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Length of hospitalisation for people with severe mental illness

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Abstract

Background—In high income countries, over the last three decades, the length of hospital stays for people with serious mental illness has reduced drastically. Some argue that this reduction has led to revolving door admissions and worsening mental health outcomes despite apparent cost savings, whilst others suggest longer stays may be more harmful by institutionalising people to hospital care.

Objectives—To determine the clinical and service outcomes of planned short stay admission policies versus a long or standard stay for people with serious mental illnesses.

Search methods—We searched the Cochrane Schizophrenia Group's register of trials (July 2007).

Selection criteria—We included all randomised trials comparing planned short with long/ standard hospital stays for people with serious mental illnesses.

Data collection and analysis—We extracted data independently. For dichotomous data we calculated relative risks (RR) and their 95% confidence intervals (CI) on an intention-to-treat basis

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Nisreen Alwan updating the review: searching, trial selection, data extraction from newly included studies, analysis, completion of report

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None known.

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based on a fixed effects model. We calculated numbers needed to treat/harm (NNT/NNH) where appropriate. For continuous data, we calculated fixed effects weighted mean differences (WMD).

Main results—We included six relevant trials. We found no significant difference in hospital readmissions between planned short stays and standard care at one year (n=651, 4 RCTs, RR 1.26 CI 1.0 to 1.6). Short hospital stay did not confer any benefit in terms of 'loss to follow up compared with standard care (n=453, 3 RCTs, RR 0.87 CI 0.7 to 1.1). There were no significant differences for the outcome of 'leaving hospital prematurely' (n=229, 2 RCTs, RR 0.77 CI 0.3 to 1.8). More post-discharge day care was given to participants in the short stay group (n=247, 1 RCT, RR 4.52 CI 2.7 to 7.5, NNH 3 CI 2 to 6) and people from the short stay groups were more likely to be employed at two years (n=330, 2 RCTs, RR 0.61 CI 0.5 to 0.8, NNT 5 CI 4 to 8). Economic data were few but, once discharged, costs may be more for those allocated to an initial short stay.

Authors' conclusions—The effects of hospital care and the length of stay is important for mental health policy. We found limited data, although outcomes do suggest that a planned short stay policy does not encourage a 'revolving door' pattern of admission and disjointed care for people with serious mental illness. More large, well-designed and reported trials are justified.

BACKGROUND

In Europe, hospital provision for mentally ill people dates back over eight hundred years. These were frequently overcrowded and included provision for the 'poor'. The distinction between 'pauper' and 'lunatic' only began to be recognised by the eighteenth century. At the turn of the nineteenth century growing public concern about mental illness led to greater provision of asylums and restrictive custodial care. This was off-set, however, by a liberal movement called 'Moral Treatment', pioneered in France (Pinel 1806) and England (Tuke 1813). This approach favoured releasing people who were physically restrained into bigger asylums (Connolly 1856) and introducing social forms of treatment based on human respect. By the end of the nineteenth century, this policy was reversed in favour of restrictive custodial approaches due to staff shortage, over-crowding and concern about safety. In the 1930's the pendulum swung again with the introduction of electroconvulsive therapy and insulin coma therapy. Optimism in the effectiveness of new treatments paralleled a move to more liberal care policies. Social attitudes continued to change after World War II, particularly with the introduction of anti-psychotic medication in 1952 (chlorpromazine and later drugs) making rehabilitation feasible even for those with serious mental illnesses. At the same time the large asylums were criticised for being 'total institutions' (Goffman 1961), inhumane and repressive (Wing 1970).

Since the 1960s, in North America and Europe, large hospitals have been closed and small local general hospital units established. This has been a gradual process apart from in Italy where public mental hospitals were rapidly closed and admissions to asylums prohibited (Jones 1985). At the same time many types of approaches to community care have evolved. For example, case management, a widely used community care regime, which is now a statutory obligation in several countries. Current mental health policies and guidance in the UK encourage short hospital stay with follow up by community programmes such as case management and the Care Programme Approach (CPA) (DOH 1999). Community care,

however, has come under criticism for its failure to provide adequate care (DoH 1994), particularly for mentally ill 'revolving door patients' (people who have repeated, frequent admissions - (Glick 1975), and 'new long stay patients' (such as people with both mental illness and behaviour problems - Todd 1976).

In many 'Western' countries, whilst bed numbers have declined, admission rates have risen (Anonymous 1996), perhaps as a direct result of community care policy (Marshall 1999). Frequent short admissions are a common pattern of hospital care for people with serious mental illness. Other countries have had quite different experiences. One of the performance indicators in the National Service Framework for Mental Health in the UK is the reduction in the psychiatric emergency readmission rate (DOH 1999).

Japan has increased bed provision in the last 30 years (Hafner 1987) and many 'developing' countries provide either non-specialist 'standard' medical care or continue to use institutions unchanged for decades (Appleby 1991). Most countries, however, are under pressure to change their bed provision and the effect of hospitalisation remains important to all. A degree of hospital care is likely to remain an integral part of any mental health service. Community care policies may change the pattern of hospital care for people with severe mental illness, favouring shorter admissions. It is therefore important to determine the effects of different lengths of hospital stay. This will help inform policy-makers in developing safe, appropriate, efficient and effective mental health services in the future.

OBJECTIVES

To evaluate the effect of 'short stay'/'brief admission' hospital care with 'long stay'/'standard' in-patient care in people with serious mental illness.

In addition, we also aimed to test the following hypotheses via sensitivity analyses:

1. Findings from well conducted randomised trials are, to an important extent, different findings from (a) trials that do not use rigorous randomisation techniques (where allocation of intervention is poorly concealed); and (b) trials that included unselected acute psychiatric admissions as opposed to only those with 'severe' mental illness.
2. Those over 65 years old have a differential response to length of hospital stay than younger people;
3. Those given a short-stay in hospital defined as less than 28 days (an arbitrary compulsory length of stay as defined by the Mental Health Act 1983 of England and Wales) have differential response to those in hospital greater than 28 days; and that
4. Findings from trials conducted in the presence of community care programmes (however defined) have a differential response to studies conducted in the absence of community care programmes.

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METHODS

Criteria for considering studies for this review

Types of studies—We included all randomised trials. Quasi-randomised trials (those that have employed alternating allocation, allocation by letter of the alphabet or day of the week) were also identified but excluded from main analysis. We used data from these in a sensitivity analysis in order to see if the exclusions were justified.

Types of participants—We included people with schizophrenia, related disorders or 'severe/chronic mental disorders/illnesses', however defined.

Types of interventions—

1. Planned short stay/brief admission - however defined within the studies.
2. Long stay or standard care - however defined within the studies. A hypothesis relating to these definitions will, however, be tested (please see Objectives).

Types of outcome measures—We grouped outcomes into (less than three months) (up to six months), (up to one year), and (two years or more from admission date).

Primary outcomes:

1 Global state

1.1 No clinically important change in global state (as defined by individual studies)

Secondary outcomes:

1 Death (suicides and all-causes)

2 Global state

2.1 Relapse (as defined by the individual studies)

3 Mental state

3.1 No clinically important change in general mental state score

3.2 Average endpoint general mental state score

3.3 Average change in general mental state score

3.4 No clinically important change in specific symptoms (positive symptoms of schizophrenia, negative symptoms of schizophrenia)

3.5 Average endpoint specific symptom score

3.6 Average change in specific symptom score

4 Leaving the studies early (any reason, adverse events, inefficacy of treatment)

5 Service outcomes

5.1 Readmission to hospital

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5.2 Leaving hospital prematurely

5.3 Discharge delayed beyond the time planned

5.4 Community care

6 Behaviour

6.1 Violent incidents (self, others, property)

6.2 Social functioning

7 User satisfaction

8 Quality of life

8.1 No clinically important change in general quality of life

8.2 Average endpoint general quality of life score

8.3 Average change in general quality of life score

9 Self esteem/psychological well-being

10 Family burden

11 General functioning

11.1 imprisonment

11.2 employment status

11.3 independent living

12 Economic outcomes

12.1 total cost of care

12.2 total health cost

Search methods for identification of studies

Electronic searches—1 Update search

We searched the Cochrane Schizophrenia Group Trials Register (July 2007) using the phrase:

[((short* or brief* or length*) in same field as (admission* or hospital*) in REFERENCE and (*hospitali*) in STUDY]

This register is compiled by systematic searches of major databases, hand searches and conference proceedings (see Group Module).

1.2 Previous electronic searches

1.2.1 We searched The Cochrane Schizophrenia Group's Trials Register (June 2005) using the phrase:

[((short* or brief* or length*) in same field as (admission* or hospital*) in REFERENCE and (*hospitali*) in STUDY]

1.2.2 We searched Cochrane Schizophrenia Group's Register (December 1998) using the phrase:

[and ((short or brief) near (admission* or hospitali$ation*) or # 42=114 or 327)]

#42 is the 'intervention' field of this register and '114 or 327' is the code for length of hospital stay.

1.2.3 We searched Biological Abstracts (January 1982 to May 1995) using the Cochrane Schizophrenia Group's phrase for both randomised controlled trials and schizophrenia combined with the phrase:

[and ((short or brief) near (admission* or hospitali$ation*))]

1.2.4 We searched EMBASE (January 1980 to May 1998) using the Cochrane Schizophrenia Group's phrase for both randomised controlled trials and schizophrenia combined with the phrase:

[and ((short or brief) near (admission* or hospitali$ation*))]

1.2.5 We searched MEDLINE (January 1966 to May 1998) using the Cochrane Schizophrenia Group's phrase for both randomised controlled trials and schizophrenia combined with the phrase:

[and ((short or brief) near (admission* or hospitali$ation*))]

1.2.6 We searched PsycLIT (January 1974 to May 1995) using the Cochrane Schizophrenia Group's phrase for both randomised controlled trials and schizophrenia combined with the phrase:

[and ((short or brief) near (admission* or hospitali$ation*))]

Searching other resources—

1. Reference searching

   We inspected the references of all identified studies for more studies.

2. SCISEARCH

   We sought each of the included studies as a citation on the SCISEARCH (May 1998) database. Reports of articles that had cited these studies were inspected in order to identify further trials.

3. Personal contact

   We sought the results from unpublished trials from authors of key studies. We contacted authors of published studies to request original data if appropriate or to seek clarifications.

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Data collection and analysis

[For definitions of terms used in this, and other sections, please refer to the Glossary]

1. Selection of trials—PJ and GZ undertook the original search for trials, and independently inspected all reports. Any disagreements were resolved by discussion, and where doubt remained, we acquired the full article for further inspection. Once the full articles were obtained, we independently decided whether the studies met the review criteria. If disagreement could not be resolved by discussion, again we sought further information and added these trials to the list of those awaiting assessment. For the updated version of the review, material downloaded from electronic resources included details of author, institution or journal of publication. NA inspected and selected all reports which were then reinspected by PJ to ensure reliable selection. We obtained full articles of the selected abstracts and independently decided whether the studies met the review criteria. If disagreement could not be resolved by discussion, we sought further information and added these trials to the list of those awaiting assessment.

2. Quality assessment—We assessed the methodological quality of included studies using the criteria described in the Cochrane Handbook (Higgins 2006), which is based on the degree of allocation concealment. Poor concealment has been associated with overestimation of treatment effect (Schulz 1995). Category A includes studies in which allocation has been randomised and concealment is explicit. Category B studies are those which have randomised allocation but in which concealment is not explicit. Category C studies are those in which allocation has neither been randomised nor concealed. We only included trials that were stated to be randomised (categories A or B of the handbook) in this review. The categories are defined below:

- **A.** Low risk of bias (adequate allocation concealment)
- **B.** Moderate risk of bias (some doubt about the results)
- **C.** High risk of bias (inadequate allocation concealment).

3. Data extraction—We independently extracted data from selected trials. When disputes arose we attempted to resolve these by discussion. When this was not possible and further information was necessary to resolve the dilemma, we did not enter data and added the trial to the list of those awaiting assessment.

4. Data management

4.1 Loss to follow up (intention-to-treat/ITT analysis): We excluded data from studies where more than 50% of participants in any group were lost to follow up (this did not include the outcome of 'leaving the study early'). In studies with less than 50% dropout rate, we assumed people leaving early had a negative outcome, except for the event of death. We analysed the impact of including studies with high attrition rates (25-50%) in a sensitivity analysis. If inclusion of data from this latter group resulted in a substantive change in the estimate of effect, we did not add their data to trials with less attrition, but presented them separately.

4.2 Dichotomous data: For binary outcomes we calculated the relative risk (RR) and its 95% confidence interval (CI) based on the fixed effects model. Relative Risk is more intuitive (Boissel 1999) than odds ratios and odds ratios tend to be interpreted as RR by clinicians (Deeks 2000). This misinterpretation leads to an overestimate of the impression of the effect. When the overall results were significant and homogeneous we calculated the number needed to treat/harm (NNT/H).

4.3 Continuous data

4.3.1 Normal distribution: Continuous data on outcomes in trials relevant to mental health issues are often not normally distributed. To avoid the pitfall of applying parametric tests to non-parametric data we applied the following standards to continuous final value endpoint data before inclusion: (a) standard deviations and means were reported in the paper or were obtainable from the authors; (b) when a scale started from zero, the standard deviation, when multiplied by two, should be less than the mean (otherwise the mean is unlikely to be an appropriate measure of the centre of the distribution - Altman 1996); In cases with data that are greater than the mean they were entered into 'Other data' table as skewed data. If a scale starts from a positive value (such as PANSS, which can have values from 30 to 210) the calculation described above in (b) should be modified to take the scale starting point into account. In these cases skewness is present if 2SD>(S-Smin), where S is the mean score and Smin is the minimum score. We reported non-normally distributed data (skewed) in the 'other data types' tables.

For change data (mean change from baseline on a rating scale) it is impossible to tell whether data are non-normally distributed (skewed) or not, unless individual patient data are available. After consulting the ALLSTAT electronic statistics mailing list, we entered change data in RevMan analyses and reported the finding in the text to summarise available information. In doing this, we assumed either that data were not skewed or that the analysis could cope with the unknown degree of skew.

4.3.2 Final endpoint value versus change data: Where both final endpoint data and change data were available for the same outcome category, we only presented final endpoint data. We acknowledge that by doing this much of the published change data may be excluded, but argue that endpoint data is more clinically relevant and that if change data were to be presented along with endpoint data, it would be given undeserved equal prominence. Where studies reported only change data we contacted authors for endpoint figures.

4.3.3 Data synthesis: For continuous outcomes we estimated a weighted mean difference (WMD) between groups based on a fixed effects model.

4.4 Scale derived data: A wide range of scales/instruments are available to measure outcomes in psychiatric care. These instruments vary greatly in quality. Instruments should be reliable (have a known degree of stability when a measurement is repeated under identical conditions) and valid (really measure what it actually purports to measure (Rust 1989). In this review we only used outcomes measured by instruments which have been published in peer reviewed journals. In addition, data from rating scales were only used if it

was (i) self-reported or completed by an independent rater or relative; and (ii) more than 50% complete.

4.5 Cluster trials: Studies increasingly employ cluster randomisation (such as randomisation by clinician or practice) but analysis and pooling of clustered data poses problems. Firstly, authors often fail to account for intra class correlation in clustered studies, leading to a unit-of-analysis error (Divine 1992) whereby p values are spuriously low, confidence intervals unduly narrow and statistical significance overestimated. This causes Type I errors (Bland 1997, Gulliford 1999).

Where clustering was not accounted for in primary studies, we presented the data in a table, with a (*) symbol to indicate the presence of a probable unit of analysis error. In subsequent versions of this review we will seek to contact first authors of studies to obtain intra-class correlation co-efficients of their clustered data and to adjust for this using accepted methods (Gulliford 1999). Where clustering has been incorporated into the analysis of primary studies, we will also present these data as if from a non-cluster randomised study, but adjusted for the clustering effect.

We have sought statistical advice and have been advised that the binary data as presented in a report should be divided by a design effect. This is calculated using the mean number of participants per cluster (m) and the intraclass correlation co-efficient (ICC) [Design effect=1+(m-1)*ICC] (Donner 2002). If the ICC was not reported it was assumed to be 0.1 (Ukoumunne 1999). If cluster studies had been appropriately analysed taking into account intraclass correlation coefficients and relevant data documented in the report, we synthesised these with other studies using the generic inverse variance technique.

5. Investigation for heterogeneity—Firstly, we considered all the included studies within any comparison to judge for clinical heterogeneity. Then we visually inspected graphs to investigate the possibility of statistical heterogeneity. This was supplemented using, primarily, the I-squared statistic. This provides an estimate of the percentage of variability due to heterogeneity rather than chance alone. Where the I-squared estimate was greater than or equal to 75%, this was interpreted as indicating the presence of high levels of heterogeneity, and when it was between 50-75% it was interpreted as indicating moderate levels of heterogeneity (Higgins 2003). If inconsistency were thought to be high, data were not summated and presented as part of the main results, but were presented separately and a sensitivity analysis performed. Reasons for heterogeneity were investigated whenever possible.

6. Sensitivity analysis—We excluded data from quasi-randomised trials and from trials including unselected psychiatric admissions from the main analysis and these were entered into a sensitivity analysis to see if these exclusions were justified.

7. General issues—Where possible, we entered data into RevMan in such a way that the area to the left of the ’line of no effect’ indicated a ’favourable’ outcome for the treatment group. Where this was not possible, we labelled the graphs in RevMan analyses accordingly so that the direction of any effects was clear.

RESULTS

Description of studies

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

1. Excluded studies—Sixteen studies were identified of which we excluded ten because they did not meet the inclusion criteria (see list of excluded studies). One study, Kennedy 1980, included participants from unselected acute psychiatric admissions and we therefore analysed this trial in a separate sensitivity analysis.

2. Awaiting assessment—No studies await assessment.

3. Ongoing studies—We are not aware of any ongoing studies.

4. Included studies—Four trials, all published in the 1970s, were stated to be randomised controlled trials where patients were allocated admission (Glick 1975, Glick 1976, Herz 1975 and Hirsch 1979). Although Glick 1975 used randomisation, important differences were found between the groups on important variables (education, socio-economic status, pre-morbid adjustment, mean dosage of chlorpromazine equivalent) which favoured the long stay group. These differences may well have occurred by chance but it is difficult to assess the degree of confounding that this introduces. Kennedy 1980 allocated participants randomly, although blinding was not reported. One study, Burhan 1969, met the inclusion criteria but used unusual methods both in its design and conduct. The results of this study, where 100 patients from a hospital cohort of over 1000 were 'randomly selected' for a short stay package added heterogeneity to the pooled results. We therefore presented the data from this trial and discussed the data separately. For the 2007 update we did not find any additional studies that we could include.

4.1 Length of studies: The study duration of Hirsch 1979 and Kennedy 1980 was one year. Burhan 1969, Glick 1975 and Herz 1975 evaluated participants for two years, and Glick 1976 was the longest study lasting just over two years (26 months).

4.2 Setting: Four trials were undertaken in the US and two (Hirsch 1979,Kennedy 1980) in the UK.

4.3 Participants: All trial participants were 'seriously mentally ill' with psychiatric disorders such as schizophrenia, affective disorders and severe personality disorders. All trials focused on adults, excluding children, adolescents, the elderly, and those with learning disabilities, organic brain disease, drug and alcohol abuse.

4.4 Study size: Burhan 1969 was the largest study with 1169 participants randomised and Glick 1976 was the smallest study with 74 participants. The other studies ranged in size between 141 and 247 participants.

4.5 Interventions: In three studies, short stay varied from one week (Herz 1975) to 21 to 28 days (Glick 1975, Glick 1976). Those allocated to short stay received other treatments such

as discharge planning and crisis resolution training (Glick 1975, Glick 1976). Kennedy 1980 reported allocating participants to either short stay or control without defining the length of short stay. No specific 'community-based' interventions were reported except for Hirsch 1979 (day care). The effects of the presence or absence of these programmes are discussed.

Two trials clearly reported the minimum and maximum duration of long stay before the trial (Glick 1975, Glick 1976). Otherwise professional carers determined length of stay. Two trials specified a cut-off for long stays (at 45 days, Hirsch 1979; 60 days, Herz 1975). No specific intervention was reported for those allocated to the long stay group after discharge. Antipsychotic drugs were the main treatment for participants and most trials reported similar use in both long and short stay participants.

4.6 Outcomes: Data relating to readmissions (not relapse), loss to follow-up, premature discharge, delayed discharge, and employment were possible to extract. Deaths (suicides and all-causes) were noted in three trials but attributed to experimental or control groups in only one (Herz 1975). No data were reported on relapse, criminal behaviour or imprisonment. Trialists used many different scales but all were reported without any reference to standard deviations, and therefore could not be summated. Only one paper presented data from a continuous measure that could be extracted in dichotomous (percentage of people improved (Glick 1975).

4.6.1 Continuous data: Details of the scales that supplied usable data for this review are shown below. Reasons for exclusion of data from other instruments are given under 'outcomes' in the 'included studies' table.

4.6.1.1.1 Health-Sickness rating scale - HSRS (Luborsky 1962): This is a global rating of psychiatric functioning. Ratings are on a scale of zero, (severely disabled), to 100 (very effective functioning). Glick 1975 reported data from this scale.

4.6.1.1.2 Psychiatric Evaluation Form - PEF (Endicott 1972): A clinician-rated scale used to assess psychological functioning during the week prior to interview. This consists of 24 individual and eight summary scales. Scoring on each scale ranges from 1-5 with higher scores indicating greater impairment. Glick 1975 reported data from this scale.

Risk of bias in included studies

1. Randomisation—No trials made fully explicit the means by which randomisation took place (Grade B, Higgins 2006). All studies, however, reported random allocation or, in the case of Burhan 1969, random selection from a large sample. Although this latter technique is unusual, it does not invalidate the study.

Two excluded studies used quasi-randomisation techniques. Caffey 1968, assigned in rotation to one of three groups, and Rosen 1976, 'assigned on a first come first served basis according to date of application and availability of beds'.

2. Blinding—No trial mentioned blinding of observers.

3. Loss to follow up—One trial, Burhan 1969, included all randomised people, that is, undertook an intention-to-treat analysis. The remaining trials reported exclusions in their analyses. Glick 1975 reported 4.5% exclusion at two years, Glick 1976 11% at two years, Herz 1975 30% at two years and Hirsch 1979 53% at one year. In this latter study only data from two outcomes were used (readmissions and lost to follow-up and one year) as intention-to-treat numbers could not be calculated.

The limited reporting of randomisation, lack of blindness for these outcomes and unclear reasons for loss to follow up would suggest that all estimates of effect are prone to bias (Moher 1998).

Effects of interventions

1. The search—From the initial electronic search we identified 206 citations. Thirty-four were ordered and assessed against the inclusion criteria. Of these, five randomised controlled trials were included in the main analysis. For the 2005 update we found 306 citations. Seven were assessed against inclusion criteria. Of these, we excluded one randomised controlled trial from the main results, and entered the study into a sensitivity analysis to determine if the exclusion is justified. For the 2007 update we did not find any additional studies to include.

2. COMPARISON: SHORT versus LONG HOSPITAL STAY

2.1 Death (all causes): We found no significant difference in the number of reported deaths (3/112 deaths in the short stay, and 4/63 in the long stay group) (Herz 1975, n=175, RR 0.42, CI 0.1 to 1.8).

2.2 Mental state: Only one trial (Glick 1975) reported percentages of people 'not improved'. We found no significant difference between short and long stay groups as measured by two different scales. These outcomes were presented only in the preliminary report of the study, and are a subset of a larger trial.

2.3 Service outcomes

2.3.1 Readmissions: All trials reported readmission data. We found no significant difference between short and long stay groups by one year (n=651, 4 RCTs, RR 1.26 CI 1.0 to 1.6), and by two years (n=229, 2 RCTs, RR 1.03 CI 0.8 to 1.4). Adding Burhan 1969, which reported very significantly fewer readmissions for those in the short stay group throughout the two-year period, introduced heterogeneity (I^2 = 71.7% at one year and 92.7% at 2 years). As this study had always been unusual in its methods and interventions we removed these results from the others and presented and discuss them separately (n=1169, RR 0.22 CI 0.1 to 0.7 at one year, and n=1169, RR 0.21 CI 0.1 to 0.4 at two years).

Adding Kennedy 1980, which is a trial that randomised all (unselected) acute psychiatric admissions, introduced heterogeneity (I^2 = 62.4% at one year), and produced an opposite trend to Burhan 1969. The short stay group had a higher number of readmissions compared to the standard stay group. The results have not been summated we have presented these separately (RR 2.23 CI 1.3 to 3.7 at 1 year). The differences were statistically significant

between the two groups. Although the readmission rates in the experimental wards were twice as high as the control wards; the average duration of a readmission to the experimental wards were only a third as long as the average readmission to the control wards. We found the average duration of stay in short stay wards were shorter for both first readmissions and all admissions by one year.

No significant differences were found between the two groups in the number of admissions to hospital because of a parasuicidal act by one year (Kennedy 1980, n=246, RR 1.05 CI 0.4 to 3.0).

2.3.2 Length of stay: Apart from Kennedy 1980, there were no standard deviations reported for average length of stay and we were unable to summate the data. For those allocated to short stays in hospital, the average length of stay ranged from 10.8 days (Herz 1975; Kennedy 1980) to 25.0 days (Glick 1975) and the long stay averages ranged from 28 days (Hirsch 1979) to 94 days (Glick 1975). The standard deviation in Kennedy 1980 is more than the mean so it did not meet the inclusion criteria. However we have reported the data in the other data tables section.

2.3.3 Premature discharge from hospital: Two trials reported abrupt, premature, discharge, against medical advice (Glick 1975, Glick 1976), and we found no significant difference between groups (n=229, RR 0.77 CI 0.3 to 1.8).

2.3.4 Delayed discharge from hospital: There were significantly fewer delayed discharges in the short stay group compared with those in long stay (n=404, 3 RCTs, RR 0.54 CI 0.3 to 0.9). Including data from quasi-randomised trials reduced this to no effect, and introduced significant heterogeneity (Chi-square 27.45, df 4, p<0.001).

2.3.5 Day care: We found significantly more post-discharge day care given to participants in the short stay group than those in the standard stay group (Kennedy 1980, n=247, RR 4.52 CI 2.7 to 7.5, NNH 3 CI 2 to 6).

2.4 Loss to follow-up: We found no significant difference in loss to follow-up between short or long stay groups at one year (n=453, 3 RCTs, RR 0.87 CI 0.7 to 1.1) and two years (n=404, 3 RCTs, RR 1.07 CI 0.7 to 1.6). At one year, just over 5% of people in both groups were lost to follow-up rising to 14% by two years. Six month data by Herz 1975 were equivocal.

2.5 Social functioning: Unemployed, unable to housekeep or unknown employment status - by 2 years People from the short stay groups were more likely to be employed at two years than those allocated to long stays (n=330, 2 RCTs, RR 0.61 CI 0.5 to 0.8, NNT 5 CI 4 to 8).

2.6 Cost of care: Only one study (Glick 1975) reported costs for outpatient services, but data are skewed (wide confidence intervals), although the data suggested that short stay care is slightly more expensive overall.

3. Sensitivity analysis—Combining the results from Kennedy 1980 for readmissions at one year increased the relative risk to 1.26 (95% CI 1.0 to 1.6). This introduced heterogeneity (I^2=62.4%)

DISCUSSION

1. Applicability of findings

The review utilised data from six trials reported from 1969 to 1980. Four came from the US and two from the UK. It is difficult to know whether the results would be equally applicable to the psychiatry of the low and middle income countries. In addition, the American definition of schizophrenia has been shown to be broader than other countries during this period (WHO 1973, Cooper 1972) and psychiatric services differ between the US and the UK. This may have yet further implications for generalisability of the results of this review. However, the fact that the findings of trials within these two very different care cultures are not substantially different suggests that the results of this review can, at least, be considered for those who work within diverse settings.

The fact that all these trials were mainly published in the 1970s may be the result of a window of opportunity to conduct studies of this sort before the large institutions closed in these countries. These trials, therefore, offer unique information about health policy that would be difficult to repeat today in high income countries where large institutions have closed. If questions remain unanswered as regards the effect of a short stay versus a longer stay policy, it may be for countries other than the US and the UK to help supply the answers.

None of the included trials had participants aged 65 years and over, therefore we were unable to test the hypothesis that this age group may have a differential response to younger age groups in this review.

2. Missing outcomes

All trials failed to report continuous data in a way that is useful to reviewers. These included outcomes for mental state, social functioning and family burden. Fourteen different scales, some of unknown validity were used. No trial reported user satisfaction, perhaps because the measurement of the consumers' views were not considered important in the 1960s and 70s. Questions regarding the effect of a short stay policy on these important variables remain unanswered.

3. COMPARISON: SHORT versus LONG HOSPITAL STAY

3.1 Death and violence—Deaths were very poorly recorded with the exception of Herz 1975. Further information on death rates was sought from trialists, but we were unable to obtain additional information. Not one trial mentioned violence, criminal offence or imprisonment as an outcome.

3.2 Mental state—All continuous data on mental outcomes could not be synthesised as standard deviations were not reported. One small trial, Glick 1975, did record relevant, but equivocal data. This came from a subset (N=61) of the full study (N=141). Data reported in

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their final report of the study could not be extracted. It is, therefore, inadvisable to draw firm conclusions from so little data but currently there is no evidence that shorter lengths of stay are harmful to a person's mental state.

3.3 Service outcomes

3.3.1 Readmissions to hospital: There were no significant differences for readmission rates between the short and long stay groups at one and two years with the homogeneous studies (Glick 1975; Glick 1976; Herz 1975). There is no evidence to support the theory that short stay policies, in themselves, promote a 'revolving door' pattern of admissions for those with serious mental illnesses. The unusual Burhan 1969 study showed how a remarkable degree of support for those allocated to a short stay policy may well substantially decrease the numbers readmitted. It is thought that this support, involving the author providing sole aftercare including daily visits, counselling and 24 hours personal access via a telephone was the reason for the introduction of heterogeneity. Lessons should be drawn for practice and research from this unusual trial, providing remarkable planning and supervision for short stay admissions. The other studies did not offer this degree of support.

Only Kennedy 1980 reported data for the outcome of readmission because of a parasuicidal act and being allocated to an initial short stay did not increase the likelihood of being admitted to hospital for a suicide attempt. Again, this is reassuring.

3.3.2 Delayed discharge/Leaving hospital prematurely: Hospitals were significantly more successful in achieving discharge on time for those allocated to short stay. However, those in the short stay policy groups were no more likely to have a premature/abrupt discharge than the long stay group. The theories of institutionalisation (Goffman 1961) could explain successful discharge of short stay patients, suggesting that longer hospitalisation leads to difficulties for patients to re-enter the real world. Planning discharge and after-care planning may also be more likely in the short-stay group; the impetus to institute these processes might not be present when there are no restrictions to the length of the admission. Also, from the consumer perspective, knowing that the admission will be short may improve engagement in discharge planning.

3.3.3 Day care: We found those people in the short stay group were prescribed significantly more day care in the Kennedy 1980 study, although no data were available from this study for readmission rates. It is not clear from other studies that this is a common outcome of a short stay policy. This may partly explain the findings in Glick 1975 where the post-discharge cost of the short stay policy is really no different, and may be more, than the standard approach to hospitalisation.

3.4 Loss to follow-up—The results suggest that, over two years, short stay patients are not significantly at greater risk to being lost to follow-up than those allocated to long stays.

3.5 Social functioning—Although data does suggest higher rates of employment/ independent living for those allocated to short stay policies, these should be interpreted with caution. The trial that provided greater after-care for the short stay group (Herz 1975) showed greatest effect on employment status. This important finding should be replicated.

3.6 Costs of care—Economic data were very poor and difficult to interpret. No study reported indirect costs (that is travel, family costs, etc.) and intangible costs (such as inconvenience). Once a person is discharged Glick 1975 suggests that there is really not much difference between the two groups, and if anything the short-stay group cost more once they had left hospital. This could, perhaps be explained if it was common to use more day hospital for these people compared with those whose hospital stay was longer as was seen in Kennedy 1980. However, should the mean length of stay be used as a measure of resources consumed, the long stay average costs would be much more than the short stay.

3.7 Sensitivity analysis—Adding Kennedy 1980 introduced moderate heterogeneity to the outcome of readmission rate at one year. Including this study increased the relative risk of being readmitted to hospital if in the short stay group at one year to 1.26 (95% CI 1.0 to 11.6). This study is considerably different from the other included studies in its type of participants. It randomised all acute, unselected psychiatric admissions including people with organic brain disorders and alcohol problems.

AUTHORS' CONCLUSIONS

Implications for practice

1. For people with serious mental illness and their carers—Data from this review can partially reassure those coming into a hospital that has a short stay policy (of less than 28 days) that they are no more likely to be readmitted, to leave hospital abruptly, or to lose contact with services after leaving hospital than had they received long stay care. They are also more likely to leave hospital on their planned discharge date and possibly have a greater chance of finding employment.

2. For clinicians—For clinicians concerned about the uncertainty and safety of short stay policy there is some reassurance that it does not promote a 'revolving door' pattern of admissions and possible fragmentation of care.

3. For policy makers and commissioners of care—Length-of-stay policies have a direct relationship to the size and provision of in-patient facilities. These, in turn, have a major impact on how resources are used. Traditionally, planners assess levels of in-patient provision based on national and international comparisons, rather than on the effectiveness of short versus long stay policies. This review attempts to address this and, based on limited data so far, commissioning short stay policies appear to be an appropriate use of resource. It also indirectly supports the commissioning of services where discharge and after-care planning is a priority.

Implications for research

1. General—The studies we identified were pioneering and important. If their findings had been more clearly reported, as is now recommended by the CONSORT statements (Begg 1996, Moher 2001) this review would have had more findings to report. Continuous data should be presented with standard deviations.

2. Specific—Further trials are needed in order to fill important gaps in knowledge, strengthen existing evidence, and allow greater generalisability to other care cultures. Trials should be large, simple, and clearly reported.

These trials should address questions regarding the processes of discharge and aftercare planning. Criteria for entry could be broad, not focusing on 'perfectly' defined single disease categories, but well described, so that readers could extrapolate results for their own circumstances. This also applies to the interventions. These too can be pragmatic but well described. Simple outcomes should also be reported. For example, death, self-harm, harm to others, criminal behaviour, employment, and homelessness are not difficult to record. Mental, social and family outcomes, user satisfaction, and costs may be more problematic, but can often be recorded clearly in order to inform a wide readership (Table 1).

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External sources

- No sources of support supplied

APPENDIX

CHARACTERISTICS OF STUDIES

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

Burhan 1969

Methods	Allocation: 'randomly selected from total hospital intake by admissions office' - no further details. Blinding: none. Duration: 2 years. Setting: Ohio USA.	
Participants	Diagnosis: 'mixture of psychotic and psychoneurotic'. N=1169. Age: adults, no further details. Sex: unknown. Excluded: 'geriatric' and 'mentally retarded' people.	
Interventions	**1**	Short stay (2-3 weeks): informed re short stay on admission, daily psychiatrist visit (5 days), counselling both recipient & family at discharge, telephone access to psychiatrist thereafter. N=100
	2	Long stay: standard care. N=1069.

	All participants had medication, and out patient care as required
Outcomes	Readmission. Unable to use - Mental state: (Hoffer-Osmond diagnostic test, MMPI (no SD). Length of stay. no SD. Employment. no data. Economic data. no SD.
Notes	This study adds heterogeneity and the results are presented separately

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Glick 1975

Methods	Allocation: 'random allocation', no further details. Blinding: none. Duration: 2 years. Setting: San Francisco, USA.
Participants	Diagnosis: schizophrenia (57% paranoid) (Mosher criteria). N=141. Age: mean 23, range 15-38 years. Sex: 50% women. Marital status: 84% single. Race: 10% black. Social class: 85% III -V. History: 'short-term' 31; 'long-term' 29.
Interventions	**1** Short term admission (21-28 days): early discharge plan, rapid assessment, "crisis resolution" management ethos, phenothiazine medication; long-range recommendations for further post-discharge treatment or rehabilitation. N=71 **2** Long-term admission (90-120 days): assessment (2-3 weeks), psychotherapy, phenothiazine medication and/or "major rehabilitative measures" (change of home/job or workshop placement). N=70 Similar fixed drug regimes across groups.
Outcomes	Readmission. Average length of hospital stay. Delay in discharge. Leaving hospital prematurely. Lost to follow-up. Mental state. HSRS, PEF. Unemployed. Economic data. Unable to use - Social function: KAS, Behavior Inventory (no SD). User satisfaction: PSECS (no SD). Family burden: PEF (no SD). Employment status: BFR, PEF, PSECS , FMECS (no SD).
Notes	Mosher criteria - see Mosher 1971. Differences reported between groups (long stay - more education, higher socio-economic status, better pre-morbid adjustment & higher mean dose antipsychotics - 644 mg CPZ equivalents versus 328 mg). First report on subset of 61 people (Glick 1974) presented only usable data from PEF & HSRS (percentage not improved)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Glick 1976

Methods	Allocation: 'randomly assigned' - no further details. Blindness: not reported. Duration: 26 months. Setting: USA.
Participants	Diagnosis: 'non-schizophrenics' (DSM-II) included affective disorders, neuroses and severe personality disorders. N=74. Age: not stated. Excluded: alcohol and drug dependencies, organic disease.
Interventions	**1** Short term (21-28 days): emphasis on 'crisis resolution' & discharge planning. N=37 **2** Long term (90-120 days): emphasis on diagnosis, treatment, psycho-social intervention, rehabilitation, & referral to post hospital care. N=37 Both groups received individual, family & group psycho-therapy
Outcomes	Readmission. Leaving hospital prematurely. Loss to follow-up. Unable to use - Social function: KAS, Behavior Inventory (no SD). User satisfaction: PSECS (no SD). Family burden: PEF (no SD). Employment status: BFR, PEF, PSECS, FMECS (no SD).
Notes	No difference between diagnostic categories between groups. No specific community programme.

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Herz 1975

Methods	Allocation: 'randomly assigned' - no further details. Blinding: none. Duration: 2 years. Setting: New York, USA.
Participants	Diagnosis: ~60% schizophrenia. N=175. Race: 'ethically diverse'. Social class: 'low income'. Excluded: those < 16 years; not living with responsible adult; with concurrent serious medical illness; substance misuse; organic brain syndrome; anti-social behaviour, and/or adolescent behaviour disorder
Interventions	**1** Brief-day: planned discharge by 7 days, then day care & OPD when necessary. N=61 **2** Brief-out: planned discharge & OPD when necessary. N=51. **3** Standard treatment: length of stay determined by carers, OPD when necessary Maximum length of stay to first significant release was 60 days. N=63
Outcomes	Readmission. Average length of hospital stay. Delayed discharge. Unemployed. Lost to follow-up. Unable to use -

	Global functioning: GAS (no SD). Mental state: MSER, PSS (no SD). Family burden: FEF (no SD).
Notes	Both brief groups received less psychotropic medication.

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Hirsch 1979

Methods	Allocation: 'at random' - no further details. Blinding: none. Duration: 1 year. Setting: central London, UK.
Participants	Diagnosis: 'functional psychiatric disorder'. N=224. History: just admitted. Age: >16 years. Sex: brief group = 47% males, standard group = 40% males. Excluded: outside catchment, <16 years, organic brain syndrome
Interventions	**1** Brief admission: discharge planned in < 8 days + community day care. N=115 **2** Standard admission: discharged at carers discretion. N=109
Outcomes	Lost to follow-up. Unable to use - Readmission. (>50% loss to follow up). Mental state: PSE (no SD). Economic outcomes. (no data). Behaviour: PBAS (details not published).
Notes	

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Kennedy 1980

Methods	Allocation: "randomisation of patients was started" -no further details. Blinding: none. Duration: 1 year. Setting: Edinburgh, Scotland, UK.
Participants	Diagnosis: 'any psychiatric disorder: organic, schizophrenia, affective, alcoholic, neurotic and other' N= 247. Age: any. Sex: males=49%. Excluded: 1 in 4 from randomisation ' in interest of continuity of care'
Interventions	**1** Admission to short stay ward N=86. **2** Admission to one of two control wards N=161.
Outcomes	Readmission rates. Day care.

	para suicide admissions. Length of stay. Unable to use - Psychiatric Status Schedule. Family Evaluation Form. GP form. Work. Reported para suicide episodes. Antipsychotic drugs. ECT treatment.	
Notes		
Risk of bias		
Item	**Authors' judgement**	**Description**
Allocation concealment?	Unclear	B - Unclear

General

CPZ - chlorpromazine

VA - Veteran's Administration

Diagnostic tools

DSM-II - Diagnostic Statistical Manual version 2.

Scales

1. Global functioning

GAS - Global Assessment Score.

2. Mental state measures

MMPI - Minnesota Multiphasic Personality Inventory

MSER - Mental State Examination Record.

HSRS - Health-Sickness rating scale.

PSS - Psychiatric Status Schedule.

PSE - Present State Examination

3. Social function/behaviour scales

KAS - Katz Adjustment Score

PBAS - Patient Behaviour Assessment Scale

4. Satisfaction scales

FEF - Family Evaluation Form.

PSECS - Patient self evaluation of current status

5. Employment status

BFR - Unknown.

PEF - Psychiatric Evaluation Form

FMECS (work) - Family Evaluation of Current Status

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

Study	Reason for exclusion
Appleby 1993	Allocation: not randomised - retrospective study.
Caffey 1968	Allocation: quasi randomised 'assigned in rotation to one of three groups'. Participants: people with schizophrenia. Interventions: brief intensive care (21 day admission) versus standard hospitalisation (discharged at doctor's discretion) Quasi-randomised - data used in sensitivity analysis.

Hafner 1986	Allocation: not randomised - cohort study.
Lehrman 1961	Allocation: not randomised - cohort study.
May 1968	Allocation: randomised. Participants: people with schizophrenia. Intervention: individual psychotherapy versus ataraxic drugs versus individual psychotherapy plus ataraxic drugs versus ECT versus no extra treatment. Outcome: length of stay and other outcomes.
Mendel 1966	Allocation: not randomised - cohort study.
Olfson 1990	Allocation: not randomised, from emergency room by nurse and psychiatrist 'according to need and bed availability'. Intervention: very brief (crisis) in-patient stay (less than 5 days) versus short term stays (21 days)
Rosen 1976	Allocation: assigned on admission on a 'first come first served basis, by date of application and availability of beds' - quasi randomised. Participants: people with functional and organic brain syndromes. Interventions: short term admission (< 3 months) versus long term admission (clinical judgment) Quasi randomised - data used in sensitivity analysis.
Singer 1975	Allocation: not randomised - retrospective cohort.

DSM-III - Diagnostic Statistical Manual version 3.

DATA AND ANALYSES

Comparison 1
SHORT versus LONG HOSPITAL STAY

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Death - 2 years follow up	1	175	Risk Ratio (M-H, Fixed, 95% CI)	0.42 [0.10, 1.83]
2 Mental state: Not improved - by 2 years	1		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
2.1 as measured by PEF scale	1	61	Risk Ratio (M-H, Fixed, 95% CI)	3.39 [0.76, 15.02]
2.2 as measured by HSRS	1	61	Risk Ratio (M-H, Fixed, 95% CI)	0.97 [0.31, 3.01]
3 Service outcomes: 1a.	4		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
Readmission to hospital (homogeneous studies)				
3.1 by 3 months	1	175	Risk Ratio (M-H, Fixed, 95% CI)	1.25 [0.72, 2.16]
3.2 by 6 months	1	175	Risk Ratio (M-H, Fixed, 95% CI)	1.10 [0.69, 1.73]
3.3 by 1 year	4	651	Risk Ratio (M-H, Fixed, 95% CI)	1.26 [1.00, 1.57]
3.4 by 2 years	2	229	Risk Ratio (M-H, Fixed, 95% CI)	1.03 [0.78, 1.36]
4 Service outcomes: 1b.	1		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
Readmission to hospital - by 2 years - Burhan 1969 only				
4.1 by 1 year	1	1169	Risk Ratio (M-H, Fixed, 95% CI)	0.22 [0.07, 0.67]
4.2 by 2 years	1	1169	Risk Ratio (M-H, Fixed, 95% CI)	0.21 [0.11, 0.41]

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
5 Service outcomes: 1c. Readmission to hospital - due to parasuicide - by 1 year	1	246	Risk Ratio (M-H, Fixed, 95% CI)	1.05 [0.36, 3.04]
6 Service outcomes: 2. Average length of stay			Other data	No numeric data
7 Service outcomes: 3. Leaving hospital prematurely - by 2 years	2	229	Risk Ratio (M-H, Fixed, 95% CI)	0.77 [0.34, 1.77]
8 Service outcomes: 4. Discharge delayed beyond the time planned in study - 2 year data	3	404	Risk Ratio (M-H, Fixed, 95% CI)	0.54 [0.33, 0.88]
9 Service outcomes: 5. Day care - by 1 year	1	247	Risk Ratio (M-H, Fixed, 95% CI)	4.52 [2.74, 7.45]
10 Lost to follow-up	4		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
10.1 by 6 months	1	175	Risk Ratio (M-H, Fixed, 95% CI)	1.41 [0.72, 2.74]
10.2 by 1 year	3	453	Risk Ratio (M-H, Fixed, 95% CI)	0.87 [0.68, 1.11]
10.3 by 2 years	3	404	Risk Ratio (M-H, Fixed, 95% CI)	1.07 [0.70, 1.62]
11 Social functioning: Unemployed, unable to housekeep or unknown employment status - by 2 years	2	330	Risk Ratio (M-H, Fixed, 95% CI)	0.61 [0.50, 0.76]
12 Cost of care: Total costs			Other data	No numeric data

ADDITIONAL TABLES

Table 1
Suggested design of study

Type of study	Patients	Interventions		Outcomes	Notes
Allocation: the randomisation process should be clearly described. Double-blind evaluation of the outcomes of a lifestyle intervention is extremely difficult, and probably impossible. Trialists should, take every precaution to minimise the effect of biases by using blind or independent raters. Intention-to-treat analysis is preferable. Trialists should describe from which groups with-drawals came, why they occurred and what was their outcome. Duration: Two year follow-up at minimum. Setting: in a situation where hospitalisation of people from	Diagnosis: people with schizophrenia or schizophrenia-like illnesses. Age: all ages. Sex: men & women. N=300.* History: people needing admission.	**1** **2**	Short stay policy: discharge planning for before day 28. N=150. Standard stay: discharge planning as before. N= 150.	Service outcomes: read-mission, use of day hospital. Loss to follow up. Functioning: including employment. Serious events: any, list. Satisfaction. Quality of life. Economic outcomes.	* Size of study with sufficient power to highlight ~10% difference between groups for primary outcome

Type of study	Patients	Interventions	Outcomes	Notes
schizophrenia tends to extend well beyond 28 days - perhaps in a low-middle income country setting				

FEEDBACK

General

Summary

1. Category: Discussion: The authors state that brief admissions to a psychiatric hospital "do not encourage a 'revolving door' pattern of care for people with serious mental illness and may be more effective than standard care." Such a conclusion would be erroneous and, in an era of aggressive cost containment, dangerous. This review merely presents a meta-analysis of four old and very different studies, each comparing 'long' with 'short' hospital stays. All the studies were performed more than 20 years ago, before the adoption of current diagnostic criteria (Diagnostic and Statistical Manual of Mental Disorders, fourth edition, and International Classification of Diseases, ninth revision) and modern treatment methods, such as use of selective serotonin reuptake inhibitors and atypical antipsychotic drugs. Further, the prominent decrease in psychiatric facilities (notably the American state hospital system) has meant that many of the patients with chronic mental illness who were institutionalised two decades ago are now subject to repeated acute care admissions to general hospitals. In the two included studies by Glick et al, a short admission was defined as 21-28 days and a long admission as 90-120 days. Clearly, the adjectives short and long have since come to have very different meanings: a four-week hospital stay today would generally be considered to be long. The other two studies were of a mixed sample of patients with either exclusively schizophrenic or 'functional psychiatric' disorders that conceivably could encompass all personality, mood and psychotic conditions. Can one draw an informed conclusion from pooling such outdated and heterogeneous data? At best this meta-analysis presents a historical snapshot of distant relevance to today's world of inpatient psychiatry. Nevertheless, profit-driven managed care companies may interpret this paper as justifying a solution to mental health cost control through the imposition of inappropriate limits on inpatient care.

I certify that I have no affiliations with or involvement in any organisation or entity with a direct financial interest in the subject matter of my criticisms.

Reply

1. Category: Discussion—The commentator is right that only old trials that met inclusion criteria for our systematic review on short versus long stays were identified, but he is wrong when he says that our findings are of distant relevance to today's psychiatry. We started the review with an important question. Over the past 40 years the lengths of patients' stays in hospital have been reduced so that mental institutions can be closed and to contain costs in many countries. As a result, there is serious public concern about the alternative community care after many deaths and repeated acute care admissions of seriously mentally

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ill patients (Todd 1976, DoH 1994). Some governments are now suggesting increasing hospital-based care as part of their modernisation programmes (DoH 1999). With all these policy changes, we simply asked: which is more effective from the patient's point of view, longer or shorter stays? The question is important to today's mental health service, and so the low level of research is both a disappointment and a challenge. We also share the concern that policy is driven by little research evidence, whether made by managed care companies in the United States or by the NHS in the United Kingdom (Knapp 1990). Yet most resources are spent on wards, staff, and buildings. There have been important advances in the treatment of serious mental illnesses, so why is there no recent robust and pragmatic research on how hospital care is organised and delivered?

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Contributors

Comment from Spencer Eth, New York, November 1999.

Reply from Paul Johnstone, Middlesbrough & Gabriella Zolese, London, November 1999.

WHAT'S NEW

Last assessed as up-to-date: 8 November 2007.

Date	Event	Description
6 October 2010	Amended	Contact details updated.

HISTORY

Protocol first published: Issue 3, 1996

Review first published: Issue 4, 1999

Date	Event	Description
22 July 2009	Amended	Plain language summary added.
30 October 2008	Amended	Converted to new review format.

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* *Indicates the major publication for the study*

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PLAIN LANGUAGE SUMMARY

Length of hospitalisation for people with severe mental illness

Over the last hundred years medical opinion as to whether people with a severe mental illness should stay in hospital for months and years versus a few weeks has changed. This has been helped by the advent of medication for some of these illnesses. Consequently, in the developed world hospital stays are now relatively short. However even in these countries there is still some doubt as to whether really short admissions are helpful because the person does not get institutionalised, or harmful because the symptoms and possible causes of the illness are not completely addressed. There are a group of patients who have short but frequent admissions (’revolving door patients’) and others who despite a variety of treatments stay in hospital for a long time (’new long stay patients’).

To identify what length of stay in hospital is most helpful, this review looks at trials comparing hospital stays of over 28 days to those that are under 28 days. Six studies were found containing a total of 2030 people, four in the USA and two in the UK. However, differences in the design of the trials made them difficult to compare to each other. In addition all of the trials were done before 1980 when there was less choice of medication and greater differences in the diagnoses between the US and the UK. One study showed a statistically significant drop in the number of people readmitted to hospital in the short stay group, but these people had almost daily input from a clinician. The remaining studies showed no significant difference between the two groups. In two trials, the short-stay group were more likely to be employed after two years. In three trials the long-stay group were more likely to stay in hospital after the date they were supposed to be discharged. Although the data from these trials is not extensive there is a suggestion that short stays in hospital in themselves do not cause people to become ’revolving door patients’. A new trial that is large, simple and clearly recorded, would help to confirm these results.

(Plain language summary prepared for this review by Janey Antoniou of RETHINK, UK www.rethink.org).