Atmos. Chem. Phys. Discuss., 8, S2561–S2564, 2008
www.atmos-chem-phys-discuss.net/8/S2561/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Parameterization of sea-salt optical properties andphysics of the associated radiative forcing" *by* J. Li et al.

Anonymous Referee #2

Received and published: 12 May 2008

Review comments on paper "Parameterization of sea-salt optical properties and physics of the associated radiative forcing" by J. Li et.al.

The authors describe a parameterization for optical properties of sea salt that they describe as a "simple functional form" in the abstract, and state that they are "versatile and efficient" in the conclusions. I did not find this parameterization to be simple nor versatile, and I doubt that it will be useful for other modelers.

Their parameterization consists of equations 16-18 and eq. 20, for which no justification was given, and which do not appear to have any physical basis. This would be fine if they provided excellent fits and were simple to use, but Table 1 lists 180 coefficients that are required, and a glance at Figure 3, the comparison of modeled to theoreti-

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cal calculations, show that this number could have been reduced considerably, and in fact, that a good number of these coefficients could be eliminated entirely. The single scattering coabledo seems to be constant over the entire range of RH for any one wavelength, yet it is parameterized by equation 17. This is neither simple nor necessary. The asymmetry parameter and the specific extinction coefficient appear almost linear, and the parameterization given by equation 18 doesn't seem to be that good; it would seem that a line could do as well. The authors never demonstrated that the variation in asymmetry parameter, for instance, which covers an extremely small range of values (0.77 to 0.79 in one case), is even important, and one questions whether a constant value would give results that are sufficiently accurate.

The authors note that the forcings are linear in the mass loading of sea salt aerosol, but later they state that sea salt can be treated as a small perturbation on the radiative effects of the atmospheric gases, so it should not be surprising that the forcing due to sea salt is linear. The correct method of determining "small," however, is not mass loading, but the contribution of sea salt aerosol to the aerosol optical thickness, which they did not give. This would provide an immediate means of determining the smallness of the forcing, and thus whether or not linearity is expected. The "interesting result" was also found that the sea salt forcing could be either positive or negative, but this result is not new and goes back to Haywood and Shine (1995) and Chylek and Wong (1995).

There are several items, some minor and some not, that require modification or additional discussion.

The notation is poorly chosen; using "WAC" to represent what could be done with a single variable, as is standard, could easily lead to confusion, as on p. 5822, line 20, where the expression "WAClx" is given.

In Figure 2, the behavior of the effective radius and the concentration with height look quite odd near the surface, calling into question the accuracy of the model. This requires discussion as to why these behave the way they do.

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In Figure 3: the axes on the graph in the upper right are incorrect.

The words "solar" and "longwave" should be replaced by "shortwave" and "longwave" throughout.

On p. 5816, line 4: "changing not only size but also composition" could be interpreted as a change in the solute, not the water:solute ratio, which is what is meant.

On p. 5816, line 23: The description of seawater drops as "solutions of water and sodium chloride and minor organic compounds" seems an odd characterization; there are other substances that are much more abundant than organics.

On p. 5817, line 1: include "or until they fall back into the sea."

On p. 5817, line 5: it would be nice to have a citation that "the size distribution of sea-salt aerosol closely resembles a lognormal" it is often parameterized as a lognormal, but this is a different statement entirely.

On p. 5817 and in Figure 2, it is not clear if these radii are wet values at the actual RH or dry values.

On p. 5819, lines 21-22: if the growth factor depends only on H as you assumed, then there will be no distortion for the lognormal. Similarly on p. 5820, line 12, the approximate equality should be an actual equality.

On p. 5821, line 12: this equation is incorrect; each of the terms in parentheses should be raised to the third power, not the entire expression.

On p. 5821, line 14: as the entire paper is on optical properties, the method of determining the index of refraction should be explicitly discussed. Volume-weighting of refractive indices, which was used, is totally ad hoc and has no theoretical basis, as opposed to volume-weighting of molal refractivities, which is then used with the Lorentz-Lorenz expression to determine the refractive index. Discussion or justification would be advised.

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On p. 5823, line 17: it was stated that "humidity effects are considered based on Kohler theory" without any discussion of how this was done; this requires discussion and citations. Aren't the particles sufficiently large that this is not an issue?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 5813, 2008.

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