**Atmospheric
Chemistry
and Physics
Discussions**

# *Interactive comment on* "Model of optical response of marine aerosols to Forbush decreases" *by* T. Bondo et al.

**Anonymous Referee #1**

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This manuscript discuss the potential effect of Forebush decreases on atmospheric optical depth via ion processes. The approach combines simple sulphuric acid model which produces a certain aerosol size distribution. From this distribution the authors calculate AOD and merge the result with a constant sea-salt AOD and calculate the resulting Ånström exponent. The idea is to study the effect of changes in ion production on aerosol optical properties.

In general, the assumptions and approaches used in this article need to be better proven as in modelling studies the results are normally strongly dependent on underlying assumptions.

1) The hiding of different sink processes in "k" is problematic as many of the processes depend on each other (e.g condensation loss of vapours vs. effect of coagulation on

total particle surface) and many of the processes are also strongly size-dependent.

2) The model simulations cover several days. It is nowhere said what is the beaviour of sulphuric acid production term: in the atmosphere, sulphuric acid is produced by OH-reactions which are solar radiation dependent. As many of the aerosol processes are non-linear, the potential assumption of constant sulphuric acid production is clearly wrong => if the sulphuric acid formation rate is sinusoidal, the new particle formation rates should also follow the similar pattern.

The previous paper by Enghoff et al., 2008 does not answer this question.

Nucleation mechanisms:

- The authors assume that the formation rate depend only on "Q", and "s" is constant is problematic. Clearly, sulphuric acid concentration (or ion-induced cluster formation rate) is not constant, and due to nonlinear processes, the resulting particle formation can not represent the real situation.

3) Due to highly non-linear processes, coagulation and condensation can not be treated separately. A half-time approach does not take into account normal short term competition between the growth and loss processes.

3) What are e.g. the nucleation mode particle growth rates resulting from the initial assumptions? The authors should also present an example of development of aerosol size distribution during the modelling period.

4) what is the effect of the particle formation size? Typically the modelling results are very sensitive to this parameter, and a sensitivity study over e.g. 2-6 molecules should be made.

5) The comparison with AERONET data can not really be made, as the AERONET sites are above the continents and in the approach of this paper, aerosol consists only of sulphuric acid and sea-salt: above land, main fraction of aerosol is typically organic, and depending on season, location and meteorology, soil compounds can have a sig-

nificant influence as well. How is the comparison with AERONET sites exactly done? Can it really be done?

6) Above oceans, cloud processing is one of the main factors modifying aerosol size distribution, not only the number, but also the size. What is the effect of cloud processing on results?

These comments are not made to depress the authors - the mechanism presented is possible and should be studied, but unfortunately, in this manusript, assumptions and simplifications made are in many parts clearly unrealistic. My recommendation would be that the authors merge their forces with some global modeller and repeat the study with more sound microphysical model.

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