

ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology

ESPS manuscript NO: 18248

Title: Proposal of an ultrasonographic classification for hepatic alveolar echinococcosis

Reviewer's code: 03314022

Reviewer's country: Italy

Science editor: Jing Yu

Date sent for review: 2015-04-15 15:41

Date reviewed: 2015-05-26 03:32

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	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"/> The same title	<input type="checkbox"/> High priority for publication
<input checked="" type="checkbox"/> Grade C: Good		<input type="checkbox"/> Duplicate publication	
<input type="checkbox"/> Grade D: Fair	<input type="checkbox"/> Grade C: A great deal of language polishing	<input type="checkbox"/> Plagiarism	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade E: Poor	<input type="checkbox"/> Grade D: Rejected	<input checked="" type="checkbox"/> No	<input checked="" type="checkbox"/> Minor revision
		BPG Search:	<input type="checkbox"/> Major revision
		<input type="checkbox"/> The same title	
		<input type="checkbox"/> Duplicate publication	
		<input type="checkbox"/> Plagiarism	
		<input checked="" type="checkbox"/> No	

COMMENTS TO AUTHORS

The manuscript by Kratzer and colleagues is particularly welcomed in the field of echinococcosis and imaging because, thanks to the long standing experience on AE of this group, it begins to fill a long stading gap in the approach to AE, providing a potentially very useful tool applicable to the most widespread imaging method (ultrasound) available especially outside the hospital, in the field setting. Overall the manuscript reads well, although some sentences need attention (see attached file). As outlined more specifically in the comments provided in the attached commented manuscript, the major points needing attention are: 1) the structure of the manuscript is not clear in some points 2) the specific aims of the classifiaction are not well stated at the beginning of the manuscript and the reader needs to wait until the discussion for this 3) from a methodological point of view, it is unclear wether the Authors classify lesions of patients (and why one or another) 4) very importantly, the entire work would be much more complete and of scientific impact if the proposed classification would be tested by evaluating the inter- and intra-observer agreement after different operators have blindly classified US images based on this classification. Even better with the inclusion of operators outside the group following these cases and therefore knowing the cases well, thus reducing the



BAISHIDENG PUBLISHING GROUP INC

8226 Regency Drive, Pleasanton, CA 94588, USA

Telephone: +1-925-223-8242

Fax: +1-925-223-8243

E-mail: bpgoffice@wjgnet.com

<http://www.wjgnet.com>

possibility of real blinding. I consider this a major point for revision and I would strongly encourage this type of analysis before the publication of the work. If not possible, the Authors should address this point in the discussion

ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology

ESPS manuscript NO: 18248

Title: Proposal of an ultrasonographic classification for hepatic alveolar echinococcosis

Reviewer's code: 03338507

Reviewer's country: Germany

Science editor: Jing Yu

Date sent for review: 2015-04-15 15:41

Date reviewed: 2015-05-26 06:56

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	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"/> The same title	<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"/> Duplicate publication	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade D: Fair	<input type="checkbox"/> Grade D: Rejected	<input checked="" type="checkbox"/> Plagiarism	<input checked="" type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E: Poor		<input type="checkbox"/> No	<input type="checkbox"/> Major revision
		BPG Search:	
		<input type="checkbox"/> The same title	
		<input type="checkbox"/> Duplicate publication	
		<input type="checkbox"/> Plagiarism	
		<input checked="" type="checkbox"/> No	

COMMENTS TO AUTHORS

General comments: (1) The importance of the research and the significance of the research findings: As a rare condition, diagnosing AE should gain more attention in the daily clinical practice. These findings have the potential of being significant, when the reliability and validity of the study and the manuscript are improved. (2) Novelty of the research: Retrospective study of US imaging is a classical and safe research design. However the research is based primarily on the experience of one radiologist, who analyzed the images. The expertise of this person is emphasized twice in the "methods". This presents two important limitations in the study, which have not been reported in the manuscript: i. Checking or improving the reliability of this study has never been an issue for the authors. I believe that at least two radiologists, or other experts, should independently evaluate the images. It is important that the interrater reliability is measured and documented in such a manuscript. This could change dramatically the results of the study, or lead to a more successful classification of the unclassified cases. ii. Diagnosing an AE is important for radiologists and physicians who provide their services in rural areas or often small hospitals. An AE specialist is rarely the first person to diagnose such a condition. This means that an ultrasonographic

classification should be a tool that should address these needs and should take into consideration the capacities of these physicians. In case only hepatic US experts are able to use this tool, the validity of the tool and the study could be questioned. Most probably such issues have been discussed among the study team. Such limitations should be explicitly referred in the manuscript. iii. There is a problem regarding the generalizability of the study. The authors parallelize the results of their work to the WHO CE US stages. I agree that there are a lot of similarities. A major difference though, is that the CE stages present not only morphologically different lesions, but primarily different stages of activity of the CE lesions. This has major consequences regarding the treatment and the prognosis of the person suffering from CE. In the case of this study there is no reference to such evidence. iv. No reference to correlations to the morphological findings of different imaging techniques, for instance MRI (Kodama). (3) The quality of presentation and readability: Very interesting and well prepared figures. The manuscript in general reads well and easy. Little polishing in English needed, but the potential is good. Table of abbreviations needed. (4) Ethics-related aspects of the research: No ethical problems detected. ? Specific comments: Title: The main and short titles accurately reflect the major topic and content. The abbreviation AE could be introduced in the title. Abstract: The abbreviation AE shall be introduced either in the title or in the abstract. How has the classification scheme been developed? The rarity of the condition has been underlined at least three times in the manuscript and once in the abstract, without providing any numbers. I would like to be informed about the prevalence and incidence of the condition for instance in endemic regions. There is no reference to limitations, reliability / validity in the abstract. ? Materials and Methods: The study design, sample size and materials are adequately defined. The method used is in general sufficient for this type of study. However it is not clear enough why this type of categorization was chosen in advance. It looks like having the categories – results prepared in advance, and attempting to build a study around this model. I believe that the team has worked very well, ending up to these five categories, but the manuscript does not describe how and why they chose them in the methods section. I would like to see these details in a revised manuscript. The study is clearly reproducible.

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Name of journal: World Journal of Gastroenterology

ESPS manuscript NO: 18248

Title: Proposal of an ultrasonographic classification for hepatic alveolar echinococcosis

Reviewer's code: 02720326

Reviewer's country: Germany

Science editor: Jing Yu

Date sent for review: 2015-04-15 15:41

Date reviewed: 2015-05-27 00:27

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
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		<input checked="" type="checkbox"/> No	

COMMENTS TO AUTHORS

see pdf document

Dear authors,

I recommend publication of your manuscript but nevertheless, a few aspects should be improved for readers of the journal, as commented below.

- Introduction:

You state the liver to be the first organ affected by metacestode development. Actually, this is mostly, but not always the site of primary infestation.

Please use the correct terms, i.e. 'Echinococcus alveolaris' (not written in italics) or 'metacestode stage of *E. multilocularis*' instead of just '*E. multilocularis*' when you write about the metacestode.

You use the term 'HAE' without explaining the abbreviation. Since you only use it once, an abbreviation is not necessary.

- Results:

n=25 is 13.5%, not 23.5%.

You should use 13.0% instead of 13%.

- Discussion:

In the 4th paragraph, you use $p < 0.005$, while you wrote 0.05 in the results part.

- Table 1:

85/185 is 45.9%, not 46.0%

Besides these regards to the content, there is need for some little improvement of the language.

Kind regards!