Reviewer’s report

Title: Interventions aimed at reducing problems in adult patients discharged from hospital to home: a systematic meta-review.

Version: 1 Date: 22 January 2007

Reviewer: Suzanne Richards

Reviewer’s report:

General

1. Remove the term ‘viz’ from text – it is unnecessary before colons etc.
2. Edit search for ‘..’ and replace with ‘.’
3. Edit search for ‘.,’ and replace with ‘.’
4. Edit search for ‘inclusioncriteria’ and change to ‘inclusion criteria’

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Is the question posed by the authors new and well defined?

General comment: The literature around the effectiveness of discharge planning from acute hospitals is vast and complex. The authors undertook a review the evidence from systematic reviews examining the effectiveness of interventions aimed at altering some aspect of the discharge process from acute hospital. From the outset this was an ambitious undertaking, and I was interested to see how the authors would deal with the major challenges that any systematic reviewer (of individual papers, or indeed of systematic reviews) would encounter when attempting a synthesis of this kind. The main strength of this paper is the rigour with which the authors have searched for good quality reviews - it will be an invaluable resource which can sign-post the reader on to good quality reviews in the field of discharge planning. Notwithstanding this, I have some reservations about the meta-synthesis approach.

Point 1. The main challenge from the outset is clearly defining what is meant by a ‘discharge intervention’, and I was left confused as to the scope and remit of the review. The authors describe using their own framework for categorizing interventions (pg 11) into one of two categories ‘discharge preparation’ and ‘discharge support/aftercare’. However in the next paragraphs (pg 12) they reproduce a detailed framework by Parker et al describing four categorizations (discharge planning protocols, CGA, discharge support arrangements, educational intervention). The results would appear structured around Parker’s framework. The authors need to clarify which framework they adopted to provide the reader with a meaningful structure around which the interventions about to be discussed would be synthesized, and, perhaps more importantly, help the reader to understand which types of interventions falling under the broad banner of discharge interventions should not be compared directly (if any).

The authors have synthesized interventions which I would probably have not combined. For example, hospital-at-home (intermediate care) type interventions reviewed by Shepperd 2001 are qualitatively different from interventions aiming to standardized the care pathways management of patients being discharged from hospital (e.g. reviews by Parker et al, or Richards & Coast). Intermediate care services aim to substitute patient bed days in acute wards with alternatives, such as an earlier discharge to intensive home nursing and rehabilitation or a stay in a rehabilitation unit, or to prevent admissions to an acute hospital ward. Thus for these types of service, the fact that intermediate care services provide comparable care to usual care is not a negative finding– as one might argue that the quality of patient care might have been reduced through substituting acute care with less intensive alternatives. In contrast, interventions aimed specifically at standardizing and implementing a discharge plan from an acute ward which does not substantially improve patient outcomes compared with usual care is arguably a negative findings – as the intervention is supplementing the normal care pathway. A fuller discussion on what constitutes a ‘positive’ outcome for different types of services would be beneficial in the methods and discussion sections.

Are the methods appropriate and well described, and are sufficient details provided to replicate the work?
Point 2. Although the authors reference a quality checklist for synthesizing data from systematic reviews, there are no methodological references given describing the rationale for conducting meta-reviews (e.g. in what context are they appropriate, and when should they not be undertaken). Is this because this approach is relatively new?

Point 3. All the reviews selected achieved high quality scores because the authors clearly defined specific types of complex intervention of interest, and also excluded interventions which they believed to be substantively different (perhaps evidenced by the lack of duplication of papers between reviews?). My own view of the literature reviewed (both within component reviews, and across different reviews) is that it is too heterogeneous for a meta-review at such a broad level of ‘discharge planning‘. Indeed, many of the original authors of the systematic reviews were so unsure the comparability of interventions selected within their own, tightly defined areas, was that that the empirical findings could not be pooled.

My concerns could probably be addressed if the discussion was structured around different types of service (e.g. Parker’s framework) and a discussion of changes in outcomes within these broad headings, rather than as it stands it currently stands. The discussion would then flow on to consider broad sub-groups of intervention in turn, without attempting to collapse it down into one over-arching category of ‘discharge planning’.

•Are the data sound and well controlled?

Point 4. The outcomes of interest (e.g. physical functioning, emotional outcomes, or social status) are not defined in the methods section and this must be inserted. From the text of the results section, it would appear that emotional outcomes include any patient-based outcome which does not contribute to physical functioning (e.g. quality of life, depression, anxiety, cognitive function, patient satisfaction etc). This grouping is too broad, and requires further clarification in both the methods and throughout the results sections. No definition of social status outcomes is given in the results section – and I am unclear as to what measures this might include.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Page 10, paragraph 4 (The third stage etc…). From besides being one of the few found for which psychometric properties had been documented [87] and which had been found to meet several important criteria [88]. Last bit of this sentence needs clarifying (? What important criteria).

Page 10, end of paragraph 4. The authors describe calculating the mean of the quality assessment scores for each review to generate the final quality judgment. What would they do if the reviewers differed wildly in their assessments, i.e. one review gave a very good quality score, whilst another rated it poorly?

Page 14 (Results). When discussing the inter-rater reliability of the two assessors, the term concordance is used incorrectly (it is not the % agreement). Where a proportion in agreement between the two assessors is given, this should be referred to as the ‘crude agreement’, whilst kappa is the ‘chance-corrected agreement’

Page 15. Replace title ‘characteristics of the final 16 studies’ with ‘characteristics of the final 16 reviews’.

Page 16 (results). The total number of patients involved in the reviews varied etc. What do you mean by involved – is this recruited into the component studies, and/or provided useable data. Either clarify your definition of how they were involved, or drop these figures.

Page 17 (results). Last sentence. Re-phrase ‘a large variety in what’ to ‘showed considerable heterogeneity in terms of’

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field
Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.