Reviewer's report

Title: Metabolic Markers and Microecological Characteristics of Tongue Coating in Patients with Chronic Gastritis

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Reviewer: Yoshihisa Yamashita

Reviewer's report:

1. General comment
The aim of the study to establish EBM in Traditional Chinese Medicine (TCM) is important and interesting. However, the reviewer is confusing with this manuscript, because the definition of the sample of tongue coating is obscure and interpretation of the results of LC-MS seems to be inadequate. Furthermore comparison of PCR-DGGE diagrams between gastritis group and healthy control group was unable to be evaluated because the method was not adequately described. In addition, the page number should be added for convenience to point out the problems in reviewing the paper. The paper is too preliminarily to be published in the scientific journals.

Specific comments
2. Page 5, lines 29-30: What type of cells did the authors count? How did the authors count the cell number after centrifugation and how did they adjust. Is the supernatant finally collected tongue coating sample? The reviewer can not image what is tongue coating samples.

3. Page 6, lines 2-7: What derived from tongue coating was analyzed by LC-MS? Washed solution of tongue coating cells was analyzed?

4. Page 8, lines 24-26 & Fig. 1: Figure 1 shows similar peak patterns between upper and lower panels. Although the authors insisted difference in the peak height, it was just 1.5 fold in this figure. Is there any significant difference between gastritis and control groups?

5. Page 11, Table 1: tR/min of compound is very closed, eg, 4.06, 4.07, and 4.08 for Vitamin D2, 3-Ketolacotse, and Metarhodopsin, respectively and 8.14 and 8.18 for Prostaglandin A2 and Leukotriene A4, respectively. On the other hand, two peak patterns in Fig. 1 showed 0.04 of difference in tR/min. How did the authors exactly align the different peak patterns? How much error did author accept when the peak patterns were aligned? No description concerned in this matter was found in this paper. This definition is critical for evaluating the results in Figs 2 and 3.

6. Page 12, Fig. 4: Label for left figure is not available. The reason why these samples were selected in this figure is not clear. What is M2? Anyhow the figures show no meaning.
7. Page 12, Figure 5: The authors emphasized 10 strips characteristics for discriminating between gastritis and control groups. However these strips seem to be individually specific. The reviewer is unable to feel consistency of these strips in gastritis or control group. In addition Chinese character is not appropriate for labeling the figure.

8. Page 13, Figure 6: There is no description for alignment of digitalized DGGE maps. This definition is critical for evaluating the results in Fig 6. The information on DGGE strips selection in PLSDA analysis should be also clarified. The authors should identify bacterial origins of DGGE strips with high VIP values in PLS-DA analysis in the same way with metabolomics.

**Level of interest:** An article of limited interest

**Quality of written English:** Not suitable for publication unless extensively edited

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