Reviewer's report

Title: Impact of treatment policies on patient outcomes and resource utilization in acute cholecystitis in Japanese hospitals

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Reviewer: Erik Nilsson

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General

Review of Sekimoto M et al "Impact of treatment policies...."

What treatment was given to patients with acute cholecystitis in nine Japanese hospitals during 26 months - acute versus delayed surgery with open or laparoscopic technique, and to what extent did the institutional strategy affect patient outcome? The authors reached the conclusion that timing of surgery and discharge of patients were determined by the institutional policy in Japan rather than by the clinical course of the patient and the surgical technique used. Clear questions and clear answers.

However, in the Abstract and in the Discussion, the authors speculate about what would have happened if laparoscopic cholecystectomy had been more rapidly adopted. They might just as well have discussed the scenario that fast track surgery5,11 had been introduced or minilaparotomy cholecystectomy9 put into practice. The authors quote the National Institute of Health Consensus Development Conference Statement on Gallstones and Laparoscopic Cholecystectomy Am J Surg 1993;165:390-98, written at a time when fast track surgery was not discussed and little information was available concerning minilaparotomy cholecystectomy. And they do not mention two later systematic reviews questioning the rapid spread of laparoscopic cholecystectomy1,4, nor the increased risk of bile duct injuries and leaks in laparoscopic compared to open cholecystectomy. Such infrequent injuries are very important although they can not be traced in a study of the present size7. Timing of surgery for acute cholecystitis has recently been updated by Papi8

Introduction
Page 3, 5 lines from bottom: the authors state that two recent reports advocate the use of laparoscopic cholecystectomy for acute cholecystitis. Both these articles were published in 1991!

Subjects
I am a bit concerned that the authors excluded not only patients with incomplete data and pancreatitis (according to UK guidelines cholecystectomy should be performed within 2 to 4 weeks after onset of disease in cases of mild pancreatitis2) but also other groups such as patients who develop acute cholecystitis during hospitalization. I understand that preoperative hospital stay is difficult to evaluate for these patients. However, one should also realize that the more exclusions you have, the less valid are your conclusions for all patients with diagnosis discussed (acute cholecystitis).

Results
The LOS reduction associated with the use of laparoscopic cholecystectomy must be considered against type of open surgery utilised and the over-all LOS which in the eyes of the reviewer is excessive. The authors noticed that the incidence of intra-operative complication was significantly higher after laparoscopic as compared to open surgery 9.7% vs 0%. This is an important finding –
what is the explanation? And why, then, was the over-all intra-operative complication rate highest among hospitals with the lowest use of laparoscopic technique?

Discussion
I would question the statement that “available evidence predominantly demonstrates the advantage of laparoscopic cholecystectomy among patients with acute cholecystitis”. Two randomised controlled trials comparing open and laparoscopic cholecystectomy have been published3,6. In the study by Kiviluoto et al6, quoted by the authors, the laparoscopic surgeons were obviously more experienced than open surgeons, which reduces the external validity of this trial. In the other study3 outcome difference between the two alternatives were minor (median stay two nights for both open and laparoscopic surgery). None of these studies tell how representative the trial patients were for all patients with acute cholecystitis in the populations concerned. The authors write that “the impact of LC on the reduction of LOS was surprisingly small”. That depends on what the authors expected. In a recent Greek study the use of laparoscopic cholecystectomy was questioned after the introduction of minilaparotomy cholecystectomy10.

References

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
The authors should update their references, consider the external validity of randomised controlled and other trials they quote, think of small-incision open surgery (minilaparotomy cholecystectomy) as an alternative to laparoscopic cholecystectomy, and omit speculations beyond the data presented. The authors state in agreement with relevant literature that laparoscopic alternatives carry higher operating room costs. Hence, small incision surgery seems a very appropriate alternative, which in contrast to laparoscopic cholecystectomy can be applied to all patients.
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
Page 9: Line 5 and 6 “…LOS was significantly shorter”
Figure 1 Adjusted length of stay – spelt incorrectly
Table 1 It seems odd to present “Adjusted cholecystectomies” as in fact cholecystectomy rate is an independent variable to be investigated. (However, I do understand the authors meaning in presenting these figures).

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 importance in its field

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

Statistical review: No

Declaration of competing interests:
I declare that I have no competing interests