ANSWERS to REVIEWERS' COMMENTS

June 29, 2013

Dear Editor,

Please find enclosed the edited manuscript in Word format (file name: 3824-edited_review.doc).

Title: *Comparison Between Open And Laparoscopic Reversal Of Hartmann's Procedure For Diverticulitis.*

Author: Nicola de'Angelis, Francesco Brunetti, Riccardo Memeo, Jose Batista da Costa, Anne-Sophie Schneck, Maria Clotilde Carra, Daniel Azoulay

Name of Journal: *World Journal of Gastrointestinal Surgery*

ESPS Manuscript NO: 3824

The manuscript has been improved according to the suggestions of reviewers:

1 Format has been updated

2 Revision has been made according to the suggestions of the reviewer

(1) ***Reviewed by 02458699***

(*Our answers to the Reviewer's comments are in red italics*).

This retrospective cohort study compares the open to laparoscopic reversal of hartmann's procedure in a homogenous population with complicated diverticulitis. It is the first to isolate diverticulitis as indication. It is well conducted and relevant. However, there are some major concerns.

- Abstract is overlong and needs cutting to the central points.
  The structured abstract has been shortened and edited according to the writing requirements of brief articles (no less than 246 words).
- Methods. How did you identify the patients? By hospital records, database?
  Data on the study population were retrieved from a prospectively maintained colorectal surgery database. This has been written in the abstract and specified in the methods.
- You state that you only operated patients with complicated diverticulitis. Were phlegmones and abscess completely excluded from the analysis?
  Yes. Only patients with Hinchey score III and IV were included in the analyses.
- You need to report which Hinchey grades you included and which you excluded in your synthesis in the method section.
  This has been added in the methods section.
- How where the patients followed up short-term and long-term? Was there any systematic follow-up after a certain period of hospital-release or where they only observed during hospitalization and then after 1 year? If only observed in hospital, for how many days was minimum for postoperative hospitalization?
  The patients were checked during their entire hospital stay (different on a patient by patient basis according to their clinics and biologics). Patients were discharged at least 1 day after time to flatus. After hospital discharge, all patients entered in a follow-up program, including 1-month, 3-months, 6-months, 12-months visits. After the first post-operative year, an annual visit is usually performed. In this study, all patients had at least 3 years of follow up, and we retrieved data from the 1-month (short-term), 12-months and 3-years (long-term) follow up visits.

- Results. Did you include all patients who had a hartmann's procedure and reversal for diverticulitis? Any exclusions and why?
  Yes. Our study population includes only patients who underwent HP for diverticulitis, as confirmed by histopathological examinations of the surgical specimens, and then operated by OHR or LHR between 2000 and 2010. As specified in the methods, only Hinchey scores III and IV were included in the analysis. All other indications for HP (e.g., carcinoma, trauma, and ischemia) were excluded. We chose these selection criteria in order to have a homogeneous sample of patients operated for the same indication, i.e. diverticulitis.
- Was the study population consecutive? This is stated in discussion but should be stated in results.
  Yes. The study sample comprised 46 consecutive patients operated for diverticulitis. This is stated in the methods.
- In table 1 OHR is has 17 and LHR has 26 hinchey III+IV. The total numbers of patients are 18 and 28. Who are the remaining patients? All patients must be accounted for.
  Thanks to the reviewer for noting these typos. Numbers have been corrected in the table 1.
- Overall, a well written study with some clinical importance. However, the selection bias are currently to massive

(2) ***Reviewed by 00181211***

(*Our answers to the Reviewer's comments are in red italics*).

I would like to congratulate the authors for their excellent results. Although it is a retrospective study I consider that it is well designed and explained. Furthermore, the discussion is well written and the conclusions are clear, so I just will enclose minor comments.

MINOR COMMENTS:

- Introduction - Last paragraph, line 7: it would be better to reference the studies when the author writes ".. the few studies that have examined..."
  The references have been added at the end of the sentences. Thanks for noting it.
- Results: - Last paragraph, line 2: I would not say "peristomal colostomy hernias" as the Hartmann procedure has already been reversed and the patient has no colostomy. The author should change the way he refers to the hernia of the incision of the previous colostomy. If he does, he should change the term also in the abstract and the discussion.
  You are perfectly right. We changed the term with incisional hernia of the previous colostomy allover the manuscript and table.
- Discussion: - Third paragraph, line 4-5: "However, the laparoscopic approach does not seem to have increased the number of patients who undergo HR, which might have been expected". It is not clear to me why the authors expected that laparoscopic approach would increase the number of patients undergoing HR.
  We meant to say that the introduction of laparoscopy to perform HR, although less invasive, has not yet influenced the % of patients that can undergo HR. It remains that only a 1/3 of patients who underwent HP can beneficiate of LHR. Further studies and especially long-term studies on LHR are needed to assess this aspect. For clarity, the sentence has been reviewed in the text.
- Sixth paragraph, line 1: "The laparoscopic HR" should be changed to LHR, as in the rest of the text.
  This has been changed accordingly.
- Table 1: - I suggest deleting the cause of perforation as the study only includes patients with diverticulitis, so no other options are possible.
  This has been changed accordingly.

(3) ***Reviewed by 00070933***

(*Our answers to the Reviewer's comments are in red italics*).

The authors present a comparative study between open and laparoscopic reversal of Hartmann procedure. The results are very interesting and in accordance with previous results, underlining the minimal invasiveness of laparoscopic surgery and the same (or better) security profile. Overall the paper is well written and discussed, but I think some improvement can yet be made:

- I would like to see the p-values in Tables (although non-significant, is point value is not insignificant).

  The p values have been added in the tables.
- Just some minor typos... Table 2 - Stapled, not Stappled. No31 is 0% on OHR. Table 3 - Length of hospital stay, not Hospitality.

  Thank you, we corrected the typos as suggested.
- The authors could discuss the technical difficulties of the laparoscopic procedure, mainly concerning adhesiolisis and difficulties in dissection. What was the empirical feeling of the authors regarding adhesiolisis and time after HP?

  Although there is not much evidence in the literature on the topic, the main challenging aspects of performing HR by laparoscopic approach are related to the laparoscopic technique per se (e.g. bidimansional vision, poor depth perception, use of rigid instruments, limited range of possible movements…) and to the adhesiolysis, which however is mainly influence by the underlying disease with a high variability patient by patient. The technical difficulties of LHR are discussed in the manuscript, in relation to our experience and results presented in the study.

  Moreover, in the present study, the HR was performed in average 4.5 months from the HP, which is within the ranges previously reported. While performing the HR, either OHR or LHR, we did not find extreme difficulties in adhesiolysis and dissection, and no conversion from LHR to OHR was needed. It is noteworthy that we had a population of patients with only diverticulitis, and thus we did not have patients with oncologic pathologies, factors that may drastically influence the clinical status of the patient.
- In the discussion, the authors state that the time interval between HP and HR did not influence morbidity or mortality in either group, but nothing is stated about that in the results section...

  This is written shortly in the results, where we say: "The only variable that was significantly associated with post-operative complications was the length of hospital stay". The discussion and interpretation of this result is in the discussion section.
- What is the author feeling about the differences in hospital stays? Why do patients have such long hospital stays, mainly in OHR?

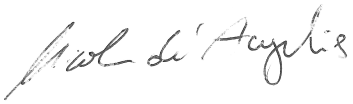
  The hospital stay was longer in the OHR group, probably due to the more invasivity of the procedure, which is usually associated to a longer time to flatus and resumption to normal diet. As colorectal surgeons experienced in laparoscopy, we expected this difference in favor to LHR.
- Regarding procedure costs, it is not explained how these values were obtained. Is this a theoretical model? Is it the average actual invoiced costs for each group? How were the cost's information retrieved?

  This is not a theoretical model, but the actual mean invoiced costs per patient in the French hospital system "Assistance Publique Hopitaux de Paris". This has also been specified in the text.

3 References and typesetting were corrected

Thank you again for publishing our manuscript in the *World Journal of Gastrointestinal Surgery*.

Sincerely yours,



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