**ROUND THE CORNER**

**Length of hospitalisation for people with severe mental illness: is the longer the better?**

**Commentary on… Cochrane Corner**

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***Biography***

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***Summary***

The Cochrane review in this month’s Cochrane Corner (Babalola 2014) compares short-stay hospital admission to long-stay/standard admission in patients with severe mental illness for a number of outcomes in a total 2030 participants from 6 randomised trials. It reached a conclusion supported by limited evidence that short admissions in mental health units does not increase the risk of death, readmission, or worsening of mental state, and poses less risk of delayed discharge and patient’s unemployment. This Round the Corner commentary examines the available evidence from previous studies and discusses its relevance to current practice.

***Declaration of interest***

None.

***Introduction***

NHS mental health beds have been reduced by 73% between 1987/88 and 2018/19, from 67,100 to 18,400 (Wyatt 2019).This reduction has been attributed to the Government policy of ‘care in the community’. The number of mental health beds occupied has also decreased, but at a slower rate, leading to increased bed occupancy (Wyatt 2019). Bed occupancy in mental health units was reported to have reached 90% by 2018/19 in England (Wyatt 2019), exceeding the Royal College of Psychiatrists’ optimal recommendation of <85% (Royal College of Psychiatrists 2011: pp. 10). While medical non-psychiatric units have reduced bed occupancy through reducing the average length of stay, mental health units have mainly reduced patient admissions. This could be explained by the reported increased average threshold for admissions across England (Wyatt 2019).

Length of hospital stay for mental health conditions varies across the NHS. Multiple factors increase the likelihood of longer hospital stay, including male gender, BAME ethnic background, being homeless or in supported accommodation, diagnoses of psychosis, and number of care coordinators (Newman 2018). Other factors not studied but recognised by clinicians include variation in admission threshold and in estimation of risk level between clinicians. The national service framework for mental health services (National Health Service, 1999) endorses short stay with good quality community care and rapid follow up. Nevertheless, the average length of stay in a mental health bed across England is still around 7 weeks (Wyatt 2019).

***The clinical question of the Cochrane review***

This review aims to compare short- *versus* long-stay admissions in mental health hospital for people with severe mental illness. A total of 2030 participants from 6 randomised trials [Box 1] were included in this review. The largest trial (Burhan 1969) involved 1169 participants. Participants were described as “people with schizophrenia, related disorders or 'severe/chronic mental disorders/illnesses', however defined”. All trials focussed on an adult population, excluding children, adolescents, the elderly, and those with learning disabilities. Kennedy and Hird’s study (Kennedy 1980) included patients from unselected acute psychiatric settings, such as patients with organic brain disease and alcohol problems, introducing heterogeneity into the pooled analysis.

The two interventions under comparison were “planned short-stay” and “planned long-stay”, however defined within the studies. The authors proposed an arbitrary cut-off value of 28 days based on the compulsory detention period for assessment defined by the Mental Health Act 1983, but there was noticeable variability in the definitions of short-stay – one (Herz 1975) to four weeks (Glick 1975; Glick 1976) – and of long-stay between studies.

Based on duration since admission, outcomes were categorised into: short (<3 months), medium (3-6 months), long (6-12 months), and longer term (1-2 years or more), but only data for long term outcomes were available for analysis. The reported outcomes were grouped into primary outcome (global state) and secondary outcomes (death, change in specific symptoms of schizophrenia, readmission, premature discharge, delayed discharge, leaving the study early, and general functioning). Binary data alone were used to express the effects of interventions, as standard deviations for scales used in the trials were not reported and could not be obtained.

***Methodology***

The original search identified 5 trials which met the inclusion criteria. The 2005 update added 1 more trial to the analysis. For the latest 2012 update of the Cochrane review (Babalola 2014), the authors searched the Cochrane Schizophrenia Group's register, which is based on MEDLINE, EMBASE, CINAHL, and PsycINFO. They also searched the references of identified studies and contacted first authors of included studies for further unpublished trials. Eventually, the review included a total of 6 randomised trials conducted between 1960-1980. Two quasi-randomised trials were identified but were excluded from the main analysis.

Two authors independently assessed quality of the included trials using the GRADE system, and risk of bias using criteria from the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011).Although all trials were randomised, no trial explicitly reported the means of randomisation. Glick *et al* (Glick 1975; Glick 1976) had the lowest risk in terms of random sequence generation and allocation concealment (selection bias) [Box 2].

Blinding of participants and clinicians could not be realistically achieved. However, other forms of blinding such as blinding of data analysts [Box 3] could have been implemented. No form of blinding was reported in any trial involved, resulting in high risk of performance and detection biases.

Excepting the Burhan trial (Burhan 1969), all trials have reported incomplete outcome data in their analyses at different percentages, the largest being in the Hirsch *et al* trial (Hirsch 1979) with 53% exclusion at one year. Only data for two outcomes could be used from this study (readmissions and loss to follow-up at one year) as intention-to-treat numbers could not be calculated.

Data was reported using standard estimation of risk ratio (RR) with 95% confidence intervals (CI). Although *p*-values were not reported, CIs are more informative than *p*-values (Du Prel 2009) [Box 4]. For statistically significant results, number needed to treat to provide benefit (NNTB) / to induce harm (NNTH) was calculated with 95% CI. Heterogeneity was assessed on clinical, methodological, and statistical levels. The authors chose to use a fixed effect model over random effects ones for data synthesis.

***Results***

Disappointingly, no study reported outcome data for the primary outcome (change in global state).

No significant difference in reported deaths was found at 2 years follow-up (n = 175, RR 0.42, CI 0.10 to 1.83). Causes of death related to mental illness were unfortunately indistinguishable from other causes.

Improvement of mental state was not different between groups, whether measured by the Psychiatric Evaluation Form (PEF) scale (n = 61, 1 RCT, RR 3.39 CI 0.76 to 15.02) or the Health- Sickness rating scale (HSRS) (n = 61, 1 RCT, RR 0.97, CI 0.31 to 3.01).

No difference in readmission rates at one year (n = 651, 4 RCTs, RR 1.26 CI 1.00 to 1.57) or two years (n = 229, 2 RCTs, RR 1.03 CI 0.78 to 1.36) was found. Interestingly, adding data from the Burhan trial (Burhan 1969) introduced elevated heterogeneity into the analysis of this outcome (I2 = 71.7% at one year and 92.7% at 2 years), resulting in significantly fewer readmissions in the short-stay group (n = 1169, RR 0.22, CI 0.07 to 0.67 at one year, and n = 1169, RR 0.21, CI 0.11 to 0.41 at two years). A newer study (Moran 2017), however, disagrees with such findings, reporting association between higher rates of emergency readmissions and shorter length of stay.

Adding data from the Kennedy and Hird trial (Kennedy 1980), however, introduced heterogeneity (I2 = 62.4% at one year) in the opposite direction: the short-stay group had more readmissions, albeit for shorter duration (RR 2.23, CI 1.3 to 3.7 at one year).

No difference in early discharge rates (n = 229, 2RCTs, RR 0.77 CI 0.34 to 1.77) was found. Significantly fewer delayed discharges were noted in the short-stay group (n = 404, 3 RCTs, RR 0.54 CI 0.33 to 0.88), which agrees with the concept of institutionalisation, where longer hospital stays make it difficult for patients to re-integrate into society. This should be interpreted with caution, though, as including data from quasi-randomised trials eliminated this effect. Even though these studies are of lower quality in terms of randomisation, it is hard to say with certainty whether this explains the heterogeneity.

There was no difference in incidence of self-harm episodes (n = 247, 1 RCT, RR 0.17 CI 0.02 to 1.30). Acts of self-harm are more common in certain groups of patients (e.g. borderline personality disorder) and no association between diagnostic category and self-harm episodes was reported in this study.

Participants in short-stay groups were more likely to be employed at two years (n = 330, 2 RCTs, RR 0.61 CI 0.50 to 0.76, NNTB 5, CI 4 to 8). Again, this agrees with the concept of institutionalisation. No difference in work attendance at either short term (three weeks) (n = 247, 1RCT, RR 1.50, CI 0.61 to 3.65) or medium term (four months) (n = 247, 1RCT, RR 1.70, CI 0.75 to 3.85) was reported.

Although the Glick *et al* trial (Glick 1975) reported that the mean cost of outpatient care was higher in the short-stay group, there was no reference to the statistical significance of this difference. This could be explained by the more intensive community follow-up, although this was not reported in the trial.

***Discussion***

*Quality of evidence*

Quality of evidence was low or very low for all outcomes. Reasons for this varied between a single study supporting findings, low number of participants, inconsistency between studies, and risk of bias related to randomisation, allocation concealment, and blinding. The fact that these trials were reported before the development of the Consolidated Standards of Reporting Trials (CONSORT) in 2001 could explain some of the biases, particularly the limited reporting of randomisation and unclear reasons for participant loss at follow-up. Implementing reporting standards using CONSORT will, in a way, also improve the design of clinical trials.

*Participants*

The broad definition of participants included different diagnostic categories. There was no clear definition or cut-off for severity, and data from trials were insufficient to report on subgroups with similar conditions or severities. This is important as different psychiatric diagnoses have different care needs and different prognoses. For example, patients with severe mental illness with prominent depressive features are at higher short-term risk for suicide following discharge (Olfson 2016).

The Health of the Nation Outcome Scale (HoNOS) (Wing 1998) [Box 5] could provide an answer to the subjective description of severity.

Matching participants is also essential to avoid bias [Box 6]. The Glick *et al* trial (Glick 1975) reported important differences between groups, including education, socio-economic status, pre-morbid adjustment, mean dosage of chlorpromazine equivalent. Authors found it difficult to estimate the degree of “confounding effect” exerted by these differences. Statistical correction for a confounding variable is theoretically possible through regression analysis, but there was no indication that this was attempted.

*Search strategy*

Other strategies to make a thorough search for trials that the authors should have considered include foreign language literature, grey literature, and references of references. Foreign language literature is particularly important here, considering the closure of large mental health institutions in the North America and Europe since the 1960s.

*Small study effect*

Since fewer than 10 studies were included in this review, using funnel plot to assess for reporting/publication bias was not appropriate (Higgins 2020). Such bias could lead to ‘small-study effect’, a phenomenon in which estimates of intervention effects in small studies tend to be greater than in large ones. Small study effects are specifically relevant to this review, given the large difference in sample size between some studies. The random effects model weighs the studies relatively equally (Higgins 2020), which could enhance the “small-study effect”, an advantage for using fixed effect model. It would have been helpful if results were expressed using both models to see if such effect was present.

*Heterogeneity*

Heterogeneity was observed in the pooled analysis of readmission rates. Considering that Kennedy and Hird (Kennedy 1980) trial is a much smaller study than Burhan (Burhan 1969) trial and included different categories of patients (patients with organic brains diseases and alcohol problems), clinical significance should be interpreted in that light.

*External validity: Old evidence for current practice*

A striking observation in the Cochrane review is the age of the trials (1960-1980). The more recent change from large psychiatric institutions to smaller psychiatric units, and the reduction of mental health bed numbers highlights that the current practice of psychiatry is different and community mental health services are utilised more efficiently. New studies in the current circumstances will better reflect the outcomes – or shortcomings – of the proposed interventions.

Not only the studies were old: the review itself was last updated in 2012. Is there any newer research in this area? Searching PubMed using similar parameters to the review revealed no studies comparing outcomes in the two interventions. Research in this area is mainly focused on factors affecting length of admission and readmission. Obviously, this is not a comprehensive search, but it suggests a dearth of research in this area – confirming what had already been noted when two updates of the Cochrane review in 2007 and 2012 failed to identify any new study. The lack of standard definitions for short- and long-term admission, the pressure on beds mentioned above, and the difference in presentation and outcome of different psychiatric diagnoses could provide an explanation for the paucity of evidence available.

*Cost of care*

No trial reported the cost of inpatient stay, indirect costs (like travel), and intangible costs (such as inconvenience). It is worth noting the exclusion of four trials in the latest update because they compared day hospital care with inpatient stay and/or focused on ‘*economic evaluation*’. Data from these trials however could shed some light on the economic aspects.

***Conclusion: Is the available evidence sufficient for clinical practice?***

This Cochrane review (Babalola 2014) provides low-quality evidence that short admission does not increase the risk of death, readmission, worsening of mental state or reduced work attendance in comparison to long admission. There is also limited evidence that short admission could indeed be associated with a lower risk of unemployment, and result in reduced delays in discharge from hospitals.

This evidence is reassuring concerning the safety and outcome of brief admissions, but is in much need of an update. The age of the trials, the low quality of evidence from the review, and the lack of differentiation for outcome measures in relation to diagnosis all speak volumes about the need for contemporary, well-designed, focussed randomised trials to inform current mental health inpatient practice.

Assessment of cost is key to decision-making, especially for health care policy makers. If studies of the cost of short- and long-admissions to health care are to be conducted, researchers should investigate direct, indirect, and intangible costs to inform decision-making regarding the most efficient management strategies.

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***Boxes and Figures***

[Box 1] Randomised trial versus randomised controlled trial

A randomised trial is an experimental study where participants are randomly allocated to either an intervention or a comparison group (e.g., another intervention). When the comparison group receive either placebo or no intervention, the trial is called randomised controlled trial.

[Box 2] Random allocation sequence

Random allocation sequence is a key component of randomised trials (Dettori 2010), and if performed correctly on a large sample, it does significantly reduce the risk of bias, especially selection bias. It has two components: generating an unpredictable random sequence (random sequence generation), and concealment until participants are assigned to intervention/control groups (allocation concealment). Please note that not all methods that may be described as “random allocation” are in fact random. Examples of methods to be avoided if random allocation is desired include using hospital chart numbers, alternating patients sequentially, or assigning by date of birth. The best methods for random allocation use a random-numbers table or a computer software program.

[Box 3] Blinding

Blinding is essentially ‘masking’ research design elements such as group assignment, treatment agent, and research hypotheses. It is particularly important when subjectivity of assessment is expected. Blinding can be done on three main levels (Page 2013):

* Blinding of participants: to minimise altered attitude and cooperation due to knowledge of group assignment.
* Blinding of health care providers: which is important when knowledge of assignment could change normal care decision or outcome monitoring, due to excitement or enthusiasm about the intervention.
* Blinding of data collectors: to ensure objectivity in recording response to interventions under comparison.

[Box 4] *p*-value and confidence intervals

*p*-value is the probability that the outcome’s results would have occurred by chance. Standard scientific practice defines a *p*-value of less than one in twenty (*p <* 0.05) as “statistically significant”, and a *p*-value of less than one in a hundred (*p <* 0.01) as “statistically highly significant”. *p*-value allows a binary (yes/no) decision to be made about a previously formulated null-hypothesis.

A confidence interval (CI) is a range of values in which the results of a statistical test fall within, with predefined probability. 95% is usually used in statistical tests of clinical trials, which means the true value of the test lies with the defined interval in 95 out of 100 times.

Confidence intervals and *p*-value are complementary measures, and usually are both reported in research articles. Confidence intervals have the advantage of providing information about the range of observed effect size, and the width of the confidence interval gives an idea of the precision of the results (Du Prel 2009).

[Box 5] HoNOS

HoNOS (Health of the Nation Outcome Scales) is an instrument developed by the Royal College of Psychiatrists to assess the health and social functioning of people with severe mental illness. The scale is widely used by mental health foundation trusts in England, and provides a validated tool to assess severity of mental illness. It is formed of 12 scales, and each scale is given a value between 0 and 4 by the clinician (Wing 1998). HoNOS scales include:

1. Behavioural disturbance.
2. Non-accidental self-injury.
3. Problem drinking or drug use.
4. Cognitive problems.
5. Problems related to physical illness or disability.
6. Problems associated with hallucinations and delusions.
7. Problems associated with depressive symptoms.
8. Other mental and behavioural problems.
9. Problems with social or supportive relationships.
10. Problems with activities of daily living.
11. Overall problems with living conditions.
12. Problems with work and leisure activities and the quality of the daytime environment.

[Box 6] Matching

Matching means pairing or similarities between participants from comparison groups in the values of the matching variable(s). These matching variables are determined based on their potential association with the outcome (usually the primary outcome).

It aims at reducing bias due to baseline group differences, thereby reducing the variability, and increasing the precision, of the group comparisons. (Simon 2007)

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