

Dear Anonymous Referees #1 and #2,

We thank you for your useful and constructive review of our work. We believe that the quality and the clarity of the manuscript has increased after the review.

Please find a new version of the manuscript revised according to your Comments. The major Comments of both Reviewers match and concern four main issues (labeled as Main Issues, MI):

- MI1. The importance of focusing the manuscript on global results, especially on the AOD resulting from SST-dependent schemes. In this sense, mostly Referee #2 points out the weakness of our comparison of 1 simulated year with climatological datasets. Referee #2 also underlines the role played by the uncertainties in the emission process due to wind speed, which is not exhaustively discussed in the manuscript.
- MI2. The removal of the regional subsections (5.2 and Supplementary Material), which require a more exhaustive analysis.
- MI3. The need to introduce the SST-dependent scheme of Jaeglé et al. (2011) in our comparison of emission schemes.
- MI4. The need of a more exhaustive comparison of our results with previous studies.

Before replying to the specific comments, we provide a response for these four issues (labeled as Main Responses, MR). In all cases we have followed the Referee's suggestions:

- MR1. We extended the simulation period from only 2006 to a period covering 2002-2006 (5 years). In this way we achieve a more robust comparison with observed monthly climatologies (U-MIAMI concentrations and AERONET coarse AOD; see Figs. 4, 5, 8, 10, 11, and 13 in the new manuscript). We also provide a comparison of our simulated 5-year averages of wind speed with NCEP climatologies (1981-2010, from NCEP/NCAR Reanalysis; see Figs. 9, 10, 12, and 13 of the new manuscript) at measurement sites. The aim is to check the representativeness of our simulated 5-year period against a 30-year climatology and we are aware that the NCEP climatologies may be not representative of the wind speed at certain sites.
- MR2. We removed subsection 5.2 and the Supplementary Material from the manuscript. However, we consider our regional results as an interesting issue for the community. In this sense, we hold them as preliminar results and we plan to present a more detailed study in a separate contribution.
- MR3. The scheme of Jaeglé et al. (2011) was introduced in our work. We found that SST-dependent schemes, particularly Jaeglé et al. (2011) lead to a better simulation of surface concentration. However, they produce an overestimation of the coarse AOD in the

observational sites located in the tropics (see Figs. 11 and 13 of the new manuscript). In the new manuscript we discuss the factors that may explain this behavior, which include uncertainties due to the use of all-sky model AOD in the comparison, the treatment of water-uptake, deposition, and optical properties in the model and/or an inaccurate size distribution at emission.

MR4. We have expanded Table 6 and included a detailed discussion in subsection 5.2 comparing with previous studies, such as Tsigaridis et al. (2013), Jeanglé et al. (2011), and Textor et al. (2007).

Below we provide an item-by-item response to the specific Comments of Referees. The author's response is labeled as MS (Michele Spada).

Anonymous Referee #1.

Page 11599, line 3: Based on the zoom results, even 0.1x0.1 should be considered as coarse? Which resolution is coarse, based on the author's interpretation?

MS: This line has been suppressed in the new manuscript. At global scales, we consider 1x1–2x2 as coarse resolution, while 0.1 as high resolution. At regional scales, we consider 50km, 10km, and 1km–5km as coarse, medium, and high resolution, respectively.

Page 11602, lines 6-10: It was mentioned earlier that the direct effect is not yet included in the model, since it is under development. How are the aerosol optical properties taken into account then?

MS: As diagnostics to evaluate the model. The model AOD calculation is detailed in section 3.4.

Page 11603, line 25: The Lacis and Hansen (1974) radiation comes from GISS, not GFDL. Maybe the authors mean that they used the GFDL version of the radiation transfer calculations developed at GISS?

MS: Yes, we used the GFDL radiative package, which includes the shortwave scheme of Lacis and Hansen (1974). This has been specified in the new manuscript (line 121-123).

Section 2.2 is not needed, the reader can refer to Perez et al. (2011) for the details on dust implementation.

MS: In subsection 2.2 we only present the main aspects of the dust module and details which are useful to understand the calculation process of the sea-salt+dust coarse AOD used in our work.

Page 11605, lines 6-8: This sentence makes no sense, since there is no indirect effect calculation included.

MS: We agree. Eliminated from the text.

Page 11606, lines 6-10: Most (all?) parameterizations are not valid in this size range anyway.
MS: we are aware of that and we indicate it in Page 11607 lines 10–13 of the ACPD manuscript (lines 203–205 of the new manuscript). We extended the M86, G03, and consequently J11 schemes beyond their upper cutoffs, since we are interested in a consistent comparison among all schemes, that include the M86/SM93 scheme in the formulation of Hoppel et al. (2002) with an upper cutoff up to $15\mu\text{m}$ in dry radius.

Page 11607, line 6: This cutoff means that coarse particles, where most of the mass is and will dominate in surface concentration comparisons, has no SST dependence? If yes, this should be made clear in the discussion that follows, especially when the SST effect is mentioned.

MS: Yes. In the new manuscript (lines 486–488) we specify that only the smaller bins are SST-dependent when using the MA03/M86/SM93 bins. However, the effect of these bins is not negligible (see Fig. 8 and 10 of the new manuscript).

Section 3.4: what refractive indices were used? Are the model results used clear-sky or all-sky?

MS: Refractive indices calculations are referenced in the new manuscript (lines 259–260). They are taken from the Global Aerosol Data Set. The model results are all-sky (indicated and discussed in the new manuscript, lines 509–511 and section 6). This may introduce uncertainties in the comparison of simulated and observed AOD, although differences between all-sky and clear-sky model results are thought to be moderate for sea-salt and very low for dust (Shindell et al., 2012).

Section 4: Why the Maritime Aerosol Network (MAN) data were not used? They are not continuous measurements, but they cover a big part of the worlds oceans. Since other cruise data were used in the manuscript, I do not understand the exclusion of MAN.

MS: This is explained in the new manuscript (lines 381–384). In our case, it is difficult to disentangle the contribution of sea-salt and dust from other aerosol species (such as carbonaceous and sulfate aerosols) that are not implemented in our model

Page 11613, last line: Since the year 2006 was simulated, the information for pre-2000 is not needed.

MS: Information pre-2000 is needed for cruise simulations.

Page 11614, lines 6-9 are repeated, and can be deleted.

MS: Deleted in the new manuscript.

Same page, line 13: all should be any of

MS: Corrected in the new manuscript.

Section 5.1.2 appears to be circular comparison. Since the model uses reanalysis winds, the comparison with satellite retrievals is like comparing the reanalysis, not the model. In addi-

tion, isnt the case that NCEP uses QuickScat in the reanalysis?

MS: As already stated in MR1, we eliminate section 5.1.2 of the ACPD document in the new manuscript

Page 11615, line 6: important peaks to what respect?

MS: Important for the global patterns of emission, surface concentration, and AOD that we are discussing in this subsection.

Page 11616, line 12: overestimate by how much?

MS: As indicated in the text (ACPD Page 11616 10–15) 0.1–0.125 with respect to observed 0.06–0.07 in 2006 and (new manuscript lines 398–402) 0.1 with respect to 0.06–0.07 with the 5-year simulation.

Page 11618, lines 9–13: A more detailed discussion is lacking here.

MS: A more detailed discussion is provided in the new manuscript (lines 440–454), presenting wind speed correlation, mean normalized bias, and mean normalized gross error for each cruise.

Page 11619, line 13: I do not see a significant influence on the applied emission scheme. The spread appears to be about the same with the measurement error bars, which show the interannual variability.

MS: In the new manuscript (Fig. 8), this spread concerns the difference between 5-year average and climatological measurements and the effect of the interannual variability is reduced.

Same page, line 20: How is the fit weighted?

MS: Not weighted in the new manuscript. It was weighted with the interannual standard deviation in the ACPD manuscript.

Page 11620, line 10: Other than the interannual variability range, the absolute uncertainty of the coarse mode AOD product should be mentioned here.

MS: At lines 323–324 of the new manuscript we indicate the accuracy of AERONET measurements (0.01 for the total AOD at 500nm) (Holben et al., 1998; Smirnov et al., 2000). As done for surface concentration, we are interested in the comparison with observed climatologies and their interannual variability.

The x-axis of figure 1 is dry or wet size?

MS: Dry radius, specified in caption of Fig. 1 of the new manuscript

The color scale in Figure 4 needs to be modified, it is important for the zero values to be clearly visible.

MS: Fig.4 of the ACPD document is removed in the new manuscript.

Figure 5: Please comment on the Arabian peninsula local maximum.

MS: As stated at lines 365–366 of the new manuscript, this local maximum is due to the strong southwestern winds of the monsoon circulation.

For both figures 5 and 6, all color bars can be deleted, except the ones appearing at the last row.

MS: Done. Figs. 5 and 6 of the ACPD document correspond to Figs. 4 and 5 of the new manuscript.

Figures 9 and 12 have the regression lines forced to pass through zero. This is not a good idea, since it potentially strongly affects the slope of the line. In addition, it implicitly assumes that when measurements have zero, the model also is zero, which is not necessarily the case.

MS: Figs. 10 and 13 of the new manuscript (that correspond to Figs. 9 and 12 of the ACPD document) present linear regression fits not forced to pass through zero.

Anonymous Referee #2

P11607 - L 6: Just as review 1 points out, by using only the SST dependence below 1.4 microns, you are removing any potential variability in the particle range controlling mass, which is the only region where you perform comparative analysis. This has to be corrected and clearly discussed or the whole SST test, results, and discussion should be removed.

MS: As answered to Referee #1, this issue is discussed in the new manuscript (lines 518–520). The use of MA03 only below 1.4 microns is due to the validity range of the experiments of Martensson et al. (2003), that concern bubble-bursting production.

We respectfully disagree with the statement that the model smaller bins are excluded from the comparative analysis performed in this work. As shown in Figs. 8 and 10, they participate in the total surface concentration (see the differences between M86/SM93 results and MA03/M86/SM93 results). We note that - due to the water-uptake process - they may also contribute significantly to the coarse AOD. Model bin 3 has a dry effective radius of 0.43 microns, which, at maritime conditions (RH=80%), corresponds to a wet radius of 0.86 (applying a growth factor of 2 as assumed in our work), affecting the coarse AOD (we assumed wet radius $0.6\mu\text{m}$ according to AERONET). In any case, as highlighted before, the new manuscript also includes the scheme of Jaeglé et al. (2011) with SST dependence for all particle size.

Section 5.1.2 This is a useful experiment, but the analysis is weak. Please present some proper statistical analysis of the results. From this, a sense of the model vs. observed U10 uncertainty can be applied to the aerosol emission uncertainty. Was the bias reduction based on resolution refinement statistically significant?

MS: Subsection 5.1.2 of the ACPD document is removed from the new manuscript. It is replaced by Figs. 9, 10, 12, and 13 and relative discussion in subsection 5.4 and 5.5. In the new manuscript we compared our 5-year simulated wind speed with the NCEP/NCAR

climatology (1981-2010).

P11615 - L19-20: Arent the trade winds actually very humid? Dont they drive tropical cyclones?

MS: Yes. This discussion is reformulated in the new manuscript (lines 360–364). Low wet deposition rates in the tropics correspond to dry condition regions close to the intertropical convergence zone.

P11615 - L25: This analysis should be performed more explicitly and quantitatively. Simply referring to shaded contour maps isnt much use to the reader.

MS: In the new manuscript, this analysis also includes the results from the Jaeglé et al. (2011) emission scheme. Moreover, the analysis is complemented with the total budgets calculated for each scheme and compared to previous studies in Table 6 and discussion in section 5.2.

P11616 - L11-14: Why are the peaks overestimated by your results?

MS: It is explained at P11616 lines 12-15 of the ACPD manuscript (lines 381–384 of the new manuscript). Because we obtain a sea-salt AOD around 0.1 and measurements are around 0.06-0.07 in these regions for the total AOD (Smirnov et al., 2011).

P11617 - L23-28+ : A strong correlation is cited but there is no quantitative analysis of this correlation presented. Provide a more robust statistical analysis.

MS: We refer to Fig. 7 of the ACPD document, where the correlation of the AEROINDOEX cruise is explicitly indicated ($r=0.60$). Fig. 7 of the ACPD document corresponds to Figs. 6 and 7 of the new manuscript

P11618 - L9-11: Do the 10m and 18m wind-speeds compare well enough? A basic calculation using a sea-surface roughness parameter of 0.0002m yields nearly a 1 m/s difference between wind at 10m and 18m. I may be wrong though. It may be worthwhile converting the 18m winds to 10m winds to be sure.

MS: We agree. In the new manuscript, we used the Hsu et al. (1994) formula to convert 10m winds in 14m and 33m winds for the comparison with the two cruises.

Figures (general): The use of shaded contour maps makes it difficult to get more than a qualitative sense of the regional differences between the functions. Suggest expanding this analysis.

MS: Shaded contour maps are intended to give a qualitative and quantitative representation of the global patterns. On the other hand, detailed evaluations and discussions are performed in correspondence of the measuring sites (Figs. 8, 9, 10, 11, 12 and subsections 5.1.5, 5.1.6, 5.1.7 of the ACPD document, Figs. 8, 9, 10, 11, 12, 13 and subsections 5.4, 5.5, 5.6 of the new manuscript).

Figs 5,6,7,8,10 - Increase font size. It was difficult to read unless zoomed far into the PDF.

Unreadable on paper.

MS: Increased font size. In the new manuscript Figs. 4, 5, 6, 7, 8, 9, 11, 12 (wind speed plot added).

Can spume droplets be mixed high enough to be observed in the U Miami instrumentation? I know that some sites are offset from the surf-zone sufficiently that spume droplets may not reach the sampler

MS: In our formulation of the M86/SM93 scheme, the open-ocean spume droplet production is not related with the surf zone, which is neglected in our work.

The U-MIAMI stations used in this work are not significantly affected by surf-zone production, but no upper cutoff is applied to these measurements (J. Prospero, personal communication, 2012).

In our simulations, spume particles generated in the open-ocean are characterized by a lifetime longer enough to affect the U-MIAMI measurement sites (as shown in Fig. 8). For example, we simulate a lifetime (τ) of around 3h for our greater size-bin (bin 8). Roughly assuming this lifetime value and open-ocean spume production conditions ($U = 9m/s$) for the transport over land, we obtain a characteristic length around 100km ($L = \tau U$), that is well above the surf-zone influenced fetch indicated by previous model studies (de Leeuw et al., 2000).

In the literature, we found previous studies comparing results from SM93-based spume emission schemes with observations from the U-MIAMI network, following the same approach used in this work (Fan and Toon, 2001).

There are many many typographical and grammatical errors. It became overwhelming to catalog them all for this review. Please carefully review the paper prior to resubmission.

MS: A careful review has been applied.

We hope our responses to your comments are clear and exhaustive.

We thank you for the reviews,

Kind Regards,

Michele Spada
Earth Sciences Department
Barcelona Supercomputing Center (BSC-CNS)
Edificio Nexus II
c/ Jordi Girona 29, 08034 Barcelona, Spain
Phone +34 934134049
mail to: michele.spada@bsc.es