Review ACP journal

Below-cloud scavenging of aerosol by rain: A review of numerical modelling approaches and sensitivity simulations with mineral dust

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1 Overview

1.1 Topic and relevance

The manuscript shows a set of sensitivity studies of below-cloud scavenging (BSC) modelling schemes for the case of mineral dust. Given the current challenges to converge both: empirical studies and modelling approaches, and the potential impact of improvements in the global climate models, the presented study is relevant enough to be published in Atmospheric Chemistry and Physics (ACP) journal.

1.2 Evaluation

The manuscript is well written, with an useful set of results, and providing a review with a substantial amount of information about both, BCS models and the Met Office's Unified Model (UM) set up. I recommend to **accept the paper with minor revisions**. Below the authors will find few suggestions organised by sections of the paper after two minor general comments.

1.3 General comments

- Although somehow the information is already included in **Table 2**, for the mineral dust community might be useful to see role of each BSC scheme in the estimations of wet deposition fluxes in [Tg yr^{-1}]. For example, in (Shao et al., 2011) and (R. Checa-Garcia et al., 2021) the estimations of wet deposition fluxes in [Tg yr^{-1}] show large differences between models, but also the ratio Dry/Wet deposition.

- Note that for mineral dust emissions using nudged surface wind speeds from reanalysis increase the consistency between models (vs winds fields from each model). Here is not much relevant because it is a single model analysis but, anyway, it is worth to mention which wind fields are the authors considering as dust burden and dust emission can be conditioned by this fact.

2 Comments/Suggestions

2.1 Title

The title is descriptive but, given that the sensitivity studies rely on Unified Model, I wonder if the authors would consider to add this information in the title (it's not a requirement, just an idea)

2.2 Abstract

- **Line 21**: Maybe a ',' before while

- **Line 25-26**: This fact is not covered by the title.

2.3 Introduction

- **Line 85**: Given that the paper is a kind of review paper, with a wider spectrum of possible readers, I would double check about consistency in nomenclature. Sometime it is used BCS rates, sometimes Λ, others BSC coefficient.
- **Line 130**: Maybe the authors can comment already here about single vs double moment approach in UM in consistency with KQ3 and KQ4. Is the *mode merging* in the UM introduced inside the BSC scheme? In other words, is it separated from other processes like sedimentation, for example? I understand that the idea is to compare simulation with and without *mode merging* but only for BSC (not other removal mechanisms).

2.4 Section 2

Here when authors describe the equations, remember that mineral dust are not usually spherical, so d_p represents (probably) an effective diameter. For large raindrops, the shape is also not spherical (as you commented in the paper there are oscillations), so also D_p would represent an effective diameter.

- **Line 152**: BSC coefficient vs BSC rate?
- **Line 158**: Here I would add something like: involving two reasonable approximations one for diameters $D_p >> d_p$ and one for falling velocities $v_t(D_p) >> v_t(d_p)$.
- **Line 160**: Note that here you write $E(d_p, D_p)$ and in other sections $E(D_p, d_p)$.
- **Equation 3.a**: Probably is $N_0 e^{-\lambda D_d}$ (λ not k).
- **Line 174**: rain droplet vs raindrops
- **Line 174**: The (Abel et al., 2012) new raindrop size distribution (DSD) seems to have more impact in low rainfall rate surface precipitation rather than larger rainfall rate. So, are the differences between (Abel et al., 2012) and previous DSD parameterization an important factor for the BSC rates? Or this only means a second order factor of discrepancies? In other words: it is likely that few models are using older parametrizations, even using the Marshall-Palmer (1948) model. Would be this an important aspect?
- **Note Section 2.2**: I would refer now (in the main paper) to the Table S2 in the supplement.
- **Note Section 2.3**: Here I would mention that later on you refer as **phoresis** the join role of all these three collection efficiencies; or the section title can be **Phoresis: ...** to be clear.
- **Equation 12**: Here it might be more readable to include the two cases like:

$$E_{rc}(d_p, D_p) = \begin{cases} ... & 20 \leq R_{e,D} \leq 800 \\ 0 & \text{otherwise} \end{cases}$$

2.5 Section 3

- **Line 315**: Arbitrary -> idealised?
- **Line 317**: I would add in the caption of the figures that it was a box-model simulation.

2.6 Section 4

- **Line 341**: For me it is unclear if both aerosols and chemistry are evaluated once per model hour or only chemistry (later on you mention about time-steps of 30 min/15 min for deposition)
- **Table 1**: As commented before, please explain that **phoresis** means all the processes described in section 2.3.

2.7 Section 5

- **Figure 2**: I understood that this figure shows the X, for which $E_X(d_p, D_p)$ has the highest values (either because X is more important or because the other mechanism decrease). Is this right? or **dominant** means significant larger contribution than other processes?

- **Line 462**: If here the authors are using the box-model simulations it would be worth to remember.

- **Line 484/Figure 4**: So I understood Λ from (eq.2) and $\Lambda_{N,M}$ for (eq. 4 and eq.5) for a distribution with **geometric** median diameter and given σ?

- **Figure 3**: The parameterization of Wang shown in Figure 3 seems to me consistent with the figure 3 of the original paper of Wang (2014), as they considered one semi-empirical model with **phoresis**, i.e. from (Andronache et al., 2006), (but not rear-capture) it seems that in their estimations **phoresis** is only relevant in higher rainfall rates. Regarding the **Laasko model**, the results seems to me reasonable as somehow they have a linear dependence with rainfall rate in their parameterization (eq. 14b).

- **Figure 7**: This is a very interesting figure with a lot of information. However, if I understood well the observations of dust optical depths (DOD) of AERONET are mostly located at Sahel region and represented by + , and the DOD of (**Kok2021**) are regional averages over close-to-sources regions represented by circles. Is that right? Then (**Kok2021**) are not observations but a **constrained multi-model by observations**. In the figure it is not so easy to see the +. It would be great to have open circles for Southern hemisphere in (d), (e) and (f). This will help the reader to understand better the results.

- **Figure 8**: This figure is also interesting, as expected the inclusion of **phoresis** and **rear-capture** decrease the optical depths and surface concentrations. It is more difficult to detect differences in dust deposition, which might be because we have to signals here: wet and dry deposition. It would be interesting to compare with specific measurements of wet deposition flux (for example, Marticorena et al., 2017).

- **Figure 11**: Here like in Figure 7, it would be good to use open circles for Southern hemisphere.

2.8 Supplementary information

- **Note 1**: consider **raindrop** instead of **rain droplet**, in the precipitation community is used raindrop-size distribution rather than droplet (it is common cloud droplet but not cloud drop).

- **Note 2**: note that Eq.S23 is correct but often are considered two cutoffs and the integral is expressed by:

$$R = C \int_{D_{min}}^{D^{max}} D^3 N(D) v(D) dD$$

this is because drops smaller than a D_{min} are not falling, and drops larger than D_{max} are eventually broken into smaller drops (or not even formed) and this can have a effect in the rainfall properties, for example (Checa-Garcia et al., 2014).

References

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