

Interactive comment on “Development and basic evaluation of a prognostic aerosol scheme in the CNRM Climate Model” by M. Michou et al.

M. Michou et al.

martine.michou@meteo.fr

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Dear Referee,

Thank you for your review, and for your questions and suggestions, which appear below in italics, together with our responses to these remarks and suggestions. Our proposed amendments to the text of our paper appear in bold. Page and line numbers refer to the version of the paper you reviewed.

1. *General comments ...*

... MACC aerosol module is a simple aerosol module developed by Morcrette et al. at 2009. The transformation of SO₂ into sulfate is done without any explicit chemistry. Nitrate and ammonium are not considered in the model. Lamarque et

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al. (2012) incorporated the online full chemistry scheme MOZART into the CESM model system. They used the Bulk Aerosol Model (BAM) coupling with MOZART to simulate sulfate, sea salt, dust, and carbonaceous species mass concentrations by considering major chemical and physical processes including emission, transport, gas phase chemistry, aqueous phase chemical reactions, dry deposition, and wet scavenging. Liu et al. (2012) introduced a modal aerosol module (MAM) in CESM which can be used to study aerosol size distribution and both internal and external mixing between aerosol components. Comparing to these previous works, the treatments of aerosol chemistry, mixing state, and scavenging in MACC aerosol module, which was incorporated in the CNRM climate model by this work, are too simple. When the authors calculate aerosol optical depth, how do they deal with particle growth caused by the uptake of nitrate, ammonium and aerosol water? And I do not find any new improvements to address these problems in this work.

We do not claim to show a development with regards to aerosol modelling comparable to what exists in some climate models, such as the CESM model mentioned in the above comment. We present a modest development, as (1) the work was initiated from scratch just less than four years ago, and (2) we have very limited man resources to devote to this subject. Back five years ago, the CNRM climate model included, concerning chemistry, the linear scheme of *Cariolle and Teyssèdre (2007)*, and prescribed aerosols. Since then, an interactive chemistry scheme has been introduced on-line (*Michou et al., 2011*). We are not pretending to possess not even a state of the art aerosol module, but a simple scheme that would interact with other parts of the model, notably radiation, keeping the numerical cost low for multi-annual simulations, and bringing aerosols from the prescribed state to an interactive one.

Table 9.1 in *Flato et al. (2013)* provides some insight into the characteristics of the CMIP5 models, particularly in relation to aerosols and atmospheric chemistry.

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Among about 40 models, half included fully interactive aerosols, while six used prescribed aerosols, and the rest used semi-interactive aerosols that consisted in climatologies calculated with a version of the model run that included an interactive aerosol scheme. For the atmospheric chemistry, 1/4 of the models included a chemistry scheme.

Our strategy to put in place an aerorol component in ARPEGE-Climat laid upon published developments integrated in the ARPEGE/IFS system, as the atmospheric component of the CNRM climate model is based on this system. When we started this, the only choice was to go for the *Morcrette et al.* (2009) model. Since then other schemes have been introduced, or are being introduced, in the IFS or IFS related systems, see for example a description of the M7 model in EC-Earth in *Noije et al.* (2014), or of the GLOMAP system *Mann et al.* (2012) in the IFS. We may benefit from these developments in the future, although the question of the level of complexity required with regards to aerosols in climate models is still under debate (*Boucher et al.*, 2013).

- 2. The consideration of aerosol and climate interaction processes is important for the implementation of aerosol module in climate model. Based on current manuscript, I cannot obtain enough information to let me understand how the authors deal with aerosol and climate interactions. I would like the authors to provide more details about the treatments of aerosol direct and indirect effects in the manuscript. And what are the values of simulated aerosol radiative forcing and cloud radiative forcing in this study? Do the authors consider the impacts of aerosol on clouds and precipitation?*

We agree that the ultimate interest of aerosols in a climate model is the two-way interactive aerosol-radiative scheme, and indeed this article does not deal with aerosol and climate interactions. The objective here is the validation of the prognostic aerosol distributions obtained with the aerosol module imbedded in the climate model, with a one-way interaction of the meteorology upon aerosols

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only. A coupling of the prognostic aerosol scheme and the radiative scheme is not active here. The framework of our simulations is that of a Chemistry Transport Model (CTM), which is the case for a number of the AEROCOM, or even ACCMIP models. The interaction of prognostic aerosols and radiative forcing in our model will be explored in another article.

The setup of our simulations is detailed in section 3.1 “Simulations”, which includes the following sentences: “The simulations performed (see Table 3 for a summary) include firstly an ARPEGE-Climat simulation with 2004 conditions for all forcing, namely SST, GHG gases and climatologies of aerosols. This climatology of aerosols is the one that interacts with the radiation scheme of ARPEGE-Climat, as in the CMIP5 simulations (see *Voldoire et al. (2012)*; *Szopa et al. (2012)*), and such a configuration allows an evaluation of the prognostic aerosol distribution independently from their possible impact on the meteorology.”

3. *IMPROVE and EMEP have provided multi-year sulfate, BC, OC mass concentration measurements at numbers of sites over USA and Europe. I would like the authors to do the comparison of their model results with these aerosol mass observations.*

We have added in our revised article a figure (Figure 11) that compares surface mass concentrations of SO₂ and sulfate. The following text has been added in the Evaluation data section:

The EBAS is a database infrastructure (see <http://ebas.nilu.no>) operated by NILU - the Norwegian Institute for Air Research - that handles, stores and disseminates atmospheric composition data generated by international and national frameworks like long-term monitoring programmes, including IMPROVE (United States Interagency Monitoring of Protected Visual Environments) and EMEP (European Monitoring and Evaluation Programme) and research projects. For this article we downloaded and processed surface concentrations of SO₂ and sulfate. These data, depending on the net-

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work, include daily, or weekly values, and for the EMEP or IMPROVE networks, which provided most of the data we used, are representative of areas away from the sources. We present in this article annual means (for 2005) from all observations available.

The following text accompanies this figure and has been added in the paper in the 4.2.2 section, after the paragraphs dedicated to the *Kinne et al.* (2013) climatology:

Figure 11, which compares observations and NudSimd2_Trans outputs of annual (2005) surface concentrations of SO₂ and sulfate, provides additional information on the modelling of sulfate. Correlation between model outputs and observations is better for the European sites (red dots) than for the US sites (black dots), noting that in all cases it is lower than 0.4. While for sulfate the means of observations and model outputs are very close (~0.7), for SO₂ the mean model value is twice that of the mean observed value, some of this overestimation being related to our sulfate precursor including H₂S and DMS in addition to SO₂.

- In this work, the authors added a new mineral dust emission parameterization into the model. This new parameterization is based on the works of Marticorena and Bergametti (1995) and Kok (2011), As shown in Table 3, simulated dust emission by the new scheme is much higher than the values simulated by the old dust emission scheme in GEMS/MACC. What are the major reasons causing such differences of predicted dust emission? In additionally, the global averaged difference of the two scheme is more than a factor of 10. According to the authors understanding, which scheme is more realistic?*

The approach based on *Marticorena and Bergametti* (1995) and *Kok* (2011) takes into account characteristics of the soil at the horizontal resolution of the model that the ECMWF approach based on *Ginoux et al.* 2001 does not consider. More specifically, the former considers the clay and sand content of the soil of the

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model to determine a soil structure, and the soil roughness that plays a role in the erodibility of the soil, where spatially broad empirical factors are used in the latter approach developed at a time where this soil information required for the *Marticorena and Bergametti* (1995) approach was not available (*Morcrette et al.*, 2009). Furthermore, by taking into account the results of *Kok* (2011), we correct for a general drawback of GCMs that is that previous model relations could overestimate the mass fraction of the dust fine mode while underestimating the fraction of coarser aerosols. For all these reasons, our preferred approach is that of *Marticorena and Bergametti* (1995) and *Kok* (2011).

Emissions in the ECMWF dust scheme are proportional to a so-called dust emission potential factor. For the simulations presented in this article we used a value of $1.e-11 \text{ kg s}^2\text{m}^{-5}$, which is the value adopted in the latest version of the aerosol code at ECMWF, but a value of $2.e-11 \text{ kg s}^2\text{m}^{-5}$ was used for the MACC Reanalysis and discussions with our colleagues at ECMWF revealed that a value of $5.e-11 \text{ kg s}^2\text{m}^{-5}$ had been used in specific cases.

Emission totals with this new dust scheme appear to be at the high end of the values reported in *Huneeus et al.* (2011) for 15 AEROCOM models, with particularly high emissions over the Middle East and Australian regions. As indicated in the article, we have adopted for our simulations here a value of 5.10^{-7} for the c_α coefficient proportional to the vertical to horizontal flux ratio, involved as such in the calculation of the dust emission flux. We computed this scaling factor, which differs largely among models (*Todd et al.*, 2008), to bring our 2004 AODs in the Sahelian region, the major global source of dust, into reasonable agreement with the satellite and AERONET observations. We could have adopted various such scaling factors depending on the region, as done by other modelling groups *Tosca et al.* (2013). We will keep that in mind as further developments of our model are put in place.

Differences of predicted dust emission by the two schemes reflect both a change

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in dust emission scheme, and a change in emitted dust size distribution. We have not done the four simulations that would allow us to draw conclusions about the relative importance of these changes. In the end, what is important to us is the final result with the new dust scheme and new size distribution, and that emissions look reasonable. In the end also, the NudSimd2 modelled AOD appear satisfactory compared to observations.

To conclude, although we are concerned by the large differences we present in our article with regards to the dust emission loads, results presented could have been very different if we had make use of different values for the two coefficients (i.e., dust emission potential factor and c_{α}) described above. Nevertheless, we think that the *Marticorena and Bergametti* (1995) and *Kok* (2011) is more realistic to use in the end, for the reasons detailed above.

5. *Based on current model validations provided by the authors, I think the incorporated MACC aerosol module in the CNRM climate model did not show good performances comparing to these observations/reanalysis. The validation of monthly mean global bin concentrations with MACC Reanalysis suggested the climate model simulation significantly underestimated mass concentrations of all the 3 sea salt bins, dust within the range of 0.03-0.5 micrometer, hydrophilic BC, and sulfate. The model significantly overestimated mass concentrations of dust within the range of 0.9-20 micrometer and hydrophobic BC. Simulated mean DJF 2003-2012 total AOD by this work is only half of the values from satellite retrievals, while simulated mean JJA 2003-2012 total AOD is about 60-70% of the values from satellite retrievals. The comparison of model simulation with AERONET observations also indicated that the model significantly underestimated AOD at numbers of sites.*

We do not fully agree to all the comments made here, and we have several arguments for that: (1) indeed, outputs of the MACC Reanalysis and from our simulations are quite different, especially in the global mean diagnostics. This is an

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interesting result by itself: the same aerosol code can lead to different aerosols distributions depending on the meteorological model it is implemented in. However, this affirmation should be qualified as AOD information is assimilated within the MACC Reanalysis system, while it is not in our climate model. *Kaiser et al.* (2012) emphasize the impact of this assimilation within the very same aerosol model. Furthermore, comments made above concerning on the one hand BC (hydrophilic/hydrophobic) and on the other hand the coarser dust aerosol bin need to be refined as for BC the hydrophilic/hydrophobic emission ratio has the inverse value in our simulations (the MACC Reanalysis ratio was incorrect), and for dust the overestimation is related to the use of a fully different dust emission scheme. We have made this clear in our paper. (2) Comparisons of the mean global seasonal total AOD of satellites and of our simulation lead to the large discrepancies outlined by the reviewer. However, the analysis we make at a regional scale reveals more than that overall affirmation. And finally, (3) comparisons with AERONET observations, although revealing underestimation at a number of sites, showed also that, in the climatological perspective, the very diverse annual cycles of the total AOD, with varying dominant aerosol types, were well represented by ARPEGE-Climat for all the AERONET stations of *Cesnulyte et al.* (2014) chosen to evaluate the same aerosol module in the ECMWF weather model. In conclusion, we think that we have a promising tool in hand, and further use of it in comparative analysis exercises, such as AEROCOM or AerChemMIP, will be of particular interest in the aerosol modelling world.

Special comments

6. *P6266, L3: Can the authors give some discussions about aerosol modeling in CESM, GISS, ECHAM, and UKCA?*

We have added the following sentences in our introduction (after p6266 line 10):

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While *Liu et al.* (2012) present in their introduction a review of aerosol treatments in global climate models, from the bulk to the sectional methods, some of which have been under development for a couple of decades, *Flato et al.* (2013) provide the references for the aerosol modules of the CMIP5 climate models (see Table 9.A.1).

7. P6268, L28: Please check the size information about dust bins.

We checked the size information about dust bins. We have reformulated the text as follows: **sea salt discriminates three particle size-bins (boundaries of 0.03-0.5 μm , 0.5-5 μm , 5-20 μm); desert dust also has three size-bins (0.03-0.5 μm , 0.5-0.9 μm , 0.9-20 μm)**

8. P6269, L7: This kind of treatment cannot reflect the impact of boundary condition on dry deposition velocity which is important for tiny particles such as sulfate, BC, OC, and first bins of sea salt and dust.

We fully agree to this. As a comparison, in *Textor et al.* (2006) 5 out of 16 models also used constant velocities, and we are talking here of full aerosol models that do not have the same constraints as climate models with aerosol modules in terms of computer time and therefore complexity. We have however added the following lines in the final paragraph of the Conclusions:

Implementing a more realistic description of dry deposition velocities by including the effect of the meteorology through the aerodynamic resistance should also be a step forward.

9. P6269, L12: How do the authors deal with hydrophobic and hydrophilic species in wet scavenging. Does aerosol bin size impact wet scavenging?

We have moved the sentences in page 6270 lines 7-11

“A detailed description of the original GEMS/MACC aerosol scheme appears in *Morcrette et al.* (2009), and the list of parameters of the scheme, together with

the values used for the MACC Reanalysis (see paragraph 3.2.1), is given in Table 1. These parameters are fully detailed in *Morcrette et al. (2009)*, and for the sake of clarity parameter names in Table 1 correspond to the ones in *Morcrette et al. (2009)*. ”

towards the beginning of this section so that interested readers have this information right away.

As for wet scavenging, the scheme makes a distinction between in-cloud and below cloud scavenging, and input parameters of the scheme include, in addition to meteorological fields such as 3D large-scale and convective precipitation fluxes or the cloudy fraction of a model grid box, the fraction of aerosol included in droplets through dissolution or impaction (D parameter in *Morcrette et al. (2009)* and in Table 1) and efficiencies with which aerosols are collected by raindrops (α parameter in *Morcrette et al. (2009)* and in Table 1). Values for the various aerosols of our scheme appear in Table 1; they are the same for the three DD and SS bins.

10. *P6269, L14: It is very questionable. How do the authors divide gas phase chemistry and aqueous phase chemistry in their study. The production rates of sulfate from the two different processes are quite different from each other.*

Our modelling is a crude first approach of reality. We do not consider explicit chemistry in our aerosol model, not to mention the gas phase and the aqueous phase chemistries. Lifetime of the so-called sulfate precursor of our model, which is considered as one 'species' only, has been approximated, as done in *Huneeus et al. (2007)* whose model was calibrated on the results of the full LOA/LMD-Z sulfur model of *Boucher et al. (2002)*. This approach has been running in a pre-operational mode at ECMWF under the GEMS and MACC auspices since 2008, and it is also part of the four latest generation of quasi-operational aerosol models of the International Cooperative for Aerosol Prediction (ICAP) programme (see *Sessions et al. (2015)*).

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11. *P6272, L23: Without chemistry, how do DMS emission impact the authors' simulation. Do the authors trace DMS transport in their model?*

No, we do not have a specific DMS tracer in our model. The DMS emissions are simply added to the SO₂ and H₂S emissions to form the emissions of our so-called “sulfate precursor” aerosol.

12. *P6273, L2: How do the authors deal with SOA in their model?*

We deal very simply with SOA in our model. As indicated in the text of our original paper, page 6273 lines 1-3 : “as our emission scheme does not describe the SOA formation, we prescribed the SOA inventory of *Dentener et al. (2006)*, representative of the year 2000.” These SOA monthly emissions are considered as a source for our organic matter aerosols.

13. *P6273, L22: Why do the authors rescale the sulfate precursor emissions?*

Results presented in the paper you reviewed included a scaling factor of 0.7 on the sulfate precursor emissions. This factor was chosen somehow hastily, and as both reviewers questioned that choice, we have rerun all our simulations without rescaling these emissions. In the end, in all diagnostics analysed in this paper, model outputs are closer to the observations in the no-rescaling case. Therefore we now show in the revised version of the article this second set of simulations. We have accordingly amended Table 2 of the paper, which shows totals of static emissions, to which we have also added a couple of references.

It has to be noted that differences in the figures/tables between the original and the revised articles are caused by the use of a different/updated version of the atmospheric model, the aerosol model remaining unchanged. Overall, as this update was relatively minor, most of the analysis presented in the original paper has remained valid in the revised version.

14. *P6273, L26: This kind of assumption is unreasonable to me. It may cover some*

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potential problems in the authors' model simulation.

We analysed these high AODs, modelled at very limited grid points, and there was no doubt that they were directly related to the biomass burning emissions at these grid points. Indeed, the monthly biomass burning emissions we use include, very occasionally in space and time, very high grid point values, several times higher than the other relative peaks of the time series. This caused the model to compute unrealistically high AODs. We clipped these values under the maxima given in the text, paying attention that the time series still clearly showed biomass burning events, and that the total monthly emissions remained very similar.

We have amended the text as follows: **as higher values, reached very occasionally in space and time during very intensive biomass burning events or volcanic eruptions, generated unrealistic high AOD (higher than 10) in the model. The impact of this limitation on the monthly or yearly total emissions, and on most biomass burning events, is very small.**

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