Author’s response to reviews

Title: Falls in advanced old age: recalled falls and prospective follow-up of over-90-year-olds in the Cambridge City over-75s Cohort study

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Author’s response to reviews: see over
Claudia Browning and Lolu da-Silva  
Assistant Editor and Senior Assistant Editor  
BMC-series journals  
BioMed Central Geriatrics  
Middlesex House  
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13 November 2007  

Dear Ms Browning and Dr da-Silva  

Falls in advanced old age:  
recalled falls and prospective follow-up of over-90-year-olds in the CC75C study  

Thank you for your e-mail of 23 October sending reviewers’ reports on our paper and for this evening’s message too. We welcome the opportunity to address the issues they have raised and hope that the revisions we have made will clarify our methods on some of their points, address a couple of statistical issues and present the findings graphically in such a way that their information is more readily understood by your readers.  

In addition to the revised paper, which includes revised versions of the two tables, there are now four figures rather than three.  

Using the Additional Material option we are including a document that presents all the figures with their titles and captions and a version of the revised paper that tracks changes made since the version originally submitted, for your editors’ and referees’ convenience. The revised Abstract has also been added via this option as pasting this into the earlier Abstract window appears to have no effect.  

The point-by-point responses to points raised by each reviewer have been added into copies of their three reports appended to this letter in a format that we hope makes for easier checking that everything has been addressed. If anything is still unclear please do not hesitate to contact us and we will try to respond as a matter of urgency. We look forward to hearing from you again.  

Yours sincerely  

Jane Fleming (Corresponding author), Fiona Matthews and Carol Brayne  
on behalf of the CC75C collaboration
Reviewer's report

Title: Falls in advanced old age: recalled falls and prospective follow-up of over-90-year-olds in the Cambridge City over-75s Cohort study

Version: 1  Date: 21 August 2007

Reviewer: David Ganz

Reviewer's report:

General

This is an interesting manuscript that addresses a growing cohort of individuals -- the "oldest old," about whom little is known. I think this manuscript is an important contribution to the literature and is generally well written but requires significant revision. My detailed comments follow below.

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

1) I am having some difficulty understanding the study's methods. There seems to be an attempt to compare retrospective to prospective data collection, as has been the case in previous studies. However, the authors seem to be comparing patients' baseline recall of falls in the year prior to baseline, to prospective collection of data on falls in the year subsequent to baseline. Because the cohort is aging over time and life events may occur in one year but not the other, to me it does not make sense to compare the two separate years. In previous work, the same year has been looked at both prospectively and retrosepectively, by having patients recall their falls at the END of the 1 year of prospective follow-up, or at intervals during the follow-up (reference 4 contains details). Figure 1 seems to be trying to get at this but the Methods section does not explain how the data in Figure 1 were arrived at. In addition, the superimposed graph of prevalence vs. incidence and the category "no falls recalled" make this Figure quite unclear. In sum, if patients in the study were not asked at the end of the year of prospective data collection to recall falls in the past year, I think the whole prospective vs. retrospective comparison should be dropped, since it applies to two different years. The study still provides important epidemiologic data of interest to researchers, and can be reported descriptively.

Time and funding limitations meant that this study did not include a further round of interviews at the end of the follow-up year so the methods reported in reference 4 could not be replicated. It was not our intention to imply that the two different years covered by retrospective recall and prospective follow-up were directly comparable, for just such reasons as pointed out here. We hope
that changes to the Abstract (para. 1), the Background (p.5, last line), Methods (p. 6) and Discussion (p.14, last para) sections all help to clarify this.

2) Page 7. Poisson (or negative binomial) regression is likely to be inappropriate for this data if falls are clustered (i.e., one fall tends to occur right after a previous fall rather than at random). A key assumption for Poisson data is that there is a random process generating rare events that are independent of one another. That assumption seems to be failed here.

Both Tables have been changed, and relevant sections of text that refer to these results, to present the relative risks obtained from re-running the negative binomial regression analyses adjusting for possible effects of clustering (as explained now in Statistical Methods, p.8 l.1, with comment also added to Discussion – Effects of multiple falls... p.18 para.1 l.1). The *cluster(varname)* option in Stata allows the analysis to treat every participant's own falls as not necessarily independent of each other. This assumption implies there may be even less lack of independence between all falls by one individual than just dependence of closely temporally clustered falls on each other. However, since this referee’s comments are a valid criticism – 39% of the falls occurred in such “clusters” and these involved 32% of the people who fell – this is an assumption worth making rather than ignoring the clustering altogether. As would be expected, this re-analysis affected the variance and hence confidence intervals (almost all now tighter) but not the point estimates.

3) Page 7. Please elaborate on the variables available for insertion into regression models, and why these variables were chosen. A significant concern in this manuscript are the multiple tests run on the data without any adjustment for multiple comparisons and without pre-specified hypotheses. These make the subsequent results difficult to interpret. This must somehow be addressed in the Methods section.

Explanation on focus in this paper added to Methods(Statistical methods, p.8, l.3-8) and Discussion (bottom of p.20 – top of p.21). Already comment on caution needed re interpreting significance in Discussion (p.15, end of para.1)

4) Page 7. In situations where the outcome of interest is common (such as number of fallers in this cohort), the odds ratio is known to overestimate the
relative risk. In cohort studies the relative risk can be calculated directly using crude data and should be. For multivariable logistic regression (not being used here but could be), methods exist to adjust the odds ratio to get a corrected relative risk (see for example Zhang and Yu, Journal of the American Medical Association 1998; vol 280; pg. 1690). More sophisticated statistical methods also exist to generate a relative risk from logistic regression, such as using predicted probabilities. Given the availability of these methods and the known tendency of the odds ratio to overestimate the relative risk when the outcome of interest is common, relative risks should be reported rather than odds ratios in all cases in this manuscript. Whichever methods you choose, please report them in the Methods section.

All risk estimates previously presented as odds ratios have been re-calculated as suggested (directly where possible, using Zhang and Yu’s method to get adjusted RR from ORs of multiple variable logistic regressions – see explanation added in Statistical Methods, p.7 last para.). Effect size was reduced as expected but these changes did not affect which variables appeared significant.

5) Page 12. The sentence states, “Of these over-90-year-old men and women 60% fell at least once in the year after interview, closely matching the proportion who remembered falling in the year before interview (58%).” As discussed earlier, comparing two separate years is problematic. There are multiple reasons why this is so, including study attrition causing only healthier patients to have data points at both times (probably less of a problem in this sample), the noise introduced into the data by health histories that change from year to year, and the relatively small sample size (although this is an issue with many falls studies). You could provide the same data to the reader (descriptively useful) without trying to make inference about it, especially as the results are mixed for single fallers, repeat fallers, and total number of falls.

See Discussion following the above quote (p.14 last para.)

6) Discussion section. Please shorten this section significantly and focus on the limitations you think are most important. The Methods section, however, should be enlarged to provide more detail.

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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)

1) In the abstract, conclusion section, the authors write that "Recall methods may underestimate numbers of repeated falls but not the proportions of fallers." However, in the results section, they state that the "proportion reported to have fallen more than once was lower using retrospective recall than prospective
reports (34% vs. 45%), as were fall rates..." The two sentences seem to be at odds. Please clarify (or refocus the abstract entirely to be consistent with prior comments).

These statements are not at odds but we hope that re-wording in both Abstract and Discussion will remove any confusion.

2) Page 6, sixth line from bottom. Sentence reading "If resident in a care home..." needs to be fixed.

Changed to clarify.

3) Page 14, "prevalence does not increase in proportion to length of recall period..." A period is needed at the end of this sentence.

Added.

4) Table 1. Please present crude relative risks instead of unadjusted odds ratios. It does not make sense to present incidence rates for recalled falls because these data are recalled rather than prospectively collected, so I would remove the incidence information.

Odds ratios have all been changed to relative risks in both tables, with corresponding changes in the text wherever these results are mentioned – see response re Major Revisions 4) above. The use of the “incidence” concept to give a measure of apparent fall frequency from remembered falls is explained in the Statistical Methods (Page 7, last para.) and again at the beginning of the Results section (p.9 last para.).

5) Table 2. Please present crude relative risks rather than unadjusted odds ratios. please remove negative binomial regression estimates of incidence rate ratios (labeled as relative risk in the "incidence" column) for reasons discussed above (inapplicability of negative binomial regression to clustered events).

ORs → RRs as described for 4) above. RRs from negative binomial regressions using data on falls frequency have been adjusted for possible effects of clustering so have not been removed.

6) Figure 1. The prevalence data appear to apply to the previous year, yet the incidence data apply to the current year. Thus the two sets of data should not be superimposed. If the authors choose to keep either the prevalence or the
incidence data (incidence data are more likely to be useful), they should provide additional detail as to how the data were derived (e.g., how does "period of recall" fit with incidence data?).

The incidence data does refer to the same (remembered) year as the prevalence data. The super-imposed format, and the category “No recalled falls” seem to have created confusion so Figure 1 had been changed, separating the information into two paired figures (1a and 1b) and showing only recall periods for which data were available for both.

7) Figure 2 is quite useful and a nice contribution.

This figure (now Figure 3) has been re-done, showing only “fall cluster units” of the same denominator (2, 3, 4 or more falls in a week) plus a “total” column to get across the fact that the total number of people affected by multiple falls in short time periods is not the sum of each separate category because some people fell in both.

8) Figure 3. Please label the y-axis of the two graphs so that we know what we are looking at. I think this should be "Percent free of falls in subsequent year" or something to that effect.

Added axis labels and Figure 3a / 3b.

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 whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.
Reviewer’s report

Title: Falls in advanced old age: recalled falls and prospective follow-up of over-90-year-olds in the Cambridge City over-75s Cohort study

Version: 1 Date: 3 September 2007

Reviewer: M. Clare Robertson

Reviewer’s report:

General

This study reports falls recalled in the previous year in a representative sample of 90 women and 20 men aged 91 to 106 years (Abstract, 105 years Results) gathered from an interview, and prospective monitoring of all falls for the following 12 months using fall calendars and telephone follow-up. The follow-up rate was excellent given the age group, and the method used for prospective monitoring is considered the gold standard. As the authors point out, the epidemiology of falls in this detail has previously not been available for this particular age group. Therefore the paper makes a very valuable contribution to the falls prevention literature.

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

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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)

1. The wording of the Abstract could be clearer regarding the fact that the fall events recorded by the two different methods were in two different time periods (two consecutive years) – for example, the second of the two objectives in the Background section and the Design section of the Methods.

Abstract sections Background and Measures edited as suggested. See also additional clarification at end of main Background section (p.5, para.1)

2. Also in the Abstract, the authors appear to imply that the two sets of fall events were compared directly and any differences attributed to the method of recording falls, rather than a combination of the method used and the fact that the time frame was different. For example, statements such as “recall methods may underestimate numbers of repeated falls but not the proportion of fallers” and that the proportion reported to have fallen was lower using one of the two methods, are potentially misleading given that the fall events being compared were in two different time periods. The wording in the Results and Discussion text concerning the use of the recalled data to predict fallers and number of falls in the year of prospective follow up, is entirely appropriate.
We hope the changes above and additional comments in the Discussion (p.14, last para) sufficiently clarify this unintended misunderstanding – see note after 1st referee’s 1st point.

3. As the authors point out, the extent of the discrepancy between retrospective recall and prospective monitoring for fall events in the same time period has been addressed in only a few studies but this particular study does not directly address this discrepancy. Please can the authors clarify whether the examples of such trials discussed on page 17 (Discussion) record falls in the same or consecutive time periods?

Re-worded with more detail to clarify.

4. The Methods section mentions use of negative binomial regression but it is not quite clear which analyses were conducted using these models, for instance in Tables 1 and 2. Negative binomials regression models can also be used when adjusting for confounding variables. Likelihood ratio tests are mentioned in the Methods but again it is not quite clear in which regression models these tests were used.

More detailed explanation added to Statistical Methods (p.7-8). Negative binomial regression was used to calculated crude relative risks associated with each socio-demographic factor using data collected on the number of falls each person suffered a) according to retrospective recall data and b) from prospective follow-up reports. RRs for follow-up falls associated with falls history were also calculated using the prospective data. Negative binomial regression was also used in multi-variable models to adjust for potential confounding effects of socio-demographic variables, with likelihood ratio testing for the contribution of these variables to adjusted models. With the tables already presenting quite a complexity of data we decided it would be too much to also show all adjusted estimates, particularly as most were only minimally different from the crude rates. Thus these findings are just summarised in the text, and adjusted rates only given where the effects of adjusting for co-variates was note-worthy.

5. Figure 1: Please provide a legend for this graph so that the reader can interpret the line and the bars without referring to the text.

Figure now split into 1a and 1b with caption.
Discretionary Revisions (which the author can choose to ignore)

1. I feel the term “prevalence” is best avoided when reporting falls, as falls are events, not a disease state or attribute.

We have kept the term as a convenient shorthand way of highlighting the distinction between how common the problem of falling is amongst this age-group (proportions of “fallers” and “repeat fallers”) and how common the falls are (falls rates). However, we recognise your advice here is correct and have tried to re-word the Methods, Results and Discussion to take this into account, putting “prevalence” in quotes to suggest this is an unusual use of the term.

2. ProFaNE (Prevention of Falls Network Europe) suggest that the rate of falls should be reported as falls per person year (not per 100 person years) (see Lamb SE et al. JAGS 2005;53:1618:22).

Changed as suggested – falls/person-year now rounded to 1 decimal point.

3. The data used in the survival analysis models in this study were time to a first fall. However, the “time to first fall” is not a particularly good interpretation of a hazard ratio (heading of last column in Table 2 and title of Figure 3). A hazard ratio (or relative hazard) can be thought of in a similar way to a relative risk – the ratio of the estimated risk of a fall at all time points (over the 12 month period) for the two subgroups being compared.

Table 2 now has Mean (and SD) times to first fall added beside the hazard ratio results, grouped together under a new heading Fall free survival. Figure 3 is now Figure 4, still in 2 parts now 4a and 4b, with y-axis labelling added.

4. Table 1 and 2: Does “Each additional year” of age start at 91 years or 94 years?

Tables amended to show that the continuous age variable takes 91 as the reference category.

5. Figures 1 and 2: I suggest that the number of participants should be placed on the top of each bar, and the graph also indicate the total number of participants in this breakdown.
Added to the revised figure replacing figure 2, now figure 3. Not added to Fig.1 now it is split as 2 paired figures.

6. Figure 3: Please can the confounding variables used in the adjusted models be provided in a footnote so that the graph provides stand alone information for the reader.

**Added to what was Figure 3, now Figure 4a&b.**

7. References:
Replace capital letters with lower case in the titles of references 4, 10, and 11. Some journal names are abbreviated and some provided in full.

**Corrected.**

**What next?:** Accept after minor essential 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.

**Declaration of competing interests:**

I declare that I have no competing interests.
Reviewer's report

Title: Falls in advanced old age: recalled falls and prospective follow-up of over-90-year-olds in the Cambridge City over-75s Cohort study

Version: 1 Date: 27 September 2007

Reviewer: Keith Hill

Reviewer's report:

General

This is a well written paper that addresses issues around falls risk for a rapidly growing and largely unexplored group within the at risk population - being those in the 90+ year age group. As such, it provides useful additional data to what is currently known.

The sample are those remaining in a long term sample, drawn initially through representative general practices in the initial wave of recruitment in 1985. The sample and subsequent analyses have pooled all participants together, irrespective of current living arrangements. It would be useful, at least for the overall descriptive data of proportion of fallers and multiple fallers, for this data to be presented separately for those still living at home, compared to those in sheltered care or institutional care (it is!). It is likely that the main regression analyses would most likely be underpowered to be sub-analysed in this way, so this section of analyses is appropriate as it stands.

A strength of the paper is the very high recruitment rate of cohort survivors. However there is very little description of the participants, other than their some basic demographics (age, living arrangements, education etc). It would be useful to have a little more health status profile for the participants, assuming this information might be collected within each wave of this longitudinal main study - for example, number / type of chronic health problems, medication use, ?? measure of function or mobility, and proportion with cognitive impairment.

The analyses provide some useful information about the influence of some of the key socio-demographic characteristics and their association with falls risk. A next step is to have a closer look at the specific intrinsic and extrinsic factors contributing to falls in this cohort, and the consequences of falls in this cohort, to provide a more detailed picture of interventions that might be considered most useful in this increasingly prevalent group (yes but, although in PhD thesis, cannot reference resultant papers not yet published!). This point should be noted in the discussion as to future directions for research in this area (comment added to penultimate paragraph of Discussion).

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

1. page 8, paragraph 2 - it would be useful to know what proportion of the initial
sample (1985) who could have still been in the sample for this study remained in the study, and the proportion who have been lost over time due to death, and the proportion who have dropped out in subsequent waves. The dropout rate, and reasons for dropping out, in particular are important to gain an appreciation of exactly how "representative" the cohort who remain in the study are.

The mention already in the paper of 84% of survivors being in the survey has been elaborated to provide more of the detail requested (Results – characteristics of the sample, p.9 para.1)

2. page 9 - insert greater level of detail of profile of participants (eg number of chronic health problems, medication use, function etc) if this information is available.

More detailed description added in text (Results – characteristics of the sample, p.9 para.2) but not tabulated as tables are already complex.

3. page 14, paragraph 1, line 13 - ?? should Figure 1 be Table 1 here - the information referred to in the associated text regarding differences in odds ratios is not in Figure 1.

Re-worded

4. page 15, paragraph 1, lines 7-10. The discussion about different people falling in the pre and post interview periods would benefit from a little more explanation of possible factors contributing to this, such as new health problems increasing the risk of someone who was previously at lower risk, or that a higher risk person may have had a successful intervention introduced that stopped them from falling.

Added more to Discussion (p.14 last para.)

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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)

1. throughout the paper, references in square brackets need to have a space between the preceding word and the square bracket.

Done

2. page 7, paragraph 1, line 7 - was a definition of "near fall" used. If so, it should be included here.

Added
3. page 8, paragraph 2, line 6 - use provide the mean age to the same decimal places as the sd of age (i.e. to one decimal place).

**Added.**

4. page 9, paragraph 1, line 1 - clumsy wording of text - suggest change "reportedly fell" to "reported falling"

**Not changed to “reported falling” as Reviewer 3 suggested because this figure includes reports both from participants themselves and proxy informants.**

5. page 13, paragraph 2, line 5 - delete the word "significant" - should only be used when talking in a statistical sense.

**Changed to “marked”.**

Discretionary Revisions (which the author can choose to ignore)

1. Consider restructuring the discussion a little. The section "Comparison with previous reports" would seem to fit better early in the discussion, and the section "strengths and limitations" is often near the end of the discussion.

**What next?: Accept after minor essential 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.

**Declaration of competing interests:**

I declare that I have no competing interests.