Reviewer's report

Title: Mortality after Primary Shoulder Arthroplasty: An analysis of 4,019 patients from 1976-2008

Version: 1 Date: 21 June 2011

Reviewer: Jos Jam JAM Van Raay

Reviewer's report:

It concerns an article whose findings are important to those with closely related research interests. Especially the finding that a high BMI is protective for 90 day mortality after shoulder replacement. The other findings are well known and does not add important information. Major revision is necessary before a decision on acceptance or rejection can be made.

The question posed by the authors is well defined. Why did they take a 90-day postoperative period into account and for example not one year down the line? One can imagine that after cardiopulmonal or neurological complications during the period shortly after operation, patients decease in the first postoperative year because of these complications. This could increase the mortality rate or do the authors hypothesize that from 90 days after operation, mortality rate does not increase further. This has to be cleared up. It would be interesting to determine the 1 year mortality.

Introduction: First sentence: Shoulder arthroplasty is an…. refractory shoulder pain. Does the authors mean “end stage of degenerative arthritis of the shoulder and other causes” that cause refractory shoulder pain? At the end of introduction: 1: determine risk factors and 2 (instead of 3) influence type of procedure

Methods: They are not very appropriate and well described. Predictors of interest: The authors anticipated that the numbers of death would be too small to analyze HHS and TSA separately. This contradicts with the second aim of the study i.e assessing whether mortality differed by the type of procedure (HHS vs TSA) Whether TSA or HHS is a predictor of interest is also not mentioned in results and conclusions in the abstract. Under diagnosis, the category tumor is missing. The Deyo-Charlson index has to be cleared up with literature/reference. What is the mortality of an matched (age, gender, comorbidity, Deyo-Charlson, ASA class) population in comparison to the study group? It were all elective cases except trauma? Were fracture cases operated immediately or in a later phase?

Statistical analysis: The authors want to assess mortality risk factor in patients undergoing HHS and TSA together. Also this contradicts again the second aim of the study whether mortality differs by the type of the procedure. Does they want to assess mortality risk factors or mortality? This is unclear and conflicting in the introduction and methods text

Results 1: The data are sound but not complete. More information is needed
concerning clinical and sociodemographic characteristics. Age upper and lower limit and median? Tumor: primary bone tumour or osseous metastasis? Rotator cuff disease: Massive rotator cuff rupture with and without cuff arthropathy? The manuscript adheres to the relevant standards for reporting and data deposition. Results 2: 90 day mortality: mostly during the First 24 h period postoperatively or within several days after operation or later for example after 2 or 3 months? Cause of death? Cardiopulmonal or neurologic event, thrombo embolic? Or directly related to the operative procedure? These data are missing. What comorbidity is associated with an increased mortality risk?

Discussion: Is balanced and supported by data. Limitations are stated i.e. BMI and ASA data are not available for the whole period. Additional adjustment could be performed without influencing the adjusted model or conclusion. Especially with such a low mortality incidence (28 cases) it is possible to study the cases individually especially when they can be isolated from the prospectively collected information from Total Joint Registry. The authors acknowledge the published work upon they build. There are no unpublished data reported. The title does not comprise what has been found, this could be mentioned in a subtitle as “predictors of mortality”. The finding of a low mortality in higher BMI patients is novel and unexpected. A high BMI is protective. What is the explanation of this finding? This does not mean that a low BMI carries a greater mortality risk after shoulder replacement. Reference 15 and 16 deal with BMI in relation to groin hernia and cardiac surgery concerning postoperative mortality. There is no reference concerning BMI and postoperative mortality in relation to joint replacement surgery in general and shoulder replacement surgery specifically. How could a large BMI play a protective role in joint replacement surgery?

**Level of interest:** An article of importance in its field

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

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests