17] a significant

time by group difference on social functioning (Social Disability Rating Scale) was shown in favour of the day centre group, but univariate comparisons at 6,12,18 and 24 months were not significant. However, these data could not be added to the meta-analysis because the numbers of patients at each follow-up point were not stated and no SDs, *p*-values or CIs were reported.

No difference in mortality at 3 months was reported for one trial[20] (no deaths were reported in either group), but CIs were wide (RR = 1.0; 95% CI, 0.02 to 47.38).

Linn and colleagues[17] reported a 32.8% increase in the mean monthly cost of psychiatric care in the day centre group, but the data were based on the costs of inpatient care and the day treatment centre only; they did not include the costs of outpatient care (day centre $245.6, outpatient $184.8). The difference was reported as not significant but this finding was difficult to evaluate because no SDs were presented.

Discussion

Day treatment programmes versus outpatient care for patients with non-psychotic disorders

There was some evidence from two small trials to suggest that day treatment programmes may be superior to outpatient care in improving psychiatric symptoms in non-psychotic patients who are refractory to outpatient treatment. However, it was not possible to summate the data from these trials in a meta-analysis. Otherwise, there was insufficient evidence to judge whether day treatment programmes were superior to outpatient care in terms of social functioning, death, number lost to follow-up, number admitted to inpatient care, or satisfaction with care. There were no data for the variables of quality of life, burden on relatives, days in hospital, or costs. From the available data it was not possible to judge what proportion of treatment-refractory outpatients would accept treatment in a day hospital.

The data from both trials were of good quality in terms of allocation concealment, follow-up rate, and the use of blind, independent evaluators. However, the generalisability of the research is limited by a lack of clarity over what degree of "treatment resistance" justifies the more intensive therapy provided by a day hospital. Differences in the degree of treatment resistance in the two trials may explain the discrepancy in satisfaction rates. The trial with the stronger criteria for treatment resistance[16] revealed that the patients were significantly more satisfied with day treatment, whereas the trial with less strong criteria[19] demonstrated the opposite.

Day care centres versus outpatient care for patients with severe mental disorders

There was insufficient evidence to judge whether day care centres were superior to outpatient care in terms of: engagement with care, admission rates, clinical outcome, costs or patient satisfaction. Only very limited data were available on numbers lost to follow-up, numbers admitted to inpatient care, mean number of days in inpatient care, mental state, social functioning, death, and costs of psychiatric care. Such cost data as were available suggested that day care centres may be more expensive than outpatient care, but this was not conclusive. No data were available on quality of life, burden on relatives, mean monthly cost of all care, or satisfaction with care.

The quality of the three trials providing data was not poor but it was not optimal. Weldon and colleagues[20] failed to specify the method of randomisation. Meltzoff and Blumenthal[18] provided only data on number lost to follow-up because of post-randomisation exclusions. Linn and co-workers[17] and Weldon and colleagues[20] did not specify whether the evaluators were blind and independent. However, all three trials had good follow-up rates. Lack of evidence precludes further discussion of the generalisability of the findings.

Conclusions

Implications for practice

Day treatment programmes

Patients or clinicians who are intending to make use of day treatment programmes need to consider how far the inconvenience of day treatment is balanced by the weak evidence for its effectiveness. Policy makers must consider how far the cost of providing day treatment programmes can be justified. In particular, they need to consider if it would be more cost-effective to provide specific psychological therapies of proven effectiveness (such as cognitive behavioural therapy) on an outpatient basis. Where day hospital facilities are being used to provide day treatment programmes, clinicians and policy makers should consider whether these resources would be better deployed by offering a treatment of proven effectiveness, such as in an acute day hospital (see review of acute day hospitals in chapter 1 of this report).

Day care centres

There is no evidence to support the use of day hospitals as day care centres. It is hard to judge whether this practice is still widespread. In the research literature, day care centres have been superseded by case management approaches and vocational rehabilitation programmes. However, in everyday clinical care it is still possible that there are day hospitals acting as either dedicated day care centres or offering day care as part of their activities. Policy makers and clinicians need to consider whether this is justifiable, given the lack of research evidence.

Implications for research

It is doubtful if there is a need for further research on day care centres. The main priority is to carry out further RCTs comparing day treatment programmes against active alternatives other than outpatient treatment, such as cognitive behavioural therapy or home-based care. Such research would need to provide a clear definition of "a treatment resistant outpatient" and define precisely the ingredients of a "day treatment programme". The research should include a detailed analysis of the cost-effectiveness of day treatment programmes and the various alternatives.

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Feedback

The HTA Programme and the authors would like to know your views about this report.

The Correspondence Page on the HTA website (http://www.ncchta.org) is a convenient way to publish your comments. If you prefer, you can send your comments to the address below, telling us whether you would like us to transfer them to the website.

We look forward to hearing from you.

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