Reviewer's report

Title: Social Health Insurance Routine Data Analysis: The Incidence of Shortened Lifetime regarding Endoprosthetic Implants

Version: 2 Date: 29 May 2012

Reviewer: Birgitte Espehaug

Reviewer's report:

This is a well-written paper on an important and timely topic, which I read as being to establish valid arguments that pertain to the initiation of a national arthroplasty register in Germany. Within the limits of the available data the paper is methodologically sound. However there are some issues that need to be clarified.

The aim of the study was stated in the abstract as finding “how high the early revision rate is” based on health insurance data. I suggest that the aim is more clearly formulated also at the end of the introduction part.

The material could be more clearly defined. What were the inclusion criteria? My impression is that the relevant operation codes are the only inclusion criteria (“focus was put on …”), but this might be stated directly. Further it is said that diagnostic codes “were evaluated”. The purpose of the latter might also be directly stated. Were there any exclusion criteria? The discussion part implies that deceased were excluded (third paragraph, methods discussion). I would also like to know how patients could be traced, that is if patients were associated with an id number.

Statistics: Was the M-W test used to test for distributional differences or differences in median values? Furthermore, the statistics part should state which variables were tested. Analyses were based on aggregated data, and I gather that individual patient follow-up data was not available for the authors. However, as all patients were followed for two years and also that relatively few patients would have died during the short follow-up the percentages reported should be similar to those from simple survival analyses. In the sentence giving the significance level, I suggest that “as usual” is deleted.

In the results part I find that many statements rather belong in the methods or discussion part of the paper. There is also a reference given in the results part. If it is not against journal preferences I strongly suggest that only results are described in this section. I also think that statements like “seems to” and “well above” should be exchanged with explicit numbers. In paragraph 5 it is stated that figures 1 and 2 “shows a negative correlation for a level of 100”. I am curious as to what is meant here, and what statistical analyses have been performed to find this number. The next sentence is also unclear although I take it that what is meant is that low volume hospital has higher revision risks. Also the meaning of
paragraph 6 (“As shown in Fig. 1 +2 …”) is unclear.

Regarding the discussion part I find that it covers many important aspects of the study. I do miss an explicit discussion of some of the findings reported, e.g. the percentage of revisions according to type of index surgery (paragraph 3 and 4 in the results section).

Further the authors might consider elaborating on the representativity of the material.

**Level of interest:** An article of outstanding merit and interest in its field

**Quality of written English:** Needs some language corrections before being published

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

**Declaration of competing interests:**

I have no competing interests. (I do however hold a position as biostatistician at the Norwegian Arthroplasty Register.).