

## ***Interactive comment on “Investigation of new particle formation at the summit of Mt. Tai, China” by Ganglin Lv et al.***

### **Anonymous Referee #1**

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General comments: This study investigates the new particle formation at a high mountain site of China. Since there were limited studies on NPF at mountain site of China, this work can provide useful data about NPF at high altitude area of China. However, this manuscript is not well organized, and lack of necessary in-deep analysis. A lot of conclusions were made arbitrarily. Some statements were not persuasive. I suggest that the manuscript should be major revised before published in ACP.

Specific comments:

1. The language requires polished. I highly suggest that the authors ask a native speaker to edit this manuscript. Some sentences in this manuscript are too obscure to understand.
2. P1, Line 14, campaign I and II seem overlapped. Please check the campaign period.

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3. P1, Line 18, Mt. Tai CANNOT show larger formation rate. Besides “larger formation rate” is misleading. this work only studied less than one year NPF, it’s difficult to conclude the formation rate here is large. Considering the gas concentration, the FR for clean site should be lower.
4. P1, Line 22, what does “limited higher PM 2.5” mean here? Usually higher particle contribution inhabits the new particle formation. Anyway, I guess the authors were trying to say that during the relatively polluted days, the GR is higher? It’s because of the gas concentration, but has nothing to do with PM concentration.
5. P1, Line 20, what does “proxy” mean?
6. P1, Line 22, recombination? Do author want to say “coagulation”?
7. P1, Line 23, “haze” is inaccurate.
8. P2, Line 7, change “around” to “over”, what does “refer to” mean?
9. Session 2.1, more information about the site should be provided, e.g. the height of the site or inlet from ground.
10. P3 Line 25, “Its measurement range of . . .” should be rewritten.
11. Session 2.3.2, the sulfuric acid estimation method used here already has very large uncertainty. Besides, the accuracy of the radiation from HYSPLIT model is far from enough for sulfuric acid estimation.
12. Equation 6, the literatures here are old. For the current knowledge, sulfuric acid is considered to contribute to the nucleation, but negligibly to the particle growth. The authors should rethink about the discussion about this.
13. P5, Line 10, change “each” to “every”
14. Session 3.1, authors should analyze what controls the occurrence of NPF, source or sink?

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15. Fig. 1, y should be  $\text{SO}_2^*\text{OH}$  to present NPF source. Comparing  $\text{SO}_2$  with CS makes no sense.
16. Fig. 3, change the color bar so that one can see the “banana curve” clearly.
17. Session 3.2, because the sulfuric acid estimation has large uncertain, author should reconsider how to analyze this part.
18. P7, Line 22, this interpretation has no evidence. It's more from author's guess, not from data. There are a few interpretations like this, e.g. line 25.
19. Session 3.4,  $\text{PM}_{2.5}$  is not directly related to NPF. Again, it should be discussed that it is source or sink that control the occurrence of NPF.
20. Session 3.5, how to combine the NAIS and WPS data. Especially these two instruments have overlapped size range. Maybe this should be included in the experimental session
21. Session 3.6, the definition of “haze” here is unclear. Visibility is not a crucial criterion for haze. The PM concentrations are similar for so called haze and non-haze days. Can it be haze or fog? Also it seems redundant and reduplicated to session 3.5.

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