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Please find enclosed the edited manuscript in Word format (file name: 20830-revised.doc).

Title: Natural history of uncomplicated sigmoid diverticulitis

Authors: Nicolas C. Buchs, Neil J. Mortensen, Frederic Ris, Philippe Morel, Pascal Gervaz

Name of Journal: *World Journal of Gastrointestinal Surgery*

ESPS Manuscript NO: 20830

The manuscript has been improved according to the suggestions of reviewers (all the changes are high-lightened in yellow):

1) Format has been updated

2) Revision has been made according to the suggestions of the editor and reviewers

Reviewer 1:

This paper concerning the natural history of sigmoid diverticulitis is named "a review" but indeed has very few characteristics of it. Should it be considered a comprehensive review? The data to justify this definition are scanty. From the beginning, this seems to be an interesting paper. However, continuing to read it, it appears mainly a reiteration of the study already published, and cited in the references, by the same authors in 2013 on British Journal of Surgery. The same results are reported, even the registration number of the trial. Nothing is said about the criteria of the review and, apart definition of diverticulitis, no methods are reported. Statistical analysis is lacking and the title is not justified on these basis. At the end, it is surprising that the data cited to describe the natural history of sigmoid diverticulitis, are mainly coming from the previous cited study. The definition of diverticulitis is in agreement with the criteria coming from the previous cited cohort study, which should indeed be evaluated and possibly compared with other studies in the literature. In my opinion, this paper has two main reasons to be rejected It is

not a review It is mainly a report and discussion of results already published 2 years ago by the same authors.

According to the reviewer's comments, we have changed the format of the paper to a comprehensive review. However, we don't agree with the reviewer's second comment. This is not a reiteration. To avoid any criticism, changes have been done, but the core of the paper remains the initial one. Most of the current literature was cited (see Table 1). Risk factors for recurrent diverticulitis were mentioned (while in our study only CRP was found to be a significant factor)…

Reviewer 2:
I read with great interest the article by Buchs et al, entitled Natural history of uncomplicated sigmoid diverticulitis. The aim of their study is important, since most of Diverticulitis reports include the complicated forms of the disease. One of major concerns in patients with diverticulitis is the risk of recurrence. The authors have stated that the recurrence rate ranges from 13% to 47%, although the risk of developing a complicated second episode is less than 2% to 5%. On the Introduction section, the authors mentioned that "it is recommended to eventually perform a full colonoscopy, in order to rule out cancer or inflammatory bowel disease". Since this is mentioned in the context of the diverticulitis approach, one recommendation would be to make clear that such colonoscopy should be done after the acute attack, as mentioned in page 6: "All patients were encouraged to undergo routine colonoscopy eight to twelve weeks after the first attack, in order to rule out malignancy".

We agree with the reviewer. It was clarified in the text.

On the section of Definitions, the authors mentioned a definition of simple diverticulitis, which I understand it was already published (Ref. 21). However, the fourth criterion, "Endoscopic: to document the presence of diverticula and rule out another associated condition" appears not to be a diagnostic criterion but a diagnostic strategy instead.

In fact, endoscopy is both diagnostic and strategic. It was clarified in the text.

On page 8, the authors address the interesting "apparent" risk factor for recurrence constituted by young age. And indeed, there is currently no evidence that younger patients should be treated different than older patients. But, is a different treatment strategy recommended by others based on the possibility of a more aggressive course? Or is it because of the increased risk of recurrence based on a longer life expectancy?

We agree: currently there is little evidence that young patients should be managed differently. The different treatment was mainly based on the longer life expectancy.

On pages 8 and 9, the authors mentioned the recently proposal of a CRP >50 to be included in the diagnostic criteria for sigmoid diverticulitis. Although this was already published (Ref. 21), was the cutoff point adequately validated? It would be adequate to mention the accuracy of the cutoff value. The authors are encouraged to add the unit measure whenever a quantity of CRP is mentioned.

This information was added, as the unit measure. The cutoff is not validated but is a proposal for a modern definition of acute diverticulitis. This information was added in the discussion.

On page 9, the authors mentioned that the natural history of sigmoid diverticulitis is benign in >95% of cases, which seems pretty obvious, was this number calculated upon the studied on Table 1? If so, how was it calculated?

No it was calculated according to our previous publications. It was changed according to the reviewer's comment.

Reviewer 3
Thank you for the opportunity to review the manuscript for the natural history of simple sigmoid diverticulitis. A few comments/questions 1. In general, this is well written, straight-forward and well organized.

We would like to thank the reviewer for the nice comment.

2. Would change the table 1 to Studies evaluating THE natural history of acute diverticulitis.

According to the reviewer's suggestion, it was changed.

3. Figure 1--would need to have a legend that suggests what you classify/define as simple and complex.

Changes were done accordingly.

4. The terms in Table 1 (mono centric, bicentric) are not widely used and should likely be changed.

Corrections were made.

5. What about the role of anti-inflammatory (along the spectrum of IBD) agents (aminosalicylates) for the treatment of simple or nothing alone?

This information was added. Of note, although the aim of this study was not to review the treatment of diverticulitis, on page 6, there is a mention about the current treatment for simple attack (antibiotics or nothing).

6. There are updated guidelines (2014 ASCRS) that discuss the recommendations for things like number of attacks and age and would suggest to highlight those instead of older recommendations.

We agree with the reviewer's comment. The most recent guidelines were added.

3) References and typesetting were corrected

4) Language was polished by Neil Mortensen (native English speaker).

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

Sincerely yours,

Dr Nicolas Buchs, MD PD