Reviewer’s report

Title: The ratio of AGE to sRAGE independently associated with albuminuria in hypertensive patients

Version: 0 Date: 11 Feb 2018

Reviewer: Antonino Dipino

Reviewer’s report:

Thank you for the possibility to review this manuscript. In this work the Authors explored the relationship between microalbuminuria and AGE/RAGE ratio in a population of hypertensive patients. The main findings the higher AGE levels and lower sRAGE levels in patients with microalbuminuria (without statistical significance), furthermore they found an independent association between microalbuminuria and AGE/RAGE ratio. Like the authors correctly explained the lack of a system that may discriminate between the different fractions of sRAGE is one of the major limitation of this study. Moreover, I believe that several points are needed to be clarified.1 - the authors measure AGE levels with a commercial ELISA kit. However, it is not clear what is the AGE that is identified with this method or the method identify all the serum glycated proteins. It is needed to clarify this point in the appropriate section. 2 - Page 9 line 2: what is the meaning of -701C?3 - a more detailed description of the study population is needed. In particular it is not clear is patients with diabetes are included in the study. Table 1 shows that in both groups there are subjects with prediabetes at least, according to fasting glucose levels. Diabetes is a key determinant of microalbuminuria and AGE/RAGE levels, so it is needed to clearly explain whether the population include or not subjects with any alteration of glucose homeostasis or taking hypoglycemic drugs. So, you should declare is you performed screening for diabetes (OGTT or HbA1c) in the population. If available, the prevalence of IFG, IGT and HbA1c levels should be provided. If the prevalence of patients with alteration of glucose homeostasis is high it is needed to adjust the multiple regression analysis according to presence/absence of prediabetes/diabetes. 4 - did the study include patients with previous cardiovascular events? If not, the results may not be applied in patients with previous CVD, this should be included in the study limitation.5 - the discussion is too long and it is not focused on the results of the present study, but on a review of literature in particular in the middle part. I believe that the discussion should be focused only on study results and difference and analogy with similar study. 6 - Statistical analyses: it is not clear the primary objective of the study and, accordingly, the calculation of the sample size. Is the primary objective the difference in AGE/RAGE ratio between the two groups? Please explain in the appropriate section.

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.
Yes

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

Yes

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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
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Needs some language corrections before being published

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