Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-147-RC2, 2019

Geoscientific Model Development Discussions

Interactive comment on "Explicit aerosol-cloud interaction in the Dutch Atmospheric Large-Eddy Simulation model DALES4.1-M7" *by* Marco de Bruine et al.

Anonymous Referee #2

Received and published: 7 August 2019

This manuscript describes a new version of the Dutch Atmospheric Large-Eddy Simulation model (DALES), which is supplemented with an interactive modal aerosol scheme. Two different cloud activation approaches are also compared. Aerosol-cloud-precipitation interactions are examined and different processes are quantified using aerosol mass as a reference. The manuscript is well-written using good English language and it is also within the scope of the Geoscientific Model Development. There were one possible bug and another possible problem with implementation, which could have an effect on results. When these issues are solved, the manuscript is suitable for publication.

General comments

Aerosol-cloud interaction (ACI) is a rather general term, so quite often this could be replaced by a more specific term. For example, "The feedback of ACI on the aerosol population" (page 1, line 4) could be just "The impact of cloud processing on the aerosol population" and the same term could be used also here "Whether ACI increases or decreases the average aerosol size" (page 1, line 16). Please check the whole manuscript.

Using the saturation adjustment method (diagnostic cloud water) and assuming a fixed value for supersaturation when calculating cloud activation are significant approximations. Their effects should be at least explained here instead of investigating these in the future (page 7, line 13). Can you really examine aerosol-cloud interactions without explicitly modeling aerosol condensational growth and subsequent cloud activation (prognostic cloud water)? What is the added value of detailed aerosol chemical composition when cloud activation is so much simplified?

Why did the "runaway activation" (page 8, line 7) were allowed only for the PN activation scheme? For me this looks like a possible reason for the observation that aerosol fluxes for activation (and cloud evaporation) are 12-13 times larger for PN simulations compared with those from KAPPA simulation. This difference is later used as an explanation for several other differences between simulation results. If the difference between activation schemes is related to a technical/numerical reason, then it should be considered as a bug and fixed.

Validating simulations against observations is not as straightforward as expected in this work (e.g. page 10, line 2). LES inputs (aerosol size distributions and composition, atmospheric variables, etc.) are not fully synchronized with the cloud and rain observations, so one-to-one comparison is not fair. I would recommend reformulating/removing all such direct comparisons.

Specific Comments

P7, Eq. 5: This equation is not valid for hygroscopicity parameter, because some species-specific hygroscopicity parameters are zeros. Did you really used this equation (and how)? This equation can give unrealistic hygroscopicity parameter values (divide by zero) and in that case all calculations should be updated. The correct way to calculate the mode mean hygroscopicity parameters is volume fraction weighted average.

P12, L27: Maybe the above-mentioned possible bug in hygroscopicity parameter could explain why KAPPA simulations produce much lower cloud droplet number concentration (CDNC) compared with that from the PN simulation? Many other explanations are based on this difference in CDNC (e.g. page 14, line 15-), so a clear explanation is required in any case. Also, why the interactively calculated CDNCs are so low compared with the available aerosol concentration, and why CDNC seems to be independent of the selected cloud supersaturation? Why does CDNC from the PN simulation decrease with altitude?

P14, L30 "None of the simulations scores best on all metrics. . .": direct comparison of observations and LES simulations is not that simple, but if observations were considered as the truth, would the new KAPPA framework be far from best? Although diagnostic cloud water is accurately predicted, it fails to predict cloud droplet number.

P16, L13: Aerosol fluxes for activation (and cloud evaporation) are 12-13 times larger in PN simulation compared with those in KAPPA simulation. The given explanation is based on different autoconversion strengths so that in the KAPPA simulation a larger fraction of cloud water becomes rain before evaporation and is therefore not counted as cloud evaporation, right? If this is the reason, then why cloud-to-rain conversion process strengths are so similar? At least for me, this looks more like a bug than a physically realistic process (see the related general comment). Because meteorology is similar for both PN and KAPPA simulations, there is no physical reason for the large

difference between cloud activation fluxes.

P18, L32-: Average median radius of activated aerosols are different for the KAPPA and PN simulations, and the explanation is related to "stronger cycling of aerosol through the clouds in the PN simulation". What about the effect of supersaturation? It is fixed (0.4%) for KAPPA, but depends on updraft velocity for PN. Lower supersaturation in the PN case could explain the difference in median radius.

Technical corrections

P1, L3: "feedback between clouds"?

P1, L10: "in this pristine ocean environment virtually all aerosols enter" - not all aerosols, but those that activate, right?

P2, L17: "which influence further ACI"

P2, L25: Please clarify "bulk" and "numerical" methods.

P2, L33: There is also an ECHAM version with SALSA microphysics.

P2, L34: Why "However" here?

P5, L14: "of the originating free aerosol mode"

P6, L19: S is saturation ratio, right?

P11, Fig. 2: The unit of sea salt mass concentration is more likely micro than milligrams per cubic meter. Also, would it be possible to separate clouds and precipitation or otherwise indicate cloud base height to the vertical cross section?

P11, L7: "κ-KAPPA"

P13, Fig. 3 (and Fig. 4): Altitude range could be increased to show also cloud tops.

P17, L11-12: Unclear sentence

P19, Table 5 and related text: Maybe 1 nm accuracy would be good enough?

P27 -: Journal names should be abbreviated

P28, L25: Manuscript is already published in GMD.

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