Geoscientific
Model Development
Discussions

Interactive comment on "An aerosol activation metamodel of v1.2.0 of the pyrcel cloud parcel model: Development and offline assessment for use in an aerosol-climate model" *by* Daniel Rothenberg and Chien Wang

Anonymous Referee #2

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This study describes the development and application of an emulator for aerosol activation in an aerosol-climate model. Given that aerosol/cloud interactions are among the largest uncertainties in current estimates of radiative forcing, any progress in their accurate representations in global models is welcome. The current study certainly contributes to this; however, several changes are needed in order to streamline and clarify the manuscript.

Major comments

1) Structure/Length of the manuscript

The manuscript is rather lengthy and reads in several section like a thesis, including a lot of 'common knowledge' material and information that can be found in standard textbooks. Many sections can be shortened so that the main focus of the study is more pronounced and trivial information is removed.

a) For example, in the introduction many activation studies are described that consider effects such as surface-tension reduction or kinetic growth that are irrelevant to the current study.

b) Another example is the lengthy description of the iteration finding the dominant modes (Section 2.3). There are many sensitivity studies that have shown that size is the most important parameter influencing CCN (and therefore droplet) number. Therefore, the discussion of the small role of the nucleation mode is redundant.

c) In general, several parts are repetitive and can be merged or omitted as they are trivial and not new. One example is the beginning of Section 3.3 (p. 11, l. 23-30) that can be summarized in one sentence. Another one is the description of the sensitivity test at the end of Section 2.3 which are a direct result of Koehler theory.

2) Comparison to other activation schemes

The comparison of the emulator to the other aerosol activation schemes seems a bit 'unfair', or at least not clear. The activation schemes by Abdul-Razzak/Ghan (ARG) and Morales Betancourt/Nenes (MBN) were developed based on fits to parcel model simulations. Only if all the initial parcel models use the same input parameters, e.g. giant CCN (gCCN), the fits will be appropriate in matching such extreme situations. The text on p. 18, l. 14 suggests that this was not the case but this should be brought up earlier.

3) Features of the aerosol size distribution in MARC

It seems that the MARC model includes many features that are not explicitly described here but might have an impact on the aerosol activation. For example, the variable hy-

groscopicity of the MOS mode is mentioned in Section 2 (p. 5, l. 16) but the processes that lead to it are not further described. What is the extent to which hygroscopicity changes? How does it compare to treatment in other models?

4) Previous work by the same authors

It is clear that the current study is a follow-up study of the previous study by the same authors (Rothenberg and Wang, 2016). However, without reading this previous paper, it is not evident which parts are actually new here and which is merely a repetition of the previous work. Clarifying this would also likely help to shorten and focus this manuscript.

5) Role of gCCNs

There are several prior studies that explored the role of gCCNs for cloud properties, and also highlighting that they have largest effects in clean environments, e.g. Feingold et al., J. Atmos. Sci., 1999; Yin et al., Atmos. Environm., 2000). Therefore, the results in Fig 10 are not that surprising (p. 17, l. 7) and should be accordingly put into context of earlier studies.

6) Applicability of the emulator

a) It is shown that the emulator performs worst for extreme cases, e.g. for very clean conditions. The reason for this is ascribed to the fact that it was not trained for these conditions. However, it seems trivial to extend the parameter range of the parcel model to cover such conditions. On the other hand, some more discussion should be given for the reasoning of less extreme conditions and therefore the applicability of the emulator for most conditions globally.

b) The current study represents the next step of the previous study, namely the implementation of the emulator in an aerosol climate model. However, the computational benefits should be discussed more in comparison to previous activation schemes.

Minor comments

p. 1, l. 3-4: Many recent model studies have aimed at reducing this 'ever-increasing complexity' by reducing the number of bins and/or making simplifying assumptions on aerosol composition (e.g., kappa Koehler theory, omission of surface tension reduction as it has been shown to be negligible etc).

p. 2, l. 17; and p. 18, l. 35: 'processes' does not seem the right word here since e.g. surface tension reduction is not a process but rather an effect.

p. 6, l. 18: What is the value of the artificial cap?

p. 8, l. 30, and several other places: The condensation of water vapor onto particles/droplets is the main factor that occurs during the adiabatic ascent of an air parcel. The latent heat release is only a minor driving force of the condensation growth.

p. 10, l. 18: What are 'proto-cloud droplets'?

p. 10, l. 19, and l. 27: 'hydrated' usually refers to a chemical reaction where water is a reactant and its reaction results in a different product (e.g. hydrated aldehydes). Better here, use 'interstitial haze particles which have taken up some water according to their hygroscopic mass' (or similar).

p. 12, l. 25/6: This statement requires an explanation. Does it imply that kinetic limitations on growth bias the 'true results' and therefore should not be included?

p. 15, l. 29: How can a model underpredict by more than 100%? Wouldn't that result in a negative concentration? Clarify.

p. 17, l. 29: 'a chain of physics which ultimately leads to the aerosol indirect effect on climate' is very colloquial and vague. Rephrase.

p. 18, l. 27: It is not clear here what Gantt et al compared causing a difference of -0.9 W m-2.

p. 19, l. 1: 'myriad facts' is very vague and should be specified.

p. 19, l. 5: This is the first time in the manuscript that it is mentioned that there is a discrepancy between satellite-derived and model-predicted cloud droplet number. In order to pose the problem, this should be mentioned in the introduction, together with appropriate references.

Figure 1: In order to show the scale more clearly, you might consider a gap on the y-axis (e.g. from 2-6)

Figure 3: The figure seems very trivial and repeats text book knowledge. In the process of shortening the manuscript, it can be omitted and just described by a sentence.

Figure 4: Define DST01, MBS, MOS and ACC in the caption.

Figure 5: 'One-one plot comparing' can be omitted.

Technical comments

p. 2, l. 2 and at several other places in the manuscript: 'Aerosol' is singular and should be followed by a verb in singular form. Alternatively, use 'aerosol particles' which is more accurate anyway.

p. 2, l. 18: replace 'change sin' by 'changes in'

p. 2, l. 19: Not clear what 'their' refers to – to 'parcel model calculations' or 'global models'

p. 3, l. 10: Remove 'J.-M.'

p. 5, l. 6: Aitken

p. 5, l. 17: replace 'forms' by 'form'

p. 5, l. 25: 'from aerosol' is redundant

p. 6, l. 33: define 'u(g)' here

p. 10, l. 10: If you decide to include these equations (which is not necessarily needed;

a simple reference to a standard text book such as Pruppacher and Klett would be sufficient), write them as equations in separate lines.

p. 12, l. 21/22: The last part of this sentence seems incomplete; 'activate' might be missing at the end.

p. 15, l. 34: Period missing after 'range'.

p. 17, l. 23: a mean relative error

Reference list: I believe that GMD does not require the full URL for papers when the DOI is listed.

Morales Betancourt and Nenes, 2014b should be cited as their GMD (not GMDDiscussion) paper.