Reviewer’s report

**Title:** SYSTOLIC BLOOD PRESSURE AS A PREDICTOR OF TRANSIENT ISCHEMIC ATTACK/MINOR STROKE IN EMERGENCY DEPARTMENT PATIENTS UNDER AGE 80: A PROSPECTIVE COHORT STUDY

**Version:** 0  **Date:** 10 Jun 2019

**Reviewer:** Reviewer 2

**Reviewer's report:**

PEER REVIEWER ASSESSMENTS:

**OBJECTIVE** - Full research articles: is there a clear objective that addresses a testable research question(s) (brief or other article types: is there a clear objective)?

Yes - there is a clear objective

**DESIGN** - Is the current approach (including controls and analysis protocols) appropriate for the objective?

No - there are minor issues

**EXECUTION** - Are the experiments and analyses performed with technical rigor to allow confidence in the results?

Yes - experiments and analyses were performed appropriately

**STATISTICS** - Is the use of statistics in the manuscript appropriate?

Yes - appropriate statistical analyses have been used in the study

**INTERPRETATION** - Is the current interpretation/discussion of the results reasonable and not overstated?

Yes - the author's interpretation is reasonable

**OVERALL MANUSCRIPT POTENTIAL** - Is the current version of this work technically sound? If not, can revisions be made to make the work technically sound?
Yes - current version is technically sound

PEER REVIEWER COMMENTS:

GENERAL COMMENTS: Overall, this study is excellent. It is very well written, clearly argued, analysis is robust (except as described below), and has clear clinical implications.

REQUESTED REVISIONS:

1) The major criticism I have is the exposure (i.e. the single BP at ED triage), rather than using multiple BPs while the patient is in the ED. BPs can often vary over time even if they are not treated, and these data may be independently predictive, i.e. persistently elevated BP. 2) The authors make no mention of potential reasons that this phenomenon may occur (i.e. pathophys). While clearly this study did not study the causative nature of this, it would be useful for a clinical audience. 3) The modeling appear robust, however, some journals favor the kitchen sink approach vs. backward elimination which can produce variable results. 4) The sentence in the results that state that SVO BPs are higher than others should be reconsidered because it did not appear that this hypothesis was directly tested. 5) If there are more data to study how changes in BP or variation may impact diagnosis, that would be good to add or as a separate study.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:
Acceptable

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