Geosci. Model Dev. Discuss., 7, C1214–C1218, 2014
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Geoscientific Model Development Discussions
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Interactive comment on "A new WRF-Chem treatment for studying regional scale impacts of cloud-aerosol interactions in parameterized cumuli" *by* L. K. Berg et al.

Anonymous Referee #2

Received and published: 22 July 2014

In the manuscript "A new WRF-Chem treatment for studying regional scale impacts of cloud-aerosol interactions in parameterized cumuli" by L. K. Berg et al., the authors present a new treatment of cloud-aerosol interactions within parameterized shallow and deep convection. After the introduction they present the modifications made to WRF-Chem for the implemented representation of cloud aerosol interactions in sub-grid convective clouds. Following the WRF-Chem setup, emissions and data description they present an analysis of the new parameterization for the 25 June 2007 for three different areas in the US, which represent deep, shallow and no convection. Finally, they compare the chemical composition of cloud droplets with observations from aircraft measurements.

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Unfortunately, the testing of the new treatment is not sufficient in my opinion. The authors should compare their results more with other well-known test cases, other models and more measurements, instead of comparing the results using the modified version of WRF-Chem with the standard WRF-Chem for a single day in 2007. Also, after reading this manuscript, I have the impression that this is very preliminary and additional work is needed. Additionally, more justification is needed, when important processes for the cloud-aerosol interaction are neglected.

Therefore, I recommend not to publish the manuscript in its present form.

Comments

Abstract

P2652, line 12ff: "Preliminary testing of the modified WRF-Chem has been completed using observations from the Cumulus Humilis Aerosol Processing Study (CHAPS) as well as a high-resolution simulation that does not include parameterized convection."
I don't think that "preliminary testing" is enough for a publication regarding a new cloud-aerosol treatment.

Introduction

P2654, line 24ff: It should be noted, however, that the modifications do not include feedbacks of aerosol on the amount of precipitation, impacts of the aerosol on the cumulus microphysics, or feedbacks between the cumulus microphysics and the radiation. These additions are topics for subsequent research."
For a new treatment of cloud-aerosol interactions, I find it crucial to have at least the feedback of aerosol on microphysics and precipitation included. Otherwise, it is hard for me to speak of cloud-aerosol "interactions". In my opinion, the authors should first implement the complete interactions, which are planned for this parameterization, before publishing.

2.2.1 Aerosol effects on the sub-grid cumulus

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P 2660, line 5f: “The activation is largely a function of the cloud updraft speed.”
But it is also a non-negligible function of the aerosol concentration. Therefore, I am surprised that the concentration and chemical composition of aerosol particles is not included for the activation of cloud droplets.

Line 12ff: “Once the droplet number concentrations are computed for each perturbation value of temperature and humidity in the PDF, they are averaged together to provide a single value of cloud droplet number concentration for each grid cell.”
Different cloud droplet number concentrations can have a significant influence on the subsequent development of a cloud. Therefore, I would imagine, that averaging the cloud droplet concentrations over the different profiles, would end up in loosing helpful information. Additionally, why are the perturbations not averaged? After that only one cloud droplet concentration has to be calculated, which would be faster concerning computational time?

Line 18: “At present, secondary activation is not considered for the sub-grid convective clouds, nor does the activation feedback on the cumulus clouds via changes in the conversion of cloud water to rain”
This is also an important process, when studying the interactions of aerosols and clouds. Therefore, I recommend, including this process as well in a final version of this parameterization.

P2661, line 3f : “passive clouds (for which the only processes are activation/resuspension and aqueous chemistry).”
Earlier, it is stated that for passive clouds the vertical velocity is set to zero. Is it not inconsistent to have activation, when no updraft is present?

P2662, line 8ff: “In the cumulus-effects-on-aerosols routine, calculations are made using the properties of an average (over the population) shallow cloud, rather than doing calculations for each shallow cloud in the population.”
Can the authors explain, why this is the case? What is the advantage of using an

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average cloud rather than a population of clouds?

P2663, line 13: “Aerosol activation is calculated as described in Sect. 2.2.1, but for shallow convective clouds, the average (over different clouds) vertical velocity is used.” Since the activation is strongly dependent on the vertical velocity, is it justified to use an average vertical velocity? Would the results differ, if the an average cloud droplet concentration is calculated based on the different vertical velocities?

P2663 line 26ff: “Cloud water can also be converted to cloud ice, but currently this is not included as part of the aerosol wet removal, as the fate of cloud ice (conversion to precipitation or detrainment near cloud top) can vary. In the future, ice processes could be incorporated in the cumulus effects routine by treating cloud-ice-borne aerosol in addition to cloud-droplet-borne aerosol.”
This is also a strong simplification. One could assume that for high cloud droplet concentration the formation of precipitation is delayed and hence cloud water is transformed into cloud ice. Is it justified, to neglect this effect?

Page 2665, line 24ff: “Because vertical velocity is assumed zero in the passive clouds, the Abdul-Razzak and Ghan (2002) parameterization cannot be used. Instead, we assume that the activated fraction for each aerosol chemical component (and size bin) is the same as the activated fraction in the steady-state updraft of the active cumulus.” Why do you need activation, when the updraft is set to zero?

5 Analyses

It is not clear to me, how the analysis of this specific day justifies your conclusion that your new treatment works properly. The model modifications clearly change your results compared to the WRF reference simulations. However, no comparisons are made with other models. Maybe this would give more insight on how the new WRF-Chem treatment of cloud-aerosol performs. Also, the analysis is quite long, while the outcome is rather low.

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Additionally to the comparison of the CHAPS date, I would suggest to use more measurement data for a more quantitative assessment of the model performance.

P 2676, line 5ff: "Some differences between the low resolution and high-resolution simulations are likely due to the averaging of the emissions over larger grid cells that produce smaller horizontal gradients in emissions that could lead to systematic differences in the aerosol loading. There are also differences in the simulated cloud field. For example, the grid-resolved simulations were free of deep convection (i.e., grid resolved clouds that one would interpret as deep convection) within the OKC analysis box while the low-resolution simulations presented here predicted a large amount of deep convection in the same box (not shown). . ..."
These are quite significant differences. Therefore, I would rather not compare these results to Shrivastava et al. (2013) for a manuscript, which deals with the evaluation of a new model modification.

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