

ESPS Peer-review Report

Name of Journal: World Journal of Gastroenterology

ESPS Manuscript NO: 3212

Title: Diabetes mellitus carries a risk of gastric cancer: A metaanalysis.

Reviewer code: 02453874

Science editor: Zhai, Huan-Huan

Date sent for review: 2013-04-15 15:02

Date reviewed: 2013-05-01 06:51

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 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 checked="" type="checkbox"/> Major revision

COMMENTS TO AUTHORS

1. All odds ratios and incidence risk ratios in the introduction should be reported with 95% CIS. It is not clear why OR:1.2-1.6 has been declared neutral.
2. The exact search phrase used in Medline containing the key words and Boolean operators should be reported, to make the search reproducible. Same should be done for search phrase used for finding previous meta-analyses.
3. Only keywords for the exposure and outcome are reported. Were keywords used for population and study design? If yes, they should be reported, if not why? Were MeSH used? If yes, please report, if not why?
4. Inclusion and exclusion criteria for both title/abstract and full text reviews should be reported?
5. There needs to be a table 1, containing characteristics of the studies that are eventually included.
6. The definitions used for I squared index are not clear. Usually over 70-75% is considered high heterogeneity, but it seems that 50% has been used here.
7. In the results section the actual pooled RRs and CIs should be reported for East Asian and Western categories. Only the pooled RR for East Asian studies is reported right now.
8. Has quality of the included studies been critically evaluated? if yes, the results should be reported, if no why?
9. The author insists on using the actual number of GC events in this analysis instead of pooling the RRs reported by each study. It is stated that this will lead to a more precise evaluation of the association of interest. I am not aware of any epidemiologic or statistical reason to justify this claim. Pooling reported RRs will not decrease the validity of the analysis compared to pooling raw event numbers. Furthermore, limiting the meta-analysis to pooling just actual event numbers has



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introduced two major problems. One is that it has decreased the number of included studies. The other one which may be even more important is that it does not let the RRs that are used to be adjusted for confounders that are measured in the studies. The author rightfully mentions the potential for bias due to confounding of the DM-GC association by multiple variables. If RRs are adjusted for confounders in individual studies, using these adjusted RRs will help to limit the bias. I strongly suggest using the RRs instead of the actual numbers because of these two reasons.

10. The explanation in the discussion to justify the qualitative difference in pooled RRs from East Asian versus Western studies is not enough/valid. Please clarify.