

Dear editors and reviewers

Thanks for reaching out to us regarding the manuscript entitled " Application of artificial intelligence in trauma orthopedics: limitation and prospects". We believe that these comments have helped us enhance the quality of the manuscript. We also have done our best to revise as well as improve the paper according to the comments. Herewith, we provided the authors' respond to each comment right after each statement. Please feel free to contact us if you need further information.

Best regards,

Corresponding author

Reviewer 1

The introduction does not clearly state the purpose or objective of the study, making it difficult to understand the scope of the research. It is highly recommended to use the below references to improve the introduction: 1. Artificial Intelligence and COVID-19: Deep Learning Approaches for Diagnosis and Treatment 2. Cancer Digital Twins in Metaverse 3. A Hybrid Echo State Network for Hypercomplex Pattern Recognition, Classification, and Big Data Analysis 4. A conceptual deep learning framework for COVID-19 drug discovery The term "neural networks" is not clearly defined, and readers who are not familiar with machine learning may find this confusing.

Thank you for your time and effort in evaluation the manuscript. I hope the answers are clear to your valuable comments on the article. Your opinion can improve the quality of the manuscript and decrease misunderstanding. We have used your suggested outstanding articles and cited them

The manuscript makes a claim about the sensitivity and specificity of machine learning in detecting fractures, but there is no data or reference to support this claim. The conclusion suggests that future studies are necessary to develop more accurate and effective detection models, but it does not specify what kind of studies or research would be required.

Duly noted. Thank you for pointing out this shortcoming. We have revised the manuscript accordingly. But as you know utilizing machine learning is about programming, which is mostly done by engineers, and each time they run a model, the bug would be found and they try to solve it. Therefore, we can't suggest any programming changes, we just can evaluate them clinically, and find their bugs; so, the engineers will be informed about what they should work on.

The manuscript does not mention any potential limitations or challenges associated with using machine learning in orthopedic surgery, such as data quality, bias, or ethical considerations.

Thank you for this valuable note. We have revised it accordingly and added one separate section for limitation and strength.

The use of jargon and technical terms may make it difficult for a non-expert audience to understand the manuscript.

Thank you for noticing this point. We have tried to define each item as much as possible, during the introduction, we have added several paragraphs explaining the basic definitions. However, as you know, machine learning is a very huge world and this manuscript will be helpful for those with at least a background in this issue.

The manuscript does not provide any practical guidance or recommendations for orthopedic surgeons or healthcare professionals on how to incorporate machine learning into their clinical practice.

Duly noted. Thank you for pointing out this shortcoming. At the end of manuscript, we have noted the combination of machine learning and individual assessment based on the current model will optimize the diagnosis. Based on current model we can't completely rely on machines; However, future models and programs may be more efficient.

Reviewer 2

The article is within the scope of the journal. It deals with an interesting topic. It is well written. The reading is fluent. A review of the state of the art about "Application of artificial intelligence in trauma orthopedics" is carried out. To accept the article, it is necessary to make some improvements:

Thank you for your time and effort in evaluation the manuscript. I hope the answers are clear to your valuable comments on the article. Your opinion can improve the quality of the manuscript and decrease misunderstanding.

- a) It must explain how the review was carried out: sources consulted, article retrieval criteria, type of review.

Thank you for this valuable note. We have revised it. This is a narrative review. In the narrative review we don't actually have definite criteria and database such as what we have in systematic review. Meanwhile, we have added some information during introduction part.

- b) A discussion section should be included in which the results obtained from the review are shown: current lines of work, possible future lines of work, limitations of the review carried out, comparison with other reviews...

Thank you for pointing out this matter. Inclusion of search details is not mandatory in narrative reviews, which may compromise the thoroughness and impartiality of the search methods. Selective inclusion of publications that support a particular hypothesis can introduce bias and hinder exploration of the existing evidence. Narrative reviews often lack descriptions of their selection and review methods, making replication and verification of their results impossible, which conflicts with scientific evidence. These reviews rely on written paragraphs to summarize research findings and do not conduct pooled analyses, which limits objectivity and instead reflects dominant opinions at the time of publication. While narrative reviews may provide a general understanding of a body of evidence, they do not fully explore alternative hypotheses and cannot ensure the correctness of dominant opinions. The aforementioned statements have been added to the manuscript.

- c) A conclusions section should be included summarizing the scientific contribution of the article, and proposing lines of future work.

Thank you for noticing this point. We have revised the conclusion part accordingly.

Reviewer 2

First of all thanks for your hard work.

1-Scoring systems such as SYNTAX and Gensini scores are basically scoring systems used to determine the extent and the severity of coronary artery disease and to determine treatment choice based on angiographic data (see: a- doi: 10.7759/cureus.22482. PMID: 35345715; and b- doi: 10.18087/cardio.2021.12.n1757. PMID: 35057723.) and these scoring systems have been used in some studies to determine the prognosis of patients and have been observed to have successful results. Scoring systems such as Heart score and Grace score are scoring systems that try to show the prognosis of patients according to their clinical status (see: a- doi: 10.3390/jcm10102210. PMID: 34065227; and b- doi: 10.1016/j.carrev.2019.07.023. PMID: 31495747. and c- doi: 10.1007/s40520-021-02039-y. PMID: 34993906.) and the main expectation from these scoring systems is to be an indicator of coronary plaque burden and extent rather than determining the severity of coronary artery disease. I recommend that this issue be focused on and discussed in more depth.

Thank you for your time and effort in evaluation the manuscript. I hope the answers are clear to your valuable comments on the article. Your opinion can improve the quality of the manuscript and decrease misunderstanding. We have used your points and suggested articles.

2-I recommend that you compare your study results with the results of the following study in the discussion section. (see: Cedro AV, Mota DM, Ohe LN, Timerman A, Costa JR, Castro LS. Association between Clinical Risk Score (Heart, Grace and TIMI) and Angiographic Complexity in Acute Coronary Syndrome without ST Segment Elevation. Arq Bras Cardiol. 2021 Aug;117(2):281-287. English, Portuguese. doi: 10.36660/abc.20190417. PMID: 34495219 ;)

Duly noted. Thank you for pointing out this shortcoming. We have compared them: line 164-180 & 185-189; reference 25

3-According to the results of the study, the Heart score system can show anatomical complexity. In this case, how do the authors explain that patients with 2VD score higher than patients with 3VD. This should be explained in detail in the discussion section.

Thank you for this valuable note. The P. value of <0.001 has been calculated between all angiography group (SVD, 2VD, 3VD, SF or MB) not just between the two group of 2VD (6.59 ± 1.50) and 3VD (6.27 ± 1.28). the difference between this two group is not statistically significant ($P = 0.1380$) and we add this explanation to the manuscript. (line 126-128)

4-CABG is often the treatment option in patients with extensive coronary artery disease, but the heart score of patients who underwent MV PCI was found to be higher than those with CABG. The reasons for this situation should be explained in the discussion section.

Thank you for noticing this point. Depending on the treating doctor's as well as patients' preference and we, as investigators, had no role in selecting the modality. We have added this to discussion part. The P. value of <0.001 has been calculated between all management group (Medical, PCI-SV, PCI-MV, CABG). It was not significant just between two group of MV PCI and CABG ($P = 0.2981$). we have added this to the manuscript. (line 128-131)