**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

Interactive comment on "Technical Note: simulation of detailed aerosol chemistry on the global scale using MECCA-AERO" *by* A. Kerkweg et al.

Anonymous Referee #3

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1. General comments

The paper describes the tool MECCA-AERO which is designed for simulating aerosol chemistry on the global scale. The integration of the module into a global model framework is explained. Interactions with other modules of the global model system are discussed. The results of the new model system are compared to the results of an aerosol dynamical model neglecting aerosol chemistry.

MECCA-AERO is an important supplement to current global aerosol models which suffer from neglecting aerosol chemistry. The methods applied by the authors mostly seem to be sound and are clearly explained in most cases. The paper is clearly written

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and well structured. I would recommend publication after some modifications outlined below.

2. Specific comments

Major points

p. 3305, line 20 - p. 3306, line 9; as well as section 2.2:
The overlap problem may occur also in the case of other compounds, such as nitrate which is considered by several ADMs. Why do the authors focus on sulfate instead of describing the problem in a more generalized manner? It should also be clarified, why the overlap is necessary. As shown in Fig. 1, many processes (such as advection, deposition, or scavenging) are considered in both modules (MECCA-AERO and ADM). This could be avoided by simply calling the aerosol microphysics submodule (Fig. 1) from MECCA-AERO.

Section 2.3:
The authors should explain in more detail why the effect of operator splitting on sea salt composition is by far largest for dry deposition. Is this just an expectation or a result of analysing model output? I would expect that sedimentation (as the authors admit some sentences above) or wet deposition is most relevant since sea salt particles mostly are large (sea salt mass is dominated by large particles). Particles of that size are mainly subject to sedimentation and also wet deposition rather than dry deposition.

Minor points

p. 3302, lines 18-19:
Amundson et al provide the description of a model component rather than a global aerosol modelling study. Hence this reference differs from the others which all refer to global aerosol modelling studies. It should be skipped or other studies describing only

'components' should be added (e.g., the Metzger et al work mentioned below)

p. 3303, line 3:
Replace 'dynamical aerosol model' by 'aerosol dynamical model'

p. 3303, lines 8-10:
These examples are science topics but no 'scientific questions' as stated before.

p. 3303, line 11:
Replace 'dynamics and microphysics' by 'dynamics/microphysics' since the two terms are used as synonyms.

p. 3303, lines 18-20:
Amundsen et al, Metzger et al, and Topping et al do not describe global models. They focus on global model components. Hence this reference list is misleading.

p.3303, lines 20-21:
A study with a model of similar capabilities was already performed by Jacobson 2002 (J. Geophys. Res., 107, D19, 4410, doi:10.1029/2001JD001376, 2002).

p. 3309, line 8:
It should be specified what 'identification' means in this context.

p. 3309, line 16:
A reference should be given for this statement.

p. 3310, line 20:
Fig. 1 should be mentioned much earlier, not at the end of the paragraph.

Section 2.3:
The authors should explain in more detail why $F_{dep}(X)$ and $F_{dep}(SS) \times f(X)$ are different. Is it just because of the different composition of aged and fresh particles or also because of their size distribution?

Eq. 11:

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Is F_{corr} the corrected flux or the correction to be added to the flux previously calculated? I would expect $F_{realdep}$ to be the corrected flux and F_{corr} to be the correction.

p. 3315, lines 9-10:
The authors should discuss which solver was used in this original MECCA-AERO version. It should be discussed why this solver cannot be used in the present version and why it had to be exchanged by the solver (ROS3) which causes problems. Is it because of the application of MECCA-AERO in a global model?

p. 3316, lines 11-14:
The authors should evaluate the consequences of this assumption. If LWCs below this threshold are not relevant for atmospheric chemistry the consequences are negligible. If they are relevant, it should be discussed whether the assumption is reasonable.

3. Technical corrections:

p. 3305, line 1:
Remove 'a'.

p. 3306, line 17:
Replace subscript aq by '(aq)'

p. 3307, line 20:
Replace 'mode' by 'model'

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3301, 2007.

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