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ESPS PEER REVIEW REPORT

Name of journal: World Journal of Gastroenterology
ESPS manuscript NO: 13163
Title: MicroRNA-1290 targets SCAI and promotes esophageal squamous cell carcinoma proliferation and metastasis
Reviewer code: 00057996
Science editor: Ya-Juan Ma
Date sent for review: 2014-08-09 15:57
Date reviewed: 2014-09-12 12:04

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

COMMENTS TO AUTHORS

Rui-Hua Shi and co-authors investigated in their manuscript "MicroRNA-1290 targets SCAI and promotes esophageal squamous cell carcinoma proliferation and metastasis" the role of miR-1290 in progression and invasion of esophageal squamous cell carcinoma. Overall, the authors used clinical samples for expression analyses, and a number of experimental state-of-the-art techniques to investigate the respective questions in an in-vitro model. The different steps of the manuscript are logical, and the results present some very interesting findings about the role of miR-1290 in esophageal squamous cell carcinoma. In general, I do not have any major concerns against a publication of this manuscript in World Journal of Gastroenterology. However, there are a number of issues that should be addressed before potential publication. 1) The most relevant limitation of the manuscript in its current version is the English grammar and typesetting. There are several grammatical errors, and the content is sometimes difficult to understand in the current version (eg in Results 3.1 the authors state: "Our results demonstrated that the relative increased folds of miR-1290 expression was markedly upregulated (>7 times)…". Furthermore, the legends and axes of the figures are insufficient at this stage (eg: legends should contain explanation of all abbreviations used in the graphs such as nc, inc, inhi etc. Or: figure 6b: x-axis not correct labelled). In summary, I recommend careful proof-reading, assistance of English native speaking editor should be considered. 2) I did not find any information about a potential neoadjuvant treatment of the included patients.

This information is vital as neoadjuvant treatment could influence miRNA expression pattern. Please provide these data. 3) The authors use only one small nuclear RNA (U6) as control, and I couldn't find any info or data on the expression level of this small nuclear RNA in the different samples (should be comparable across samples). The fact of using just one control should at least be mentioned as limitation of the study, as normalisation to several controls is recommended in order to achieve more robust data. 4) The authors state in the methods that all data were presented as mean including STDEV. However, in Results 3.1: the authors use scatter blots, box plots and the Median to compare groups. This implicates that the data do not underlie normal Gaussian distribution what mandates the use of non-parametric statistical tests. The authors should please assess their data for distribution and use then adequate tests and presentation. 5) The authors state that they use the TNM classification according to the WHO. I couldn't find the provided reference "Bosman FT, Carneiro F, Hruban RH, Theise ND. WHO classification of tumours of the digestive system: World Health Organization, 2010:410- 417". However, as far as I am aware, AJCC and UICC both use classifications that have N0/N1/N2/N3 stages and not only N0 and N1 stages. Furthermore, there is usually a distinction between Stage I – Stage IV disease. On which basis do the authors choose to just compare N0 versus N1, and why do they combine Stage I and II in one group versus Stage III (Especially given that Stage I and Stage II both are defined as disease Stages without lymph node metastasis compared to higher Stage disease) ? In addition, the allocation of patients into T1+2 versus T3+4 or G1 versus G2-4 groups seems random. The authors should explain why the combine patients into the respective groups. Does maybe the direct comparison between all groups (N0-N3, T1-T4, Stage I – Stage IV) result in non-significant differences between groups due to small sample size? Or is there a reason of combining patients to these groups? 6) The statement "Hence, upregulated miR-1290 expression was closely related to ESCC progression and metastasis." is in my opinion not supported by the data presented in this part. The authors should re-phrase

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		[ ] No records	[ ] Major revision

COMMENTS TO AUTHORS

Well done research with important conclusion