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ESPS Peer-review Report

Name of Journal: World Journal of Gastroenterology

ESPS Manuscript NO: 6288

Title: The effect of bile acids on COX2 expression in rat model of duodeno-esophageal

Reviewer code: 02594127

Science editor: Wen, Ling-Ling

Date sent for review: 2013-10-12 16:28

Date reviewed: 2013-11-05 16:40

CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
<input type="checkbox"/> Grade A (Excellent)	<input type="checkbox"/> Grade A: Priority Publishing	Google Search:	<input checked="" type="checkbox"/> Accept
<input type="checkbox"/> Grade B (Very good)	<input checked="" type="checkbox"/> Grade B: minor language polishing	<input type="checkbox"/> Existed	<input type="checkbox"/> High priority for publication
<input checked="" type="checkbox"/> Grade C (Good)	<input type="checkbox"/> Grade C: a great deal of language polishing	<input type="checkbox"/> No records	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade D (Fair)	<input type="checkbox"/> Grade D: rejected	<input type="checkbox"/> Existed	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E (Poor)		<input type="checkbox"/> No records	<input type="checkbox"/> Major revision

COMMENTS TO AUTHORS

I think that this manuscript is interesting. The findings are not totally new, except for the Cox2. There is a previous publication in French about the same lesions after esophago-jjeunostomy in rats (Chir Pediatr. 1990;31(4-5):245-50). But the article is nice. Well written and well presented. Figure 2 is not very good.



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Name of Journal: World Journal of Gastroenterology

ESPS Manuscript NO: 6288

Title: The effect of bile acids on COX2 expression in rat model of duodenoesophageal

Reviewer code: 00003826

Science editor: Wen, Ling-Ling

Date sent for review: 2013-10-12 16:28

Date reviewed: 2013-12-07 10:02

CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
<input type="checkbox"/> Grade A (Excellent)	<input type="checkbox"/> Grade A: Priority Publishing	Google Search:	<input type="checkbox"/> Accept
<input type="checkbox"/> Grade B (Very good)	<input checked="" type="checkbox"/> Grade B: minor language polishing	<input type="checkbox"/> Existed	<input type="checkbox"/> High priority for publication
<input type="checkbox"/> Grade C (Good)	<input type="checkbox"/> Grade C: a great deal of language polishing	<input type="checkbox"/> No records	<input checked="" type="checkbox"/> Rejection
<input type="checkbox"/> Grade D (Fair)	<input type="checkbox"/> Grade D: rejected	BPG Search:	<input checked="" type="checkbox"/> Minor revision
<input checked="" type="checkbox"/> Grade E (Poor)		<input type="checkbox"/> Existed	<input type="checkbox"/> Major revision
		<input type="checkbox"/> No records	

COMMENTS TO AUTHORS

Reflux of duodenal contents has been linked to esophagitis. Previous studies have suggested that bile acid induced COX2 expression may play a role in the pathogenesis of esophagitis and Barrett's esophagus. In this manuscript, the authors performed EDA in rats and confirmed previous findings. My major concerns regarding this manuscript are: 1) lack of novelty; 2) descriptive, lacks detailed analyses of molecular mechanisms. Additional concerns include following: 1. Page 3, line 1, needs a reference for "...killed as described previously." 2. Elucidation of figures should be in the text content, not paragraph title. 3. Figure 2 needs to show esophagus from both EDA and control animals. 4. PCNA labeling needs to be presented, and what is n=? for 75+/-5%, etc. 5. Both bile acid and PGE2 concentration were from what number of animals, presented as mean+/- SD? 6. Figure 4, COX2 mRNA and Westernblot are needed to quantitate its expression changes. 7. Finally, in page 7 paragraph 3, the authors stated "In this study, we demonstrate that bile acids ..." This reviewer does not think there is enough evidence provided in the manuscript.



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ESPS Peer-review Report

Name of Journal: World Journal of Gastroenterology

ESPS Manuscript NO: 6288

Title: The effect of bile acids on COX2 expression in rat model of duodeno-esophageal

Reviewer code: 02504712

Science editor: Wen, Ling-Ling

Date sent for review: 2013-10-12 16:28

Date reviewed: 2013-12-24 19:58

CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
<input type="checkbox"/> Grade A (Excellent)	<input type="checkbox"/> Grade A: Priority Publishing	Google Search:	<input type="checkbox"/> Accept
<input type="checkbox"/> Grade B (Very good)	<input type="checkbox"/> Grade B: minor language polishing	<input type="checkbox"/> Existed	<input type="checkbox"/> High priority for publication
<input type="checkbox"/> Grade C (Good)	<input type="checkbox"/> Grade C: a great deal of language polishing	<input type="checkbox"/> No records	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade D (Fair)	<input type="checkbox"/> Grade D: rejected	BPG Search:	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E (Poor)		<input type="checkbox"/> Existed	<input type="checkbox"/> Major revision
		<input type="checkbox"/> No records	

COMMENTS TO AUTHORS

This is an excellent experimental study which probably adds to the existing literature. While I agree with most of the points put forward by the study, I think the conclusions are a little too strong and need to be toned down a little bit. In particular, the comment in the results suggesting that the histological features may depend on the volume of reflux contents etc. The volume of reflux contents was not measured in this study and such a comment in the results is totally inappropriate. There is a similar comment in the discussion and the authors may put this forward as a suggestion but must indicate that this hypothesis remains to be proven. The final conclusion of the manuscript is also very strong. The comment on bile acids inducing the growth of oesophageal cancer needs to be proven and this study suggests this mechanism but other factors may be involved either independently or in combination with bile acids. This experiment has proven that COX2 is unregulated in the oesophagus. However this does not prove the suggested mechanism. It may be implicated either independently or with other factors. There is new evidence in this study, but it is not compelling. With regards to the benefit from COX2 inhibitors, this should be a completely different experiment and should not form part of the conclusion. The last two sentences in the conclusion are unnecessary. I like this manuscript and I would recommend that it is published provided that the manuscript is revised as per comments above. I am keen that it appears as a strong publication which reflects what the experiment shows without exceeding its remit.