Reviewer's report

Title: No association between the aluminium content of trabecular bone and bone density, mass or size of the proximal femur in elderly men and women

Version: 2 Date: 13 April 2006

Reviewer: Magnus K Karlsson

Reviewer's report:

General
This is a good scientific paper. The research question and hypothesis is clearly presented, the methods and material are well described, the data seems sound and well controlled, the standard of the paper is high, the conclusions and discussions well balanced, the title and abstract convey what has been found and the writing is acceptable

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

My concern is the close association with presentation of previous work. The same group has published a paper in Osteoporosis International 2005; 16(12): 1982-9 called "The aluminium content of bone increases with age, but is not higher in hip fracture cases with and without dementia compared to controls". This paper includes a larger cohort of patients than in the current paper and for me it seems as if the individuals in this presentation is picked from this larger cohort. In the present paper, it seems as if the authors have added DXA measurements of bone mass and bone size in the presentation, then they repeat the presentation and the conclusions from the previous paper that aluminium content is associated with age but that there is no difference in aluminium content in men and women, individuals with dementia or not or in patients with a hip fracture or osteoarthritis. These conclusions were also drawn in the former patients but as mentioned, evaluated in a larger cohort.

That is, the added new data in this report is the DXA data but for me it is unclear why the authors did not include these in the former report. The association with the relevant endpoint a hip fracture was already evaluated in the former paper, now the authors add association with DXA data. But in addition, they repeat the evaluation to see if there is an association between aluminium and gender, age, dementia or not, hip fracture or osteoarthritis. I think this is an ethical problem when they did the same evaluations as in the larger cohort. Now they repeat close to half of the calculations in the former paper in this new manuscript, probably using the same cohort of patients (with some excluded).

The authors ought to clarify why they divided the data into two papers and the rational for repeating several of the calculations in this smaller cohort, then presented as new data. If still they find it of value to present the same calculations in this smaller cohort, I advice them to be even more clear and give a statement that similar data has been presented before including the same patients (as I can understand) in addition to some more individuals in the former publication.

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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)

The manuscript is sent to me including two tables 1, two tables 2, two figures 1 and two figures 2
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:** No

**Declaration of competing interests:**
'I declare that I have no competing interests'