**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Regional and global modelling of aerosol optical properties with a size, composition, and mixing state resolved particle microphysics model" *by* F. Yu et al.

Anonymous Referee #2

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The manuscript by Yu, Luo and Ma presents the application of a very detailed aerosol microphysics scheme to both a global and a regional model. The authors present a detailed theoretical discussion about how aerosol optics change because of coating, particle size and particle composition, and compare the models' results with AOD retrievals.

Although this work is very interesting and well presented, it is extremely poorly validated. The authors fail to convince the reader why the application of such a sophisticated scheme improves the models in any way, given the very expensive scheme incorporated, compared with a very simple aerosol scheme with no aerosol microphysics.

Other than the theoretical discussion of aerosol optics (which lacks any type of validation), the authors only discuss AOD and hardly mention extremely interesting results that their models can provide, like how aerosol optics change with size distribution, mixing state, aerosol water, absorption amplification and, most importantly, compare them with measurements which are available. After having included a very CPU-expensive mechanism, their only comparison with measurements is AOD, which agrees "within a factor of 2" (summary, point 4). I see no improvement here when compared with even the simplest aerosol models used by the scientific community. This paper has a strong potential to become very interesting and innovative, but the authors refer from using their models' full potential. I suggest that the paper can be published in ACP only after having properly validated the models' optics with focus on the calculated aerosol microphysics, as well as addressing the following minor comments.

Specific comments

1) The second half of the abstract is confusing. I suggest rephrasing it to make it clearer.

2) Throughout the manuscript the expression "yyy coated on xxx" appears. I suggest changing it to either "xxx coated by yyy" or "yyy coat xxx".

3) The authors frequently mention that coated aerosols are not internally mixed. To my understanding, there can only be two mixing rules: external and internal. As a further division, the internal mixtures can be homogeneous, which is what the authors name as internal, but they can also be inhomogeneous, with coated aerosols being only one type of them.

4) Frequently in the manuscript the description of the figures is a repetition of the figure legend, especially after figure 6. The legend should describe the figure, but the text should analyze and explain it.

5) Page 95, line 26: Why not primary species as well?

6) Page 97, top: How do secondary organic and secondary inorganic aerosols being treated?

7) Page 98, line 1: I guess phase separation is assumed without interactions between the phases?

8) Page 98, top: The authors incorrectly use the term "coated" for hydrated aerosols that have a sea salt or POC cores, since they assume mixing and set the core to zero. It introduces confusion. See also comment #3.

9) Page 98, last line: "should cover" is too general. They do, or not in this study? If not, what is the solution/suggestion/caveat?

10) Page 99, line 3: why not sea salt as well?

11) Page 99, lines 4-5, and table 2: if coating is being ignored, the term "core" should not be used.

12) Figure 1: why use wavelength as the x-axis and not size parameter?

13) Page 101, lines 18-20: references are missing.

14) Page 102, line 1: "internally mixed" should be "homogeneously internally mixed". See also comment #3.

15) Page 102, middle and page 106, lines 25-26: The discussion about bins and log-normals is confusing. One would expect that a modal representation has lognormals, and a bin parameterization has monodisperse particle populations at each bin. If the approach is different, it should be explained.

16) Page 102, line 24: "explicitly simulated" should be briefly explained.

17) Page 102, lines 26-28: Is this consistent with ISORROPIA II? Why not use aerosol water from ISORROPIA II?

18) Figures 3 and 4: Why BC is coated over SE Asia but not over tropical forests?

Competition with POC? Any experimental evidence for such a difference?

19) Page 104, last paragraph: The kappa values used for the single components should be mentioned, either in the text or in a table.

20) Page 109, line 24: How far apart are these stations?

21) Page 109, bottom: Is the model having clouds as well at the times no measurements are available due to clouds?

22) Page 110, line 15: Are the boreal forest conditions representative for the NE US?

23) Page 110, second half: A comment about the validation of the nucleation scheme is needed.

24) Page 112, line 6: "Analysis" should be "Theoretical analysis".

25) Pate 112, line 9: I suggest starting the sentence by "Model results suggest that . . .".

26) Figure 1: What is the chemical composition of the shell? Adding the coarse dust in the figure can be interesting.

27) Figure 7a is too busy. I suggest splitting it in 3.

28) Figure 7b: Adding CF<25% can be interesting.

Technical corrections

1) Page 96, line 7: "inter" should be "intra".

2) Page 103, line 8: "partition theory" should be "partitioning theory".

3) Page 105, line 5: "main continents" should be "continents".

4) Figure 2 legend: First word should be "Dependence".

5) Figure 9 legend: "overlapped" should be "overlayed".