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PEER-REVIEW REPORT

Name of journal: World Journal of Clinical Cases

Manuscript NO: 62079

Title: Alström syndrome with a novel mutation of ALM and Graves' hyperthyroidism: a case report and literature review

Reviewer's code: 03728338
Position: Peer Reviewer
Academic degree: MD, MSc, PhD
Professional title: Research Scientist, Surgeon
Reviewer's Country/Territory: Germany
Author's Country/Territory: China

Manuscript submission date: 2020-12-30
Reviewer chosen by: AI Technique
Reviewer accepted review: 2020-12-30 21:22
Reviewer performed review: 2020-12-30 21:59
Review time: 1 Hour

Scientific quality	[ ] Grade A: Excellent [Y] Grade B: Very good [ ] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish
Language quality	[Y] Grade A: Priority publishing [ ] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [ ] Accept (General priority) [Y] Minor revision [ ] Major revision [ ] Rejection
Re-review	[Y] Yes [ ] No
Peer-reviewer statements	Peer-Review: [Y] Anonymous [ ] Onymous Conflicts-of-Interest: [ ] Yes [Y] No

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SPECIFIC COMMENTS TO AUTHORS

The authors claim that this is the first case to report the association between AS and Graves' hyperthyroidism. A thorough literature review confirms this statement. The authors also describe two new mutations, which, to the best of my knowledge, have not been previously reported indeed. This is a well-written manuscript. The novel mutations are alone a very good reason why this manuscript is worth publishing. The case is very detailed presented. The title is representative of the content, the structure is good and the manuscript discusses all important aspects. I congratulate the authors on their efforts, also on testing the patient's family. However, I have a concern before the manuscript can be accepted for publication: - the association between AS and Graves is not convincing. One can argue that in that case it was just a coincicence. Do the authors have enough evidence that this is indeed an association and not a coexistence? If yes, they should propose a causality mechanism. If not, I recommend to rewrite the statement of purpose and part of the manuscript, to focus only on the new mutations (as innovation). In this case the literature review should focus on all thyroid diseases found in AS patients (eg. cancer).

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PEER-REVIEW REPORT

Name of journal: World Journal of Clinical Cases

Manuscript NO: 62079

Title: Alström syndrome with a novel mutation of ALM and Graves' hyperthyroidism: a case report and literature review

Reviewer's code: 05644467
Position: Peer Reviewer
Academic degree: MD

Professional title: Doctor

Reviewer's Country/Territory: China

Author's Country/Territory: China

Manuscript submission date: 2020-12-30

Reviewer chosen by: AI Technique

Reviewer accepted review: 2020-12-31 13:14

Reviewer performed review: 2021-01-01 13:16

Review time: 1 Day

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [Y] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish
Language quality	[ ] Grade A: Priority publishing [Y] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [Y] Accept (General priority) [ ] Minor revision [ ] Major revision [ ] Rejection
Re-review	[ ] Yes [Y] No
Peer-reviewer statements	Peer-Review: [Y] Anonymous [ ] Onymous Conflicts-of-Interest: [ ] Yes [Y] No

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SPECIFIC COMMENTS TO AUTHORS

It's an interesting case both diseases ocuured in one girl. The correlation between the two disease is unclear. Please explain your conclusion：Manifestation of hyperthyroidism may suggest rapid progression of AS.

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PEER-REVIEW REPORT

Name of journal: World Journal of Clinical Cases

Manuscript NO: 62079

Title: Alström syndrome with a novel mutation of ALM and Graves' hyperthyroidism: a case report and literature review

Reviewer's code: 00646519
Position: Editorial Board
Academic degree: PhD

Professional title: Doctor

Reviewer's Country/Territory: Mexico

Author's Country/Territory: China

Manuscript submission date: 2020-12-30

Reviewer chosen by: AI Technique

Reviewer accepted review: 2021-01-02 16:17

Reviewer performed review: 2021-01-06 16:25

Review time: 4 Days

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [ ] Grade C: Good [Y] Grade D: Fair [ ] Grade E: Do not publish
Language quality	[ ] Grade A: Priority publishing [Y] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [ ] Accept (General priority) [ ] Minor revision [ ] Major revision [Y] Rejection
Re-review	[ ] Yes [Y] No
Peer-reviewer statements	Peer-Review: [Y] Anonymous [ ] Onymous Conflicts-of-Interest: [ ] Yes [Y] No

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SPECIFIC COMMENTS TO AUTHORS

In the manuscript the authors present a case of AS with two apparently novel mutations associated with hyperthyroidism, these are some of my comments: In the case summary, authors should mention the other possibility apart from the truncated protein. Please clearly specify the term "abnormal liver function". How was diabetic nephropathy identified in the laboratory? this is not possible. At the end of the laboratory section, the authors describe normal kidney function which contradicts their previous sentence of diabetic nephropathy. In the imaging section the authors describe pulmonary function with obstructive ventilation disorder, this cannot be done by imaging, please correct it. How did the authors conclude that their mutations were gain? In the follow-up section, the authors mention the absence of an ovary after cystectomy never mentioned previously. In follow-up section, the authors refer that development was normal like the other children, this is very difficult due to the evolution of AS. In the sentence "In the present case, the patient carried pathogens ..... the exon 16 mutation may play a dominant role ..." the authors must be very cautious since AS is autosomal recessive. The article does not present the "in silico" analysis of the reported mutations. In the conclusion the authors state "Our case suggests that the manifestation of hyperthyroidism may indicate a rapid progression of AS", but they had previously mentioned that the development of the proband was comparable to children of their age, this is contradictory. Figure 4 does not show the truncated protein rather the evolutionary conservation of the amino acid and the stop codon. This has to be corrected. A protein modeling would be very useful and interesting. The main concern is that the article presents many inconsistencies and incongruities throughout the text, the manuscript must be completely restructured in a scientific and synoptic way before being considered for publication.