

Interactive comment on “Modeling sea-salt aerosol in a coupled climate and sectional microphysical model: mass, optical depth and number concentration” by T. Fan and O. B. Toon

Anonymous Referee #1

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In this paper, the authors identify weaknesses in existing sea-salt aerosol source functions, and evaluate a new hybrid source function that is able to represent number, mass and optical properties in a global microphysical aerosol model. They identify suitable observational datasets with which to compare the new source function, and benchmark the model's performance. The authors also identify the importance of having well constrained removal parameters in order to accurately simulate aerosol properties.

The manuscript is well written and structured, though quite lengthy.

I have two main comments. The first is that the simulations presented lack other aerosol species, in particular sulfate, when the vast majority of other microphysical aerosol

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models routinely represent them. This seems to negate somewhat the advantages of using a microphysical model, as coagulation and condensation are ignored. These may be important in some instances for growing sub-CCN size aerosol to CCN sizes, with implications for removal (and therefore mass, number and optical properties). The lack of other aerosol species also drastically limits the number of observations that can be used to evaluate the model.

My other comment would be that the manuscript may be better suited to Geoscientific Model Development than Atmospheric Chemistry and Physics, as it evaluates a recent (hybrid) parameterisation rather than fundamentally changing the way we think about sea-salt parameterisations. Nevertheless, the work is a useful contribution, and I would recommend publication after consideration of the points listed below.

Specific comments

Particle swelling: may be more accurately described as hygroscopic growth?

p24502, l8: ‘...we used...’, recommend using the present tense when describing your study and its results.

Is there a reference for the CARMA model? Or is this the first time it has been described? If the latter is the case, it should be stated.

It is quite odd that a complex microphysical aerosol model should neglect treatment of other aerosol species, especially sulfate and its precursors. I think some comments on why other components are neglected (not yet available within CARMA?), the potential impact this may have, and the limitations this imposes on the results and discussion, is necessary before the manuscript can be published. Even over remote oceans, where there may be little anthropogenic influence, emission of DMS can contribute significantly to sulfate aerosol.

p24503, l19: ‘Coagulation is not considered in the model since the low number concentrations and short lifetime of SSA indicate that it is not an important process’. Is this

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true even at ultrafine sizes of SSA, where concentrations may be considerably higher?

p24504, last paragraph: could add couple of sentences in here describing how number fluxes shown in Fig. 1 vary with wind speed.

p24514, l8: Does assumption of 100% of SSA residing in cloud water not mean that removal is overestimated, particularly in number? According to Fig. 1, there is emission of sea-salt aerosol down to 0.01 μm radius. These aerosol are too small to be activated (e.g. at supersaturation of 0.1%), and therefore presumably should also not be nucleation scavenged. This would clearly have a dramatic impact on the number size distribution. Perhaps this assumption is necessary as there is no coagulation nor condensation (due to lack of other aerosol species) represented in your simulations. This is perhaps a key limitation in the assessment of the ability of the model to reproduce number concentrations and the size distribution at smaller aerosol radii.

p24515, l10: 'Solubility factor'. I don't think this parameter has very much to do with solubility, and should therefore be renamed. As the authors state in the sentence before, the assumption is that all SSA are in cloud water for the purposes of nucleation scavenging, and therefore that all aerosol are soluble. Perhaps a better term might be 'cloud to rainwater conversion factor'. Presumably this factor is per timestep?

p24514, l19: That lifetimes are tuned also means that burdens and removal are tuned, which has implications for the conclusions about how well the source parameterisations reproduce the observations.

p24515, l7: It would be useful to describe how the Savoie & Prospero (1977) results were obtained. Are they indicative of sea-salt mass only, or likely to be influenced by other aerosol components?

p24515, l25, and Fig. 6: I think it would also be useful to show the Gong results in this plot. How well Gong reproduces the seasonal variation should also be commented on.

Section 3.2.2: The implications (for the CMS parameterisation / model) of this section

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are not clear to me. My impression from the text is that, under certain conditions, optical depth from sea-salt can be estimated directly from wind speed. This then gives two estimates of optical depth (the other calculated in the model). The two estimates may give similar results, but I don't think this comparison can be used to evaluate model skill.

p24523, l9: 'This mode is probably due to sulfate and organic aerosols from the oceans or pollution aerosols that are not represented in the model.' I agree, though does this also have implications for Sect. 3.2.1, 'Comparison with AERONET optical depth'?

Fig. 14: The mode from sulfate and organic aerosol in Fig. 14 may be reduced in winter, when organic emissions including DMS are likely to be at their minimum. Could you also show a plot for December or January, and comment on this?

p24525, l1: It is also possible that these 'uncertainties' may alter the shape of the size distribution also.

p24526, l17 and Fig. 18: I'm not sure that this is a new result, it has been well described before. This paragraph and figure could be removed.

p24527, l5: 'The high optical depths near Peru are due to the effect of the Andes Mountains on the NCEP wind field.' This is interesting, has this been described / investigated anywhere?

Technical corrections

p24500, l6: 'We aimed at finding...' should be 'We aimed to find...'

p24500, l11: 'the research...' should be 'this research...'

p24500, l24: Sea-salt or sea salt? Need to be consistent with hyphenation throughout.

p24501, l14: fine-mode and ultrafine-mode, specify sizes

p24501, l16: 'SSA particles activate...' should be 'SSA particles can serve...'

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p24501, l21: '...as small as 0.01 um...', radius or diameter?

p24503, l7: 'advective' should be 'advection'.

p24503, l14: '2° × 2.5°', state which is longitude and which is latitude.

p24504, l13: 'They are...' should be 'There are...'

p24505, l6: 'Martensson et al' should be 'Mårtensson et al', and elsewhere

p24505, l29: 'stands for' should be 'is'

p24505, l29: 'However, as shown in Fig. 1, Caffrey et al., (2006)'s number flux is about one magnitude higher below 0.1 um compared to Clarke et al. (2006).' Suggest clarifying to 'However, as shown in Fig. 1, below 0.1 um Caffrey et al., (2006)'s number flux is about one magnitude higher than Clarke et al. (2006).'

p24506, l11: 'see' should be 'show'

p24506, l14: 'fit demands' should be 'fit the demands'.

p24508, l7: Suggest deletion of 'therefore',

p24508, l8: Sentence 'The wind field in CAM...' is repeated information. Suggest that following sentence 'The model runs in an offline mode...' could be moved to Model Description section.

p24508, l24: Weibull wind speed distribution, again repeated information

p24509, l6: 'is the a two-parameter'

p24510, l10: '...and we accept vg we just calculated.' should be '... and we accept vg as in Table 2'?

Eq. 13: not all terms are defined?

p24510, l16: 'Associating the formula of vg and Re,' can be deleted.

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p24510, l16: 'gravitational sedimentation velocity' is same as 'fall velocity'? Should be consistent and stick to one term.

p24510, l20: 'vg varies a little with location since the wet radius depends on location.' Expand on this? Do you mean wet radius depends on humidity, which varies with location?

p24511, l6: '...where the constant of proportionality vd is called...' could be changed to '...where vd is...'

p24511, l9: 'We use the method described in Zhang et al. (2001)'... to calculate dry deposition velocity

p24511, l22: 'It is determined'. What is determined, Rs?

p24511, l24: '...respectively.', add reference to Table 2 at the end of this sentence?

p24512, l10: 'The dry deposition...' should be 'Dry deposition...'

p24514, l10: 'percentage' should be 'fraction'

p24515, l8: 'Since...' should be 'As...'

p24516, l6: '...than CMS...' should be '...than the CMS...'

p24516, l7: 'Note that we did not apply...', this sentence needs rephrasing.

p24516, l10: '...results to the CMS...' should be '...results to the CMS source function...'

p24516, l18: 'since' should be 'as'

p24516, l18: 'production' should be 'source'

p24516, l19: Please provide IMPROVE reference

p24520, l22: 'The wind speed dependence...', of Total number? SSA number?

Eq. 24: Terms need defining.

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p24521, l19: 'Again, the model and the data are in different years, which could also bring in some of the discrepancies.' to 'Again, the model and the data are in different years, which could introduce some discrepancies.'

p24522, l6: '...different than the data,' to '...different to the data,'

p24522, l14: 'radius' to 'radii'

p24524, l11: 'Vigniti' should be 'Vignati'

p24525, l15: 'rage' should be 'range'

p24525, l22: Generally not good idea to start a sentence with a number, suggest rephrasing

p24527, l3: 'removing' should be 'removal'

p24527, l14: 'among' should be 'between'

p24527, l29: 'most' should be 'mostly'

p24528, l16: 'SSA model' could be 'SSA source function'?

p24529, l10: 'tends' should be 'tend'