**Atmospheric
Chemistry
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Interactive comment on "A parameterization of size resolved below cloud scavenging of aerosols by rain" *by* J. S. Henzing et al.

Anonymous Referee #2

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General comments The article discusses a suitable topic for publication in ACPD but in the present version it lacks of important parts and can be published only after major revisions of the text. The authors discuss the development of the parameterisation but it is impossible to reproduce the results because the information on the parameters in the final formula is missing. Furthermore the new parameterisation is not evaluated. I have found a total lack of reference to works done on the subject of below cloud scavenging, both from the modeling and measurement point of view. I find essential an evaluation of the TM4 sea salt model against measurements, since it was not done before and not presented in this paper either. Some of the paragraphs are not clear and it is necessary to clarify the concepts.

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Specific comments 1) In the introduction there is a total lack of reference to other works done on the below-cloud scavenging, very small introduction of the process itself and lack of previous works done on the evaluation of the process (the authors can start looking at Andronache et al, ACP, 3, 131-143, 2003 and Zhang et al., Atm. Env. 38, 4653-4665, 2004)

2) Paragr. 2.2 Formula (4) gives the fitting formula, but there is not an indication of the parameters A0, A1, A2, therefore the formula cannot be reproduced. Please insert a table with the values for the used class bins, otherwise the formula is not useful.

3) Paragr. 3 The authors have not included a comparison with measured sea salt data, claiming that since the sea salt function is not validated it does not make sense to do it. In reality it is important to know how the whole model behaves because other processes are included in the model and used to evaluate the importance of the below scavenging against other removals. It is like claiming that since emission inventories for other aerosol compounds are quite uncertain one should not compare the model results with observations. I think it is quite important to see how TM4 sea salt model behaves and strongly suggest presenting the model evaluation in this paper. The authors can start using data from various Gong's papers (Gong et al. JGR, 102, D3, 3805-3818, 1997; Gong et al, JGR, 102, D3, 3819-30, 1997; Gong et al., 107, D24, 4779, 2002).

4) Parag. 3.1 - The source function used in the paper is from Gong's 2003, but later in the paragraph talking about the mass emissions, there is a reference to Monahan et al 1986, can the authors clarify what they have used from Gong and from Monahan? - Please report the original formula from Gong, it makes easier for the reader to follow, without needing to search for the reference while reading. - Which value of theta has been chosen in the source formula? - Gong source function and Gerber parameterisation are often used in models, therefore it is useful if the authors report the formula of dry sea salt source function ($dF/dRdry = dF/dRwet*dRwet/dRdry$) that they have finally used.

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5) Parag. 3.2.1 - in the expression fi= exp[-BiDt] what is Bi? - in the expression Dtno-mix = N*Dt what is N? - The choice of six hour interval for the no mixing time is due to the available meteo data, but what is the sensitivity of the model response to this choice? - What is the consequence of assuming all aerosol particles acting as condensation nuclei, instead of having an activation radius? The rest of the paragraph "The second step . . . per unit of time" is not clear and needs to be explained more extensively, please explain also how the information from ECMWF fields are used for that.

6) Paragr. 4.2 "A removal proces .... is not the case", super-micron particles will probably dry deposit not far away from the sources, please comment of this.

7) Parag. 5.1 - "However, it is unlikely. . . .with this distribution", please explain why. - I have understood that the de Wolf distribution is used, and in the figure 1, the Marshall Palmer distribution seems to be "closer" to that than the "Joss drizzle" distribution, how it is possible that calculating the scavenging coefficient using the "drizzle" distribution the results are closer than using the Marshall Palmer distribution? - Please explain the sentence "The central plus. . . .. distributions"

8) Paragr 5.2 In the sentence "The aerosol mass of a sea salt particle with dry. . ...." Do the authors mean the total mass including water? Please explain.

9) Paragr. 5.3 How is the evaporation of falling rain droplets done in the model? Which parameters are taken into account?

10) Conclusions The authors write that the removal parameterisation is presented and not evaluated, it is true, and therefore after having done an evaluation of modeled sea salt concentrations against observations, a sensitivity run should be done without the newly developed parameterisation to see the difference in the results and include this in the paper.

11) Fig. 4 Simulated monthly variations of global and hemispheric SEA SALT emis-

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sions for the year 2000

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