Reviewer's report

Title: Outcome comparison between percutaneous cholecystostomy and cholecystectomy: a 10-year population-based analysis

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Reviewer: Fabio Cesare Campanile

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The manuscript reports a very large population-based comparison between percutaneous cholecystostomy and laparoscopic cholecystectomy. The topic is very interesting and it became very relevant after the Tokyo guidelines, first in 2007 and then in 2013, considered the use of percutaneous cholecystectomy mandatory in the "severe" and strongly suggested even in most moderate grade cholecystitis. The controversy about the role of percutaneous cholecystostomy is fed by the absence of reliable data about its outcomes and many authors maintain that there is no evidence to support the recommendation of PC rather than cholecystectomy in elderly or critically ill patients with acute cholecystitis. This study can give a relevant contribution to our understanding of the role that percutaneous cholecystostomy may play in the management of the acute cholecystitis.

The manuscript is well written. However, some remarks are necessary:

1. The introduction correctly delineates what is known about the topic. However, the role that the Tokyo guidelines attributed to the percutaneous cholecystostomy (not only as an alternative procedure in critically ill patients but even as a bridge to surgery in the moderate grade) and the related controversy in the literature should be described.

2. The authors describe the need to "conduct in-depth population-based research...for Asian population"; however, even in the Western population only one large registry study compares percutaneous cholecystostomy and cholecystectomy.

3. The research question could be better explained at the end of the introduction. Here, the aim of the study appears to include the comparison of the two procedures outcomes and not only its extension to the Asian population.

4. In the methods section (Data definition paragraph) it is not clear what is the meaning of the expression "first relevant operation". Do the authors mean the actual first operation received by the patient or there is the possibility that the first PC or CCS was considered "not relevant"? And what about the sentence "When patients had undergone PC and CCS as first
relevant operation"? Of course, if they received one of them could not had the other at the same time.

5. In the "Measurement Outcomes" I find: "Thirty-day mortality was used to refer to patients who died within one month after undergoing AC or CCS. This designation refers to patients who died during hospitalization. In-hospital mortality was used to refer to patients undergoing PC or CCS who died during hospitalization." I believe that only the "in-hospital mortality" refers only to patients who died during hospitalization. In other words, "In-hospital mortality" is the portion of "30-days mortality" occurred before discharge. This could be a typo.

6. A large part of the "results" section actually repeats the data already shown in the tables. The entire section could be more readable if a proper reference to the appropriate table is done in the text, avoiding to repeat all data.

7. This is my main concern: the controversy about the role of the percutaneous gallbladder drainage is based on the opinion, expressed by some of the authors (Winbladh et al published the nice review on this particular aspect mentioned at reference #7) that the mortality is far higher after PC than CCS, even for critically ill patients. This aspect is difficult to study because the general conditions are far worse in the average PC than CCS patient and a proper statistical comparison is impossible. In this study the authors have the data about the severity of the illness of their patients and they can stratify their population by age, causes of procedure and CCI score. However, they use this stratification to compare costs and LOS (see table 4) but not morbidity and mortality. In Table 2 only the mean values for the entire, unstratified, population are given. This very large study could greatly improve its value if the authors could provide us with data comparing the morbidity and mortality of patients divided in age, causes of procedure and CCI score groups: it would be of the utmost importance to know if the morbidity and mortality of patients with acute cholecystitis and CCI 3 or higher (or those older than 70) is actually higher in those who underwent PC Vs. CCS or vice-versa. I think that the addition of the subsets analysis could justify a revision of the paper in order to further improve its interest. As a matter of fact if we are able to prove that mortality is higher in PC or CCS, in the selected group of critically ill patients we could determine if the Tokyo guidelines recommendations are adequate and current or should be revised.

8. In the discussion section the authors analyze the temporal trends of PC and CCS in Taiwan and found an increase of the PC administration by 18.34% per year from 2003 to 2012. They consider that the aging of the population is at the basis of such an increase. Do the author think that the publication of the Tokyo guidelines in 2007 have also affected this trend?

9. Among the limitations the authors may want to include the fact that their "readmission due to complications" analysis is limited to a 1 month period while it has been shown that acute or emergency cholecystectomy may be needed far beyond that limit.
Are the methods appropriate and well described?
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Yes

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