Reviewer's report

Title: Meta-analysis of prophylactic corticosteroid use in post-ERCP pancreatitis

Version: 1 Date: 11 September 2007

Reviewer: Takeshi Tsujino

Reviewer's report:

General
The authors conduct a meta-analysis of prophylactic administration of corticosteroid for the prevention of post-ERCP pancreatitis. I have several major and minor concerns that may improve the manuscript.

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

1. The results of the present study do not seem very impressive. None of the well-designed RCTs have shown that corticosteroid dose not prevent pancreatitis after ERCP (eg, De Palma GD et al. Am J Gastroenterol 1999, Sherman S et al. Gastrointest Endosc 2003), while only one small-scale RCT (Kwanngern K et al. J Med Assoc Thai 2005) found its efficacy; on the contrary, gabexate, of which the same leading author has recently reported a meta-analysis, was found to be effective for the prophylaxis of post-ERCP pancreatitis in the well-designed multicenter RCT (Cavallini et al. N Engl J Med 1996; 335:919-23). I do understand that a meta-analysis definitely provides solid evidence, but I wonder why the authors conducted this study.

2. In addition, the authors state in Background that the contradictory results of corticosteroid in the prophylaxis of post-ERCP pancreatitis can only be resolved from large prospective RCTs. The US multicenter RCT by Sherman et al. (Sherman S et al. Gastrointest Endosc 2003) is one of the largest studies (n = 1,115) on the pharmacological prevention of post-ERCP pancreatitis. Do they consider that the power of this RCT is inadequate to draw conclusions?

3. I am afraid, but I have to say that Discussion needs to be improved further.
   1) In the 1st paragraph in Discussion, the authors have described repeatedly what was written in Background.
   2) In the 2nd paragraph, they discuss on the retrospective study by Weiner et al. and the small RCT by Kwanngern et al. Unfortunately, I consider that these two studies are less important, as compared to well-designed RCTs (eg, Sherman S et al. Gastrointest Endosc 2003). I’d rather the authors discussed these RCTs.
   3) Recently, RCTs have had a tendency to be targeted to the patients at high-risk for pancreatitis, the very group of patients who could be benefit from pharmacological prevention. This meta-analysis did not address the stratified data, which seems the major limitation of this study.
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. Methods; The authors excluded patients with chronic pancreatitis and pancreatic cancer. However, some RCTs included both or either of these groups of patients (eg, De Palma GD et al. Am J Gastroenterol 1999, Sherman S et al. Gastrointest Endosc 2003).

2. Results; In order to make the readers understand easily, the authors would express the incidence (percentage) of post-ERCP pancreatitis in the two groups.

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 of limited interest

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.