Reviewer’s report

Title: The Osteoporosis Care Gap in Canada

Version: 1 Date: 17 October 2003

Reviewer: David A Henry

Reviewer’s report:

The authors identify an issue of public health significance – the under treatment of subjects who have had fragility (minimal trauma) fractures.

They have addressed this through a quasi systematic review, confining their analysis to studies of Canadian patients and institutions.

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

My three main areas for comment are: 1) is their review adequate? 2) why did they confine their review to Canadian studies? 3) are their conclusions reasonable?

1. a) A quick Embase search did not identify any other Canadian papers. The authors have published in the field and presumably know it and the other authors fairly well, and they have hand-searched bibliographies. So I think the library search is probably OK.

b) The main conclusions of the paper relate to the reported level of diagnostic activity and treatment, and the extent to which the conclusions can be applied outside of the study populations to other Canadians. In making these assessments the methodological quality of the studies is of some importance. How were subjects selected, is there a possibility of ascertainment error or bias in relation to use of diagnostic tests or treatments? I realize this information will be limited but more needs to be said than the brief comments in the final paragraph of the discussion section. I think the authors should summarise the methodological adequacies of the studies in a table.

2. In the introduction to the paper the author say “inadequate osteoporosis evaluation and treatment has been documented in countries other than Canada”, referring to 8 individual studies carried out in USA, UK, Italy and Israel. They presumably were aware of at least one study done in Canada as they did it (ref 16). This is then used as a justification for reviewing and presenting only the remaining 3 Canadian studies plus their own. I guess they can argue that they are only interested in what happens in their own country, but what a pity they didn’t go the extra yards and do a full systematic review of all available studies (they already have identified 12). The marginal increase in workload would have been modest and the paper would be genuinely more interesting to an international readership.

3. A major thrust of the Discussion is that the diagnosis of osteoporosis is often not made after minimal trauma fractures and this leads to under treatment. The arguments seem to be for better diagnostic screening facilities. I wonder of this is correct? Should fragility fracture itself, rather than ‘osteoporosis’, be the indication for treatment? This is reminiscent of the early arguments about use of cholesterol-lowering drugs in patients after acute coronary syndrome. We used to mess about measuring lipids and now the tendency is to treat them all. Shouldn’t we do the same after fracture?

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

4. A minor point, but the ‘main message’ of the study (Conclusion) is not that fragility fracture is a
major risk factor for osteoporosis. The studies reviewed here were not designed to study that association.

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

**Declaration of competing interests:**

None