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ESPS PEER REVIEW REPORT

Name of journal: World Journal of Meta-Analysis

ESPS manuscript NO: 14853

Title: Prophylactic tracheal intubation for upper GI bleeding: A meta-analysis

Reviewer code: 00073423

Science editor: Xue-Mei Gong

Date sent for review: 2014-10-28 09:20

Date reviewed: 2014-11-06 21:12

CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
<input type="checkbox"/> Grade A: Excellent	<input checked="" 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"/> Existing	<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	BPG Search:	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E: Poor		<input type="checkbox"/> Existing	<input type="checkbox"/> Major revision
		<input type="checkbox"/> No records	

COMMENTS TO AUTHORS

A major argument for the publishing is, that it is his is a first meta-analysis on the topic of prophylactic endotracheal intubation during endoscopy for upper GI bleeding. The authors could not convincingly background their choice to perform this metaanalysis. Though authors state in the introduction, that "Although significant morbidity and mortality can result from pulmonary aspiration that may complicate endoscopy...", but this statement has no references, especially in the setting of endoscopies for upper GI bleeding. It is not surprising, that there are no prospective studies on the topic. Probably it is not so actual and important? The meta-analysis based on of retrospective studies must be evaluated with caution. Retrospective data have potential bias. Nevertheless, we have to admit, that the meta-analysis provides us available information. Some revisions could be performed: 1. In the introduction to make an accent why this problem is important in this specific setting (Upper GI bleeding). 2. Perhaps introduction could be shorter? Especially first part? 3. Probably one of endpoints could be the success of endoscopic hemostasis? 4. Conclusions: I do not believe that there is "the need for a randomized controlled trial to assess the issue of prophylactic intubation prior to endoscopy in patients with UGIB." 5. I would advise to make the recommendations that there are no data indicating the need of intubation?



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ESPS PEER REVIEW REPORT

Name of journal: World Journal of Meta-Analysis

ESPS manuscript NO: 14853

Title: Prophylactic tracheal intubation for upper GI bleeding: A meta-analysis

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Science editor: Xue-Mei Gong

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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"/> Existing	<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	BPG Search:	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E: Poor		<input type="checkbox"/> Existing	<input type="checkbox"/> Major revision
		<input type="checkbox"/> No records	

COMMENTS TO AUTHORS

The authors have undertaken a comprehensive literature search across four electronic databases (although studies were limited to the English language) and have followed MOOSE guidelines in its conduct. The quality of included studies was assessed using a quality assessment tool for quantitative studies which has been formally compared to the Cochrane risk of bias tool. Specific Comments: 1. Abstract: UGIB – should be spelt out in full before first mention. As should ACG and AGA in the results section or alternatively include them amongst the list of abbreviations. 2. Methods: Reference to Figure1 and Table 1 should only be made in the results section. Insert (AA,MB) after ‘All studies that met the inclusion criteria were reviewed by two investigators’ 3. Statistical analysis – I would usually suggest the investigating sources of heterogeneity by performing subgroups on study location/time period/exclusion of abstracts but there are too few studies to do this. This could be mentioned as a limitation. Also, where formal tests for publication bias undertaken i.e.: Begg/Egger tests or was the assessment based on visual inspection. If so, please specify. 4. The discussion is a little oddly structured, I would have expected to see it begin with the first outlined strength (i.e.: “this is the first meta-analysis....”) and then go on to discuss salient findings. There is also no discussion of the strengths/weaknesses (critical appraisal) of the four included studies – which is an important component of a review. The authors state ‘the results should be interpreted with caution in light of the limitations of meta-analysis’, but I think the point should be made that it is not the meta-analysis but the limited number of studies feeding into it which must be cautiously interpreted. For example,



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in figure 2, the study by Rehman et al is responsible for 78.6% of the pooled effect estimate, and was the only study to record a pneumonia event in the no PI group. In my opinion, the studies by Koch and Tang with their extremely high upper 95% CIs just inflate the observed association. The validity of the pooled estimate, really comes down to how reliable the estimate from the Rehman study was – this needs commentary in the discussion. 5. Table 1 - as this review is not an analysis of RCTs (where baseline characteristics between the groups would be expected to be equal), I think it would be useful for the reader to know a bit more about the differences in baseline characteristics of the study populations under investigation i.e.: age (age-group), gender, comorbidity status, did the case-control studies use population-based controls or hospital controls etc. Where available, this information may help determine potential sources of heterogeneity and selection bias. The authors could also note individual study adjustments i.e.: Rehman et al matched controls on the propensity of intubation (which would mean that the differences in findings wouldn't be expected to differ by the indication/prognostic factors leading to intubation), what did the other studies do? I would remove the months from the time column and just state the years (period) in which the study was conducted. I would also suggest adding the study reference number into the reference column. 6. Figure 5 - It might be a good idea to plot the confidence limits around the SEs to aid the reader's visual assessment of asymmetry in the funnel plot.



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ESPS PEER REVIEW REPORT

Name of journal: World Journal of Meta-Analysis

ESPS manuscript NO: 14853

Title: Prophylactic tracheal intubation for upper GI bleeding: A meta-analysis

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CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
<input type="checkbox"/> Grade A: Excellent	<input checked="" 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"/> Existing	<input type="checkbox"/> High priority for publication
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<input type="checkbox"/> Grade E: Poor		<input type="checkbox"/> Existing	<input checked="" type="checkbox"/> Minor revision
		<input type="checkbox"/> No records	<input type="checkbox"/> Major revision

COMMENTS TO AUTHORS

1.UIGB should exclude in short title 2.Meta-analysis should exclude in key word, copmlication,pneumonia and aspiration could be add. 3. Figure 5 can be omit.



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ESPS PEER REVIEW REPORT

Name of journal: World Journal of Meta-Analysis

ESPS manuscript NO: 14853

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Reviewer code: 01557574

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<input type="checkbox"/> Grade A: Excellent	<input checked="" type="checkbox"/> Grade A: Priority publishing	Google Search:	<input checked="" type="checkbox"/> Accept
<input type="checkbox"/> Grade B: Very good	<input type="checkbox"/> Grade B: Minor language polishing	<input type="checkbox"/> Existing	<input type="checkbox"/> High priority for publication
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COMMENTS TO AUTHORS

Dear Author, This article should be published (Prophylactic tracheal intubation for upper GI bleeding: A meta-analysis). But, the priority is not high for publication. Because everybody knows that prophylactic tracheal intubation is not necessary before endoscopy. Sincerely yours.