Reviewer's report

Title: Paradoxical Effect of Obesity on Hemorrhagic Transformation after Acute Ischemic Stroke

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Reviewer: Dong Zhao

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The manuscript by Chi Kyung Kim et al examined “paradoxical effect of obesity on hemorrhagic transformation after acute ischemic stroke” by a retrospective clinical data collection from 744 consecutively enrolled patients with first-ever acute ischemic stroke. Among 744 patients, 379 patients were excluded and data of 365 patients were analyzed for the association between BMI level (or obesity status) at hospital admission and risk of HTf. The research question addressed in this study is very important with scientific and practical value in understanding and guiding management of obesity and stroke. However some methodology defects in this study made the reliability of the conclusion a question mark. My comments and revision suggestions are as follows.

Major compulsory revision

1. Among the patients excluded, 70/379 (18%) was due to incomplete data of MRI, that 70 patients should have been included in the analysis if they had complete data. It is better to provide a comparison of characteristics of those 365 patients included and 70 patients excluded, especially the proportion of BMI classification. The HTf may have higher case fatality rate, if most of the 70 patients died?

2. In multiple logistic regression analysis, the micro-bleed was treated as a confounder. The data in table 2 showed that two obesity groups had higher micro-bleed rates but lower HTf. I think the microbleed and HTf should be considered as bleed with different extent and be of importance for clinical treatment decision and prognosis consideration. So, it is better to use HTf + microbleed as dependent variable instead of independent co-variables in multiple regression analysis, at least an additional analysis.

3. Antiplatelet or anticoagulation treatments before stroke attach may be important potential factors for HTf or microbleed. Even a small number of patients (22 patients if no overlap) had that history, it should be included in multiple regression analysis, like Thrombolysis (24 patients).

Minor essential revision

4. AF was considered as a confounder or a maker of the subtype (as indicated by the authors in result part. If the authors think that subtype may have an impact, why do not they direct using the type of ischemic stroke as co-variants? I do not think it is reasonable to add AF in multiple regression analysis. Instead, history of
antiplatelet or anticoagulation treatments should be added in.

5. Some descriptions are not accurate.
1) The second line under method part, what is ictus? If it is an abbreviation, please write it in a correct way.
2) The criteria of hypertension should be # not > for SBP and DBP.3) the definition of smoking is not usual because it included all exsmokers.

**Level of interest:** An article of outstanding merit and interest in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests