



PEER-REVIEW REPORT

Name of journal: World Journal of Clinical Cases

Manuscript NO: 47070

Title: Clinical differentiation of acute appendicitis and right colonic diverticulitis

Reviewer's code: 00069988

Reviewer's country: Croatia

Science editor: Jia-Ping Yan

Reviewer accepted review: 2019-03-13 08:25

Reviewer performed review: 2019-03-13 09:54

Review time: 1 Hour

SCIENTIFIC QUALITY	LANGUAGE QUALITY	CONCLUSION	PEER-REVIEWER STATEMENTS
<input type="checkbox"/> Grade A: Excellent	<input type="checkbox"/> Grade A: Priority publishing	<input type="checkbox"/> Accept	Peer-Review:
<input type="checkbox"/> Grade B: Very good	<input checked="" type="checkbox"/> Grade B: Minor language	(High priority)	<input checked="" type="checkbox"/> Anonymous
<input type="checkbox"/> Grade C: Good	polishing	<input type="checkbox"/> Accept	<input type="checkbox"/> Onymous
<input checked="" type="checkbox"/> Grade D: Fair	<input type="checkbox"/> Grade C: A great deal of	(General priority)	Peer-reviewer's expertise on the
<input type="checkbox"/> Grade E: Do not	language polishing	<input type="checkbox"/> Minor revision	topic of the manuscript:
publish	<input type="checkbox"/> Grade D: Rejection	<input checked="" type="checkbox"/> Major revision	<input checked="" type="checkbox"/> Advanced
		<input type="checkbox"/> Rejection	<input type="checkbox"/> General
			<input type="checkbox"/> No expertise
			Conflicts-of-Interest:
			<input type="checkbox"/> Yes
			<input checked="" type="checkbox"/> No

SPECIFIC COMMENTS TO AUTHORS

All comments are put directly in the manuscript Word document in RED.

INITIAL REVIEW OF THE MANUSCRIPT

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PEER-REVIEW REPORT

Name of journal: World Journal of Clinical Cases

Manuscript NO: 47070

Title: Clinical differentiation of acute appendicitis and right colonic diverticulitis

Reviewer's code: 03479745

Reviewer's country: Singapore

Science editor: Jia-Ping Yan

Reviewer accepted review: 2019-03-15 02:55

Reviewer performed review: 2019-03-16 06:27

Review time: 1 Day and 3 Hours

SCIENTIFIC QUALITY	LANGUAGE QUALITY	CONCLUSION	PEER-REVIEWER STATEMENTS
<input type="checkbox"/> Grade A: Excellent	<input type="checkbox"/> Grade A: Priority publishing	<input type="checkbox"/> Accept	Peer-Review:
<input type="checkbox"/> Grade B: Very good	<input checked="" type="checkbox"/> Grade B: Minor language	(High priority)	<input checked="" type="checkbox"/> Anonymous
<input checked="" type="checkbox"/> Grade C: Good	polishing	<input type="checkbox"/> Accept	<input type="checkbox"/> Onymous
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		<input type="checkbox"/> Rejection	<input type="checkbox"/> General
			<input type="checkbox"/> No expertise
			Conflicts-of-Interest:
			<input type="checkbox"/> Yes
			<input checked="" type="checkbox"/> No

SPECIFIC COMMENTS TO AUTHORS

1. Title. The authors could probably add their methodology into the study title (STROBE statement). This would probably help portray a more accurate reflection of the hypothesis/methodology of this manuscript. 2 Abstract. The abstract is well summarised and reflects the work, the authors could perhaps provide a better



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conclusion rather than to merely just report the findings that have been reflected already in their results section of the abstract. It defeats the purpose of the conclusion section if you are merely reporting the results which you have already represented earlier. 3 Key words. The keywords reflect the manuscript purpose. Authors may want to consider removing "nonetheless" as the opening word in their core tip section. 4 Background. The authors have set the stage well for this uniquely Asian problem in their background. I applaud them for identifying this gap in literature and attempting to contribute to literature with this methodology. 5 Methods. The authors have described their methodology well and appears to have done due diligence accordingly as well. However, may i suggest the following: 1. Under the Design and patients section A. "Based on the findings of the CT scan and an ultrasound, patients with AA were diagnosed with complicated appendicitis if they had gangrenous appendicitis, perforated appendicitis, or appendicitis complicated with an intra-abdominal abscess." This statement is redundant and may appear to be confusing when the earlier sentence says they have already confirmed all diagnosis by CT. It can simply be said the both simple and complicated appendicitis were included in this study. As there is no subgroup analysis in this group of complicated appendicitis, there is no need to specifically address this point now. Furthermore the authors discuss extensively later in the manuscript their rationale for including this group which addresses their inclusion. B. "All CT findings were reviewed by several different radiologists and surgeons. " Why is this line used again after stating that it was diagnosis was achieved with CT at clinical presentation? Does it imply that retrospectively the CT images and diagnosis was reviewed again after the IRB was approved and this study conducted or was it just obtaining data from the reports? Appears confusing again, especially if 2 lines before this it was mentioned that diagnosis was confirmed by CT. C. "Patients with previous history of appendectomy were excluded because appendicitis was quite unlikely for



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those patients." Please remove "because appendicitis was quite unlikely for those patients" as this is understood implicitly and it is a fair inclusion criteria. D. State exclusion criteria for Diverticulitis Does your cohort of patient include complicated diverticulitis? If not would i suggest you state explicitly at the start of your article that this study involve comparing acute appendicitis (both simple and complicated) with acute simple diverticulitis. If it included complicated diverticulitis, can i suggest you include that explicitly as well as well as reflect the numbers and proportion somewhere in your article. Also with regards to this, is the protocol of the centre thereafter to perform a colonoscopy or any other form of repeat imaging to confirm the diagnosis of diverticula in the right colon? If there was, you could mention it. 2. Study variables Most appear appropriate - why did the authors choose to include ALT as a variable? 3. Categorization of continuous variable From a methodology point of view, i can understand what the authors are trying to do with regards to using AUROC for CRP but is there a reason they chose to do this rather than using literature defined cut-offs of CRP from previous studies looking at this issue? Issue being using your own cohort to establish this cut off would introduce an element of reproducibility, especially since this was a study done retrospectively in nature. Otherwise, the authors have done well to explain their methodology from a univariate to a multivariate perspective. 6 Results. The results are appropriately represented. 7 Discussion. Does the manuscript interpret the findings adequately and appropriately, highlighting the key points concisely, clearly and logically? Are the findings and their applicability/relevance to the literature stated in a clear and definite manner? Is the discussion accurate and does it discuss the paper's scientific significance and/or relevance to clinical practice sufficiently? The authors once again summarise the results in the first paragraph again, this is unnecessary. They should consider using the last 2 lines of their first paragraph only "Previous studies have reported prolonged pain and higher age as predictors of



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ARCD and nausea/vomiting and leukocytosis as predictors of AA; most of our results were consistent with the results of previous studies [9-12]. On the other hand, history of diverticulitis, RLQ pain, and high serum CRP levels have not been previously established as predictors of ARCD." The authors then go on to describe the pathophysiology and relationship with clinical symptoms which is relevant. However i have issue with this 2 lines at the end of a paragraph - "Interestingly, our study showed that 93.9% of the patients without RLQ pain had RLQ tenderness during palpation. This discrepancy of symptoms and physical findings may underscore the importance of careful palpation in patients with acute abdominal pain regardless of the position of abdominal pain." On many counts, i do not feel this statement should be included. Not only is it confusing as there is no discrepancy, i do not think the authors should be making this point based on their study hypothesis, simply for the fact that RLQ tenderness also exists in appendicitis, also in view of the results they represent. From my understanding based on their hypothesis background and results based on the methodology performed, patients who present with RLQ pain as symptoms are more likely to have diverticulitis, the role of RLQ tenderness, is not useful in differentiating AA from ARCD. I think the authors must be cautious in their discussion of CRP and Leukocytosis in this setting of clinical differentiation. It just simply falls into the concept of "it may be statistically significant but is it clinical significant?" I would strongly urge the authors to re-word this portion of the discussion to highlight this issue. So are you therefore proposing based on your study a CRP value of this is the clinical differentiator? Also not withstanding the fact that the appendicitis group has a proportion of complicated appendicitis? also there is not date if the ARCD group has any patients with complicated diverticulitis (which hopefully the authors will address) 8 Illustrations and tables. Adequate. 9 Biostatistics. Adequate 10 Units. Meets standard unit references. 11 References. Appears appropriate. 12 Quality of manuscript



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organization and presentation. The manuscript is appropriately organised and presented. There is no glaring issues with language and grammar. This is commendable as the authors are not native speakers of English. 13 Research methods and reporting. The authors have done well to come up with this hypothesis and their aim has been clearly spelt out. Their methodology was extensively described as well. They have probably complete most of the STROBE Statement. However, they did not mention the use of STROBE in their methodology in the manuscript but only in their cover letter which i would encourage them to do if they have fulfilled the checklist. This would certainly make their manuscript more robust. 14 Ethics statements Authors have met the ethical standards. Submitted the necessary documents for proof. In summary, i appreciate this article and feel it may contribute to literature in this unique situation of an Asian disease. The authors did indeed identify a gap in terms of this question with a lack of robust methodology but however in view of the retrospective nature of this study with its inherent limitations. This is the short-fall of this study methodology. I urge the authors to correct and revise based on the suggestions to enhance the strengths.

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