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ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology
ESPS manuscript NO: 28650
Title: Hepatitis E Virus: Western Cape, South Africa
Reviewer's code: 02462637
Reviewer's country: Japan
Science editor: Ze-Mao Gong
Date sent for review: 2016-07-12 19:34
Date reviewed: 2016-07-15 16:52

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
[ ] Grade A: Excellent	[ ] Grade A: Priority publishing	Google Search:	[ ] Accept
[ ] Grade B: Very good	[Y] Grade B: Minor language polishing	[ ] The same title	[ ] High priority for publication
[Y] Grade C: Good	[ ] Grade C: A great deal of language polishing	[ ] Duplicate publication	[ ] Rejection
[ ] Grade D: Fair	[ ] Grade D: Rejected	[ ] Plagiarism	[ ] Minor revision
[ ] Grade E: Poor		[Y] No	[Y] Major revision
		BPG Search:	
		[ ] The same title	
		[ ] Duplicate publication	
		[ ] Plagiarism	
		[Y] No	

COMMENTS TO AUTHORS

The authors conducted an assessment of anti-HEV IgG seroprevalence in the Western Cape Province of South Africa, comparing seroprevalence between a number of risk factors within all major ethnic/race groups. The seroprevalence was 29.1% (21.9% age-adjusted) and was similar in three major ethnic groups, both HIV positive and negative individuals. The high seroprevalence indicated that HEV is endemic in the Western Cape, South Africa. The single risk factor for seropositivity in multivariate analysis was pork consumption [OR 2.052 (1.39-3.03), p<0.001] within their questionnaire. The authors also demonstrated a recent clinical case who infected with HEV genotype 3 strain in the Western Cape. The study is organized well and the manuscript is well written, but some parts had better be revised. Major problem The authors concluded that HEV genotype 3 is currently circulating in the Western Cape from just one clinical case by HEV genotype 3 strain. The case is possibly a rare case and HEV genotype 3 may not be circulating. "HEV genotype 3 is currently circulating" is an arbitrary speculation. The authors should change or exclude too strong expressions. Title page Article summary line: The authors wrote, "HEV is endemic in Western Cape, South Africa and genotype 3 is currently circulating in humans." The summary seems inappropriate

for the authors' observation. The authors should change it to the fact or the neutral inference. Whether genotype 3 is currently circulating is unknown from the authors' observation. The last sentence of "Findings" of Abstract "A recent clinical case demonstrates that HEV genotype 3 is currently circulating in the Western Cape." is too strong. It is not a finding but a speculation. The authors should change or exclude too strong expressions. The tenth line of the third paragraph of Discussion "Our clinical case confirms that HEV genotype 3 is currently circulating in Western Cape. Two further cases of HEV genotype 3 infection in immunosuppressed patients have also been recently documented in Cape Town11, 12." should be like "Two cases of HEV genotype 3 infection in immunosuppressed patients have been recently documented in Cape Town11, 12. Our clinical case was also infected with HEV genotype 3. These findings suggest that HEV genotype 3 is currently circulating in Western Cape." The second sentence from the end of the text, "Recent cases suggest HEV genotype 3 is currently circulating in South Africa." is appropriate, I think. Minor problems The fifth line of "Clinical case of hepatitis E" of Findings "The patient worked as a truck driver but had no history of recent travel outside of the Western Cape." "recent" is not sufficient. Concrete period should be described as "within 2-3 months". The ninth line of "Clinical case of hepatitis E" of Findings "PT-INR" rather than "INR" will be better for the readers worldwide. References "The authors" are the first for some references and "Article title" is the first for other references. Please make sure appropriate format for the World Journal of Gastroenterology. References Journal names are abbreviated for some references and are not abbreviated for others. Please make sure appropriate format for the World Journal of Gastroenterology.

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ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology
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CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
[ ] Grade A: Excellent	[Y] Grade A: Priority publishing	Google Search:	[ ] Accept
[Y] Grade B: Very good	[ ] Grade B: Minor language polishing	[ ] The same title	[ ] High priority for publication
[ ] Grade C: Good	[ ] Grade C: A great deal of language polishing	[ ] Duplicate publication	[ ] Rejection
[ ] Grade D: Fair	[ ] Grade D: Rejected	[ ] Plagiarism	[Y] Minor revision
[ ] Grade E: Poor		[Y] No	[ ] Major revision
		BPG Search:	
		[ ] The same title	
		[ ] Duplicate publication	
		[ ] Plagiarism	
		[Y] No	

COMMENTS TO AUTHORS

Interesting manuscript. Being the study prospective, it is important that the Authors indicate the start date and the completion date of participants enrolment.

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Name of journal: World Journal of Gastroenterology
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CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
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		[ ] Plagiarism	
		[Y] No	

COMMENTS TO AUTHORS

This study describes the prevalence of anti-HEV IgG in hospital patients and blood donors in Western Cape, South Africa, and identified consumption of pork as its risk factor. The study also reported a case of fulminant hepatic failure following acute HEV infection with genotype 3 in a patient who had chronic alcoholic liver disease. The study provides useful information for better understanding of HEV epidemiology in South Africa. However, the study needs to be improved for the following points: Major comments 1. In both abstract and result section in the main text, prevalence should be accompanied by a numerator and a denominator, and its 95% CI. 2. Table 2 should be improved by: - Including age group and sex as potential risk factors. The authors should present statistical analysis for the association between age group and positive IgG, which I guess should be statistically significant (by looking at Figure 1). - Table 2 should present: i) numerator, denominator and % (prevalence) of positive IgG in each variable (for example, for each of "sausage consumption YES" and "sausage consumption NO"); ii) odds ratio and its 95% CI for each of all the variables, irrespective of statistical significance; iii) just one P-value for a category with more than two groups (e.g., race). - The authors should clarify which variables were included in a

multivariable analysis. It is unclear whether race was included in the model. In addition, if age is associated with the outcome, this should be also included in the multivariable model. 3. I cast strong doubt on the accuracy of the "known HIV status" obtained from a structured questionnaire. Positive patient's history on HIV might be adequately specific, but "not known HIV status" does not really rule out HIV infection, unless patients are systematically screened at hospital for HIV. The analysis on HIV should be removed, or this limitation needs to be discussed. 4. Major limitation of the current study is the recruitment of study participants in hospitals and blood bank, which cannot be considered to represent the general population. I recommend the authors to present prevalence according to the origin of the patients (either recruited at inpatient/outpatient, or blood bank). The authors concluded that higher prevalence observed in the current study (compared to two historical ones in South Africa) is related with higher sensitivity of the kit. But, the fact that the population studied are from hospital, and these subjects cannot be considered as a representative sample from the general population, should be clearly acknowledged in the discussion as a limitation. The authors mention in the discussion that "population that was studied had fewer white children and elderly blacks, compared to the normal population", but the question is not only the difference in distribution of age and ethnicity. Those who came to hospitals may be richer than the general population who cannot afford to pay medical care. Such a socioeconomic difference may impact on the different distribution in terms of known risk factors for HEV infection (pork consumption, lack of access to hygiene). 5. In the discussion, the authors mention that the association between pork/pig and HEV seropositivity is typical of European populations. But, this association has been previously reported in numerous studies in sub-Saharan Africa (at least in Ghana, Gambia and Nigeria). The discussion needs to incorporate these historical African studies. Minor comments 1. Period of recruitment of study participants should be reported in the result. 2. Table 1: confidence intervals are not properly presented.