Reviewer's report

Title: Epidemiology and outcomes of people with dementia, delirium and unspecified cognitive impairment in the general hospital: prospective cohort study of 10,014 admissions.

Version: 0 Date: 10 Feb 2017

Reviewer: Rowan Harwood

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1. This is a report of a cohort study of older people admitted to hospital as a medical emergency. Linked routine health service data were used to achieve complete follow up for mortality and hospital readmission. The term 'cognitive spectrum disorders' (CSD) was coined to cover the overlapping diagnoses of delirium, dementia, delirium superimposed on dementia and uncharacterised cognitive impairment. About 1/3 of admission had a CSD. Mortality and readmissions were considerably higher in those with CSD than without. Different categories of CSD had broadly the same outcome.

2. This is an important study that adds considerably to the data available in the field. Population inclusion was reasonably complete (79%); the study was larger than any previous work; cases were (relatively) unselected; linkage to validated and complete health services electronic records appears to have been well-done, and adds considerable strength to the study. An 'incident cohort' was defined as those who had not been previously admitted in the past 6 months.

3. Some issues would benefit from further explanation and discussion. These are minor points.

   a) The 21% not recruited included those who were likely to be discharged immediately, those thought likely to die, those being admitted to critical care and, doubtless some who were missed or declined to be assessed. If available, these different reasons should be given. Some demographics and follow-up data is available on this group. Potentially their exclusion might have an impact on prevalence and outcome statistics cited.

   b) Furthermore, the notion of 'unselected' study should be more critically discussed. The study was of 'medical' admissions, which is itself arbitrary to a degree, and liable to vary by time and place. Who is admitted or immediately discharged depends on alternative services in place, and social expectations. A frail older person with a fractured pelvis or neck of humerus, might be deemed trauma/orthopaedic or geriatric medical. Someone with cancer and neutropenic sepsis might be oncology/haematology or medical. Someone with a brain tumour might be diverted to a regional neurosurgical centre. We need to know exactly who was included or excluded for the population reported (ie something of the local service configuration)?
c) The prevalence of CSD is slightly lower than that reported previously in the best studies. Possible explanations would be lack of selection, or different selection, or different age profile (e.g., if the current studies population was more 'young old'). For example, 17% had known dementia, and 5% possibly undiagnosed dementia, compared with 40% given in previous hospital admission studies. The proportion of people with delirium who had underlying dementia looks particularly low. The age-specific prevalence's given in figure 1 are welcome; but at age 80-84 I would expect about 20% of the general population to have dementia, the highest possible prevalence here (dementia, DSD, low AMT) is no more than 15%; at age 85+ perhaps 30% of the general population will have dementia, in this study about 22%. The figure for delirium is more expected, if on the low side of the range in previous studies.

d) The figures in appendix table 1 are also very welcome, but appear to be different from those in figure 1 (and the text) and are more in keeping with those from the literature. Is this because it refers to the incident cohort? Which figures are more meaningful? Does appendix table 1 need to be in an appendix given the online format?

e) an explanation of how the incident cohort differs from the whole sample would be useful (in terms of interpretation and usefulness of data). This could be in terms of a rationale for defining the incident cohort. The size of the incident cohort should be given in the text.

4. The introduction is quite long, but well written and informative. I would support its inclusion without editing.

5. Ethics: the study used routinely collected clinical data and follow-up was from public-health datasets. Adequate permission was obtained and safeguards were employed. Appropriate ethical review was obtained. I have no concerns about the ethics.

6. Discussion: good, but might be more structured ('BMJ-style' maybe: summary, strengths and weaknesses, context/other literature, interpretation, implications/further work). Might the insensitivity of the AMT and CAM have impacted on results? Discussion paragraphs 4 and 5 is very important. Discussion paragraph 6 ('Further longitudinal research …') states that worse outcomes might be due to less effective care. This was not supported by Goldberg et al BMJ 2013;347:f4132 in which best-practice specialist care for this population resulted in no difference in mortality compared with standard medical and geriatric care.

7. It would be nice to see readmissions cited separately from deaths + readmissions in the tables. It might be possible to combine tables 3 and 4 (especially if not citing proportions to 3SF).

8. Minor issues
a) Abstract methods AMT score 8… '/10' would be useful to add

b) Abstract Findings 'dementia superimposed on delirium' should read 'delirium superimposed on dementia'

c) Introduction 2nd page line 34 'prevalence depended on the population studies… age would be another potential explanatory factor

d) care over use of past tense throughout (eg methods, 2nd page, line 15 'where death is expected')

e) citing p-values to 3 significant figures is unwarranted (as indeed for some of the proportions)

f) Results last line 'mortality and readmission …. were poor'. 'Poor' is a judgement. 'High' would be a more appropriate statement of fact. The mortality may (or may not) have been inevitable, the re- admissions may (or may not) have been unavoidable or necessary. No data are provided to justify this statement, nor indication of any comparator.

g) All tables give n as well as % (and labelling unclear)

h) Appendix table 1 'Logistic regression' should not cite p-values without effect size (ie odds ratios); or should state that the figures are p-values if the crude proportions are considered adequate to express ES (which they are)

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes

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I sit on the External Steering committee, on behalf of the funder (UK NIHR), of the parent study behind this work

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