Review of manuscript "Comparison of two different sea-salt aerosol schemes as implemented in air quality models applied to the Mediterranean Basin" By P. Jimenez-Guerrero and coworkers.

The manuscript presents the results of two chemical transport models, CHIMERE and CMAQ, applied to the Mediterranean Basin, using two different sea salt emission schemes with the aim of examining the sea salt mass differences in model results. I'm not in favor of the manuscript publication on ACP because the methods that are applied are not scientifically sound, it lacks of convincing formulations and analysis of the results.

Major concerns:
1) Formulation of sea salt emission functions
In paragraph 3.3 the authors analyse the differences in the source functions. Equation 3 should report the formulation given in Monahan et al (1986), however the reported expression is not the formula included in Monahan publication. The original formulation is

$$\frac{dF}{dr} = 1.373 U^{3.41} r^{-3} \left(1 + 0.057 r^{1.05}\right) x 10^{1.19 e^{-B^2}}$$

with $B = \frac{(0.380 - \log r)}{0.65}$ eq. 12 of the original Monahan article. The constants in the formula are different from manuscript equation 3. dF/dr is the number of aerosol droplets per unit area of the sea surface per increment of droplet radius and not aerosol mass. Furthermore r is not the radius of the bubble at formation but the aerosol droplet radius as reported by Monahan and looking at the introduction of the original manuscript it is clearly said that the reported formulation is for "open-ocean sea-surface aerosol generation" and not from "… oceanic whitecaps along the coast" as reported in the submitted manuscript. Although stated by the authors that the formula is for coastal emissions, the formula is applied everywhere over the oceans, yet there is confusion in the expression formulation, in the meaning and the application.

At which relative humidity the radius of the equation 3 are taken? How this has been changed to make it compatible to the nature of the aerosol particles described in CHIMERE in terms of the actual relative humidity?

The formulation contained in equation 5 was derived by Gong (2003) and corrected by Zhang et al(2005) correction factors that the authors mention as Co and C80. In the following formulas Co does not appear; it is not clear from the equation 10 of Zhang et al, how the expression 6 in this manuscript comes from. It is not clear what is the relative humidity RH, what is the relationship between the relative humidity at which the original formula of Gong is given and the relative humidity RH.

How this function has been used to make it compatible with the description of aerosols in CMAQ which is modal?

2) Use of measurements for model evaluation

In paragraph 3.2 the models are evaluated against AOD and sea salt concentration measurements.

A) It is stated that for "…stations with a strong influence of SSA (Forth Crete, Oristano,…) in order to quantify the skills of the model for reproducing the SSA in column. In these stations the total AOD at 550 nm is assumed to come mainly from SSA". Forth Crete is at the Northern coast of Crete, not far from Finokalia. Looking at the paper of Querol et al. 2009, mentioned also by the authors, particularly at figure 4 and 5, it is clear that sea salt component in the aerosol at Finokalia is not et all the major component, actually it is a minor component compared to dust, nitrate and organic matter, therefore the assumption made is clearly wrong.

Furthermore it SSA may not be the major component either for Lampedusa nor for Oristano where dust outburst from Sahara can reach the islands. In F. Barnaba and G. P. Gobbi, ACP, 4, 2367–2391, 2004, maps of the AOT over the Mediterranean Basis clearly show this (figure 6).

B) The AOD calculation from model output is done using the expression 2, it is explained in the following text that since only SSA was included in the calculation of the extinction only the SS mass contained in PM2.5 and PM10 was used. The formula assumes a dependence on RH only for NH4, NO3 and SO4, and not for PM2.5or PM10. Sea salt particles are very hygroscopic therefore the amount of water attached to the particles cannot be disregarded as the formula may suggests. Therefore the calculation of the modeled SSA AOD is not done correctly.

C) Measurements of SS concentrations are used for a statistical analysis of the modes' results, but they are not used to discuss the ability of the model in terms of spatial or seasonal patters, yet there is a discussion on the model spatial ability of calculating SSA. Furthermore it is not clear which stations have been used.

Finally the structure of the manuscript is confusing. Formulations of sea salt function as well as deposition parameterisations are described only after the evaluation of the model results. The aerosol description in two CTMs are not sufficiently described, there are only references but the article should be self-explaining for what concerns results' interpretations.