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ESPS Peer-review Report
Name of Journal: World Journal of Gastroenterology
ESPS Manuscript NO: 5342
Title: Predictors for failure of stent treatment for benign esophageal perforations - a single center 10-year experience
Reviewer code: 00057695
Science editor: Ma, Ya-Juan
Date sent for review: 2013-09-02 16:06
Date reviewed: 2013-09-03 20:44

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

COMMENTS TO AUTHORS

I have the following comments: 1. In the narrative there are many words that are not spaced. This may be due to the way the article format was downloaded in my computer, or this was actually the case in the submitted draft. If it is the latter, this needs to be clarified and corrected. 2. Abstract: I felt the abstract was a little bit long and hence can be made shorter by trimming unnecessary details. Also correct the time range for the successful SEMS. 3. In the management strategy: give the name antibiotics used and their dosages and frequency. Also how were the feeding jejunostomy inserted? 4. Results, line 9: change the word (acute) in (acute esophagectomy) to (emergency). 5. In the Clinicopathological findings, paragraph 2 , line 7: the number of recovered patients was 33 and not 30. Also after the remaining patients add the number of patients between brackets (n=3). 6. Discussion: the first paragraph contained some repeated sentences. Second paragraph, it was mentioned that "SEMS placement can be completed in any hospital where endoscopic service is available". I disagree with this statement as stent placement is hardly available in many endoscopic units around the world despite the widespread availability of diagnostic endoscopic services. This is specifically true in the developing and under-developed countries. Covered stents are very expensive and cannot be afforded by many. I suggest the authors highlight the cost of endoscopic stenting in their discussion. 7. How many attempts at redo stenting is tried before failure is declared and surgery is contemplated? I believe under this setting, the prognosis is poor and surgery carries high mortality rate. Further clarification is needed here. 8. Regarding referred patients, inevitable delay is expected especially in countries where referral to tertiary centers is hampered by bureaucracy. This will adversely affect the outcome of endoscopic stenting. Unless a fast-tack referral system is available, delays are

expected with subsequent higher morbidity and mortality. This needs to be alluded to in the discussion. 9. When is the appropriate time for stents removal, if they are not biodegradable? This should be mentioned in the Methods and should be supported by references. 10. Were there any stent-related complications, or complications during or after removal of the stents?

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ESPS Peer-review Report
Name of Journal: World Journal of Gastroenterology
ESPS Manuscript NO: 5342
Title: Predictors for failure of stent treatment for benign esophageal perforations - a single center 10-year experience
Reviewer code: 00058455
Science editor: Ma, Ya-Juan
Date sent for review: 2013-09-02 16:06
Date reviewed: 2013-09-22 12:58

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

COMMENTS TO AUTHORS
1.For a patient of EPR, is the response of treatment depended on stent itself or feeding jejunostomy or percutaneous thoracic drains? 2.Is the timing of percutaneous thoracic drains or stent, which one provided real value for a patient of EPR? 3. The authors should clarify if only stent providing the good outcome of treatment for EPR. 4. A reference as below might give other answer about the stent for EPR. Endoscopic stent insertion versus primary operative management for spontaneous rupture of the esophagus (Boerhaave syndrome): an international study comparing the outcome. Schweigert M, Beattie R, Solymosi N, Booth K, Dubecz A, Muir A, Moskorz K, Stadlhuber RJ, Ofner D, McGuigan J, Stein HJ. Am Surg. 2013 Jun;79(6):634-40.

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ESPS Peer-review Report
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Science editor: Ma, Ya-Juan
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Date reviewed: 2013-09-30 12:21

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

COMMENTS TO AUTHORS
This is an interesting manuscript because a large referral unit has been able to manage complex patients with esophageal perforations with a single protocol for 10 years. However, esophageal perforations are varied and many do not require specific treatment while others should have an esophageal resection. While the results from the paper appear impressive, they do not discuss whether they would have taken a different course for some patients. Would they recommend now that those with delayed presentation and with pleural sepsis had esophageal resection? Further would they recommend a conservative management with no stent for patients with a minimal perforation? Is there a different strategy that they would apply to perforations of the lower esophagus? Was it possible to predict treatment failure in a timely fashion that an esophagestomy is still feasible? I think the language is a little verbose. I do not think they need to repeat all the results in the tables in the description of the results but to highlight the important findings.

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ESPS Peer-review Report
Name of Journal: World Journal of Gastroenterology
ESPS Manuscript NO: 5342
Title: Predictors for failure of stent treatment for benign esophageal perforations - a single center 10-year experience
Reviewer code: 00008985
Science editor: Ma, Ya-Juan
Date sent for review: 2013-09-02 16:06
Date reviewed: 2013-10-12 22:57

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

COMMENTS TO AUTHORS
I have the following comments / questions to the authors: 1. Is there any patient treated with SEMS insertion under LA or intravenous sedation instead of GA? Because in the reviewer's institution, we never insert SEMS in GA. 2. The selection of patient for SEMS probably is safer to base on patient's clinical condition rather than just "intention to treat". Not sure if the last patient of "cardiovascular comorbidity" could have different outcome if operated promptly. 3. The authors did not mention when and how to retrieve the SEMS after insertion. The retrieval of SEMS could be extremely challenging especially in older days when only metallic stents were available. If the stent left in situ for too long, it might not be possible to retrieve it. In the benign condition, if the stent is left too long period may cause long-term problem. 4. I would be very cautious to give a comment / conclusion that SEMS is indicated for all/most EPR patients since I still believe clinical condition is probably the most important consideration factor in choosing the most appropriate treatment strategy for these patients. 5. It would be better if the authors could provide which type of SEMS they used in these patients. 6. There are some obvious grammatical mistakes.