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

Anonymous Referee #1

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The manuscript "Development and basic evaluation of a prognostic aerosol scheme in the CNRM Climate Model" by Michou, Nabat and Saint-Martin describes the development of a simple aerosol scheme in the CNRM-CM global climate model. The scheme is a modified version of the aerosol module of the ECMWF integrated forecasting system (IFS), described by Morcrette et al. (2009). In particular, a new parameterization of mineral dust emissions has been introduced, based on the work of Marticorena and Bergametti (1995) and Kok (2011). Moreover, the three bins describing the size distribution of dust aerosols have been shifted, the parameter settings for in-cloud scavenging efficiencies, and dry deposition and sedimentation velocities have been changed, and the hydrophilic fraction of black carbon emissions has been changed from 20% to 80%. A number of simulations have been carried out for present-day conditions (2004 and/or 1993-2012), both in a free-running mode and in a configuration where

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Interactive Discussion
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meteorological fields are nudged towards ERA-Interim reanalyses. To prevent unrealistically high values of aerosol optical depth (AOD), re-evaporation of aerosols during stratiform precipitation was deactivated in the free-running simulations. The simulations are evaluated by comparing with results from the MACC aerosol reanalysis, the aerosol climatology of Kinne et al. (2013), total AOD retrievals from MISR, MODIS, and AERONET, and vertical extinction profiles from CALIOP.

The developments presented in this study are a first step in the development of an aerosol scheme in CNRM-CM. Understandably, the degree of complexity of aerosol schemes to be used in climate models to be used for long, centennial-scale simulations is a compromise between model quality and computational performance. In this case, the choice was made to stay with the simple scheme already implemented in IFS, and introduce some modifications where needed. This has some obvious advantages, but also some important drawbacks. In particular, the production of sulphate from gaseous precursors and the ageing of the various aerosols cannot be realistically described with such a model. Moreover, the current scheme does not include any description of ammonium nitrate, which is expected to become of growing importance during the course of the century (e.g. Bellouin et al., 2011). Despite these deficiencies, the developments reported in this study are a worthwhile contribution to the development of CNRM-CM. In general, the description of the model and the presentation of the evaluation results are adequate, although the formulation can be improved in various places (see comments below). The authors have tried to evaluate their aerosol simulations using various sources of data. This gives some reasonable insight in some important aspects of the simulations, but some additional budget analyses would be helpful.

Specific main comments:

1. The new description of mineral dust is presented as an important improvement. However, compared to the ranges derived from the AeroCom models analyzed by Huneeus et al. (2011), the new dust emission module produces very high emission to-

GMDD
7, C2366–C2381, 2014

tals (both globally and in most regions shown in Table 3), especially in the free-running simulation. To substantiate their claims, the authors should include an evaluation of the dust size distribution. This can be done by comparing Angstrom coefficients obtained with the new and original scheme against measurements from selected AERONET stations, dominated by dust.

Moreover, one of the reasons for replacing the original scheme is that it did not performed well. For this the authors refer to "preliminary results using the original GEMS/MACC dust scheme". This should be explained in more detail. If possible, the authors should include one or more references to scientific papers in which the quality of the original scheme is analyzed.

Similarly, it should be explained in more detail why the bins describing the size distribution of mineral dust have been shifted compared to the original description. Are there any references the authors can refer to?

2. A more detailed analysis of the global budget of the different aerosol components should be included. Currently, the authors compare global mean mixing ratios for the 12 aerosol tracers in the simulations with results from the MACC reanalysis. They should also compare the simulated global loads/burdens and lifetimes or deposition rates for both wet and dry deposition (the latter including sedimentation) for the different aerosol components with ranges estimated by other models (e.g. Textor et al., 2006; Tsigaridis et al., 2014). This will also be helpful to evaluate the impacts of the change of the parameter settings shown in Table 4.

3. In Figs. 6-9, the authors show maps of simulations results next to maps of climatological datasets from MODIS Aqua, MODIS Deep Blue, MISR, and Kinne et al. (2013). Rather than showing the AOD maps for these evaluation datasets, Figs. 6 and 7 should show the AOD map for the simulation in the first panel, and in the other four panels show the bias between the simulation and each of the evaluation datasets. Figs. 8 and 9 could then show the corresponding relative biases, rather than the rela-

GMDD
7, C2366–C2381, 2014

tive differences between the evaluation datasets. The sign of the biases in these maps should be opposite to that currently shown in the top left panel of Figs. 8 and 9. Instead of showing both absolute and relative biases, the authors can also decide to keep only the absolute biases.

4. In Fig. 10, the authors show maps for both the coarse and the fine aerosol fraction. Since these are complementary (they add up to 1), one of these rows should be removed. I suggest to keep the results for the fine mode, as in Kinne et al. (2013).

5. The comparison of both "anthropogenic sulphate" and "natural aerosols" with the climatology from Kinne et al. (2013) doesn't make sense and should be removed. Without a reference simulation using pre-industrial emissions or the use of tagged tracers, it is impossible to diagnose the anthropogenic sulphate and natural aerosols from the presented simulations, so including such a comparison and concluding there are large discrepancies is only misleading. Thus, the last two rows of Fig. 10 and the corresponding discussions (Page 6284, line 23-25; Page 6285 line 5-15; Page 6292, line 14-17) should be removed.

6. I consider it a weak point that "re-evaporation is not applied in the free-running simulations". Instead of just switching it off, it would have been better to try to solve the underlying problem.

7. The discrepancies between the simulated BC concentrations and the MACC reanalysis, shown in Fig. 1, seem to be inconsistent with the change of the hydrophilic emission fraction from 20 to 80%, as indicated in Sect. 2.3.1 and Table 4. Could it be that these numbers got mixed up?

8. A constant set of dry deposition velocities is assumed, independent of the meteorological conditions. To improve their results, the authors have modified the applied deposition velocities compared to the original implementation. A more realistic approach would have been to extend the description by including the effect of the aerodynamic resistance. This could be mentioned in the final paragraph of the Conclusions.

GMDD
7, C2366–C2381, 2014

9. Please clarify in the text that DMS is emitted as sulfate precursor. In reality part of the sulfur from DMS will be removed from the atmosphere as MSA before it gets converted to SO4. Please include a short discussion of the errors one can expect when this is neglected.

10. The conversion of sulfate precursors to sulfate aerosols is described by a simple exponential function with a time constant depending on latitude only. In reality, the level of oxidants will also depend on the season. For instance, the oxidation of SO2 in the gas phase occurs by reaction with the hydroxyl radical (OH), the concentration of which is strongly dependent on the amount of sunlight.

Minor comments and technical corrections:

1. I suggest to change “CNRM Climate Model” to “CNRM-CM6.1 global climate model” in the title.

2. Abstract, line 2-3: Please change “in the CNRM-GAME/CERFACS climate model” to “in CNRM-CM, the climate model of CNRM/GAME and CERFACS”.

3. Abstract, line 5-6: Remove hyphen in “sea-salt”.

4. Abstract, line 9: Change “of 2004 conditions and” to “time slice simulations for 2004 conditions and”.

5. Abstract, line 13-15 / Page 6279, line 23-25 / Page 6291, line 19-20: Why does the internal variability have little impact on the seasonal climatology of the AODs? In my opinion, low internal variability just means that small ensembles or short time-slice simulations are sufficient to calculate the model’s climatology. Please clarify or rephrase.

6. Page 6265, line 7: Change “largely” to “strongly”.

7. Page, 6265, line 10: Remove “the” in “the aerosol-cloud effects”.

8. Page 6265, line 12: Change “of aerosol distribution” to “of the aerosol distribution”.

GMDD

7, C2366–C2381, 2014

9. Page 6265, line 22: Remove “/Welcome.html”.

10. Page 6266, line 13: Change “such an issue” to “this issue”.

11. Page 6266, line 16: Change “simulation” to “simulations”

12. Page 6267, line 4: Change “evolution” to “upgrade”.

13. Page 6267, line 6: Change “Integrated Forecast System” to "Integrated Forecasting System”.

14. Page 6267, line 6-8: Change formulation to “forecast models of . . .”, followed by the two institutes.

15. Page 6267, line 10: Change “specificities of” to “changes in” or something similar.

16. Page 6267, line 17-18. Change to “The land surface of ARPEGE-Climat is modelled with”. The current formulation is misleading, because it suggests that SURFEX is run offline.

17. Page 6268, line 6: Replace “onto” by “to”, and specify the reduced Gaussian grid, e.g. “an Nxx reduced Gaussian grid”.

18. Page 6268, line 26: Remove hyphen in “sea-salt”, and change “3 size-bins particles” to “three size bins”.

19. Page 6269, line 2: Change “separate a” to “separate into a”.

20. Page 6269, line 3-4: Change “a sulfate precursor, named SO2, and a sulfate aerosol, named SO4, cohabit” to “a gaseous sulfate precursor, mainly representing sulfur dioxide (SO2), and a sulfate aerosol (SO4) are included”. It is confusing to refer to the precursor as SO2. For instance, in Table 2, SO2 is used for sulfur dioxide only.

21. Page 6269, line 4: Change “adds up” to “adds”.

22. Page 6269, line 6: Change to “The scheme describes a number of physical aerosol processes, including”.

GMDD

7, C2366–C2381, 2014

23. Page 6269, line 8: Please clarify what is meant with “as a function of the aerosol”. Is there a dependence on both aerosol size and type?

24. Page 6269, line 11: According to Sect. 2.3.1, in the original scheme described here not 80% but 20% of the black carbon is assumed hydrophilic, so these percentages should be interchanged.

25. Page 6269, line 14: Change “chemical species” to “explicity chemistry”, and “but is done along with an exponential function” to “but is done assuming exponential decay”.

26. Page 6269, line 19: Change “model lowest level” to “lowest model level”.

27. Page 6270, line 3-6: Move this part up, e.g. it can be included at the beginning of the third paragraph of this section. Also, change “transports” to “transport”.

28. Page 6270, line 8: Change “the list” to “a list”.

29. Page 6270, line 26-27. Please clarify what is meant with “The efficiency of scavenging rates corresponds to the lowest values of Textor et al. (2006).” I don’t see how this is achieved, given that these efficiencies depend on the aerosol tracer, and that these are model specific.

30. Page 6271, line 1: Change “Huneeus et al. (2007)” to “Huneeus (2007)”.

31. Page 6271, line 9: Please also give the size boundaries of the bins.

32. Page 6271, line 27: Change “normalized” to “normalization”.

33. Page 6272, line 1: Change section title to “Prescribed anthropogenic and natural emissions”.

34. Page 6272, line 8: IPCC has already been introduced in the Introduction. Better to refer to the IPCC reports as “Assessment Reports” in the introduction as well.

35. Page 6272, line 18: Remove “Apart from these anthropogenic sources”.

36. Page 6272, line 23-25: It is mentioned that the applied climatology of DMS emis-

GMDD
7, C2366–C2381, 2014

sions has the same temporal and spatial characteristics as the data set applied for volcanic emissions. If that is indeed the case, it cannot be correct. Please clarify.

37. Page 6273, line 6-7: Change “considered” to “included”. It should be stated that the total emitted sulfur remains unchanged.

38. Page 6273, line 10: “noting that the option was rejected”. It is hard to believe that the possibility that any of the sinks is overestimated, even at a regional level, can be excluded. Please adapt this statement.

39. Page 6273, line 22-23: Clarify in the text why the sulfate precursor emissions are scaled and why biomass burning is excluded here. Change “excepted” to “except”.

40. Page 6273, line 26-28: Doesn’t that mean that the scaling factors applied to the emissions from biomass burning are too high. Please clarify.

41. Page 6274, line 2: Change “small” to “relatively small”. In fact, the impact is not that small, because doubling the amount of SOA emissions from Dentener et al. (2006) would give 38.2 Tg OM/yr, while according to Table 2 only 34.7 Tg/yr is emitted in the model. Please explain what causes this difference.

42. Page 6274, line 6: Remove “in” in “in within”.

43. Page 6274, line 7: It is not true that “both the intra and inter-annual variabilities come from the biomass burning emissions”. Natural emissions also have seasonal variability, and trends in anthropogenic emissions contribute to interannual variability. Please adapt the text. Also change “variabilities” to “variability”.

44. Page 6274, line 21: Change “consists in” to “consists of”.

45. Page 6274, line 22: Change “with a spectral” to “with spectral”.

46. Page 6274, line 25: Remove “as classically in nudged simulations”, and include the reference to Zhang et al. (2011) to the end of the sentence.

GMDD

7, C2366–C2381, 2014

47. Page 6275, line 1-2: Change “the comparison of modelled aerosols is the most realistic one” to “modelled aerosols are most realistic”.

48. Page 6275, line 6: Change “Nudging, or not, the humidity” to “Whether or not humidity is nudged”.

49. Page 6275, line 13: Change “that are our evaluation sets” to “used in our evaluation”.

50. Page 6276, line 4: Please clarify if the anthropogenic emissions applied in the MACC reanalysis as the same as the data sets used in the simulations presented in this study. If so, this should be mentioned in Sect. 2.3.3. If not, what are the differences?

51. Page 6276, line 8-9: Were there really no sulfur emissions from volcanoes or oceans used in the MACC reanalysis? Seems strange, so please check this. Also, change “volcanos” to “volcanoes”, and “no specific direct H2S or sulfate” to “no direct sulfate”. Note that because there is no H2S tracer in IFS, it should be obvious that direct H2S emissions were not included in the reanalysis. Moreover, the only H2S emissions accounted for in the ARPEGE-Climat simulations are from volcanoes. If these were not included in the reanalysis, this implies that H2S emissions were not considered.

52. Page 6276, line 12: Also give corresponding resolution in degrees, as before.

53. Page 6276, line 20: Change “largely used in the modelling aerosol community” to “widely used in the aerosol modelling community”.

54. Page 6276, line 21: Change to “monthly product of total AOD at 550 nm”.

55. Page 6276, line 27: Change “Kan” to “Kahn”

56. Page 6277, line 6: Remove “(1o)”.

57. Page 6277, line 7: Remove “courtesy of B. Koffi” and include a statement in the Acknowledgments.

GMDD

7, C2366–C2381, 2014

58. Page 6277, line 23: Include space after “using the”.

59. Page 6277, line 28-29: Include “monthly” before “climatology”.

60. Page 6278, line 6: Change “issued from” to “produced by”.

61. Page 6279, line 8: Remove “timescale”.

62. Page 6278, line 15: Change to “As a preliminary step, we looked”.

63. Page 6278, line 17: Change to “mean global monthly” to “global monthly mean”.

64. Page 6279, line 3: Change “ARPEGE-Climat internal variability” to “internal variability in ARPEGE-Climat”.

65. Page 6279, line 4: Change “FreSImd2” to “FreSimd2”.

66. Page 6279, line 5: Change “response of” to “variability in”.

67. Page 6279, line 10: Change “over west” to “west”.

68. Page 6279, line 11: Include “, respectively” after “DD”.

69. Page 6279, line 18: Change “of the central” to “in the central”.

71. Page 6279, line 20: Remove comma after “large”, include comma after “seasons”.

72. Page 6280, line 4: Remove quotation marks around “bins”, here and in other places.

73. Page 6280, line 13: Change “release suppressed for” to “which is suppressed in”.

74. Page 6280, line 18-20: However, also the distributions of SS and DD determine the relative impact of wet scavenging.

75. Page 6281, line 10: Change “other three simulations” to “other two simulations, as well as the MACC reanalysis”.

76. Page 6281, line 15: Change “of proportion of bare soil” to “for the bare soil fraction”.

GMDD

7, C2366–C2381, 2014

77. Page 6281, line 22: An enhancement factor of 20.9 is enormous. Can the authors explain why they expect it to be reasonable?

78. Page 6282, line 4: Remove “brother”.

79. Page 6282, line 8-10: Please include a reference to the study where this is shown. Is it Cesnulyte et al. (2014)? Also, I would propose to already include such a statement in Sect. 2.3.2.

80. Page 6282, line 14: Change to “fairly made as an unrealistic hydrophilic/hydrophobic fraction was assumed”.

81. Page 6282, line 16: Change “tropospheric “bin” concentrations” to “tropospheric binned concentrations”.

82. Page 6282, line 21: Correct “three dust bins” to “two coarser dust bins”.

83. Page 6282, line 25: Change “lat-lon plots” to “global maps”.

84. Page 6282, line 26-27: The authors claim that the transport is more efficient with the meteorological fields in the MACC reanalysis. However, this cannot be concluded, because also the representation of the aerosols is different, e.g. their size distributions.

85. Page 6283, line 3: Remove “within continents”.

86. Page 6283, line 5: Change “SS of” to “SS in”.

87. Page 6283, line 6: The claim that the new dust scheme performs much better than the original one” is not substantiated sufficiently (see main comment above).

88. Page 6284, line 1: The fact that the model performs better in JJA than in DJF could be related to the fact that the time constant for sulphate production is assumed independent of the season (see main comment above).

89. Page 6284, line 1: Change to “mean relative bias”, implying that the relative biases are averaged.

GMDD

7, C2366–C2381, 2014

90. Page 6284, line 2: Since these percentages indicate negative biases, a minus sign should be included.

91. Page 6284, line 6-7: Please comment on the positive biases observed over the Arabian Sea and South America in summer.

92. Page 6284, line 22-25: Change “the fine mode ... aerosols).” to “and the fine mode.” (see main comment above).

93. Page 6285, line 19: Change “aerosol “bin” AODs” to “binned AODs”.

94. Page 6286, line 2: Change “Kanpur North India” to “Kanpur, northern India”.

95. Page 6286, line 6: Change to “around 1”.

96. Page 6286, line 7: Change “model outputs is that, (...), the model shows a nul to low bias” to “model is that, (...), it shows a low bias”.

97. Page 6286, line 22: Change to “The underestimation”.

98. Page 6286, line 24: Change to “under the influence of dust storms”.

99. Page 6287, line 22 / Page 6293, line 1: A correlation coefficient of 0.5 means that 25% of the variance is explained. It would be better to use a higher threshold value to distinguish good from bad performance.

100. Page 6287, line 28: Change “badly” to “poorly”, and remove the quotation marks.

101. Page 6288, line 1: Change “quality” to “data quality”.

102. Page 6288, line 7: Remove quotation marks around “well” and “near-by”. Remove hyphen in “nearby”.

103. Page 6288, line 5: Change “thumbnail” to “graph”.

104. Page 6288, line 9: Change “correctly” to “well”.

105. Page 6288, line 10-12: Rephrase this sentence and remove the last part.

GMDD

7, C2366–C2381, 2014

106. Page 6288, line 13-15: Why are these station names written with capital letters? If not too much work, change the names in Figure 14 and in the text.

107. Page 6288, line 14: Change “badly” to “poorly”.

108. Page 6288, line 17: Are all four self-references needed?

109. Page 6288, line 18: Change to “investigation regarding specific conditions, representativity, and quality of the site, which”

110. Page 6288, line 21: Change “repartitions of” to “component contributions to”

111. Page 6288, line 22: Change section title to “Evaluation of vertical distributions”.

112. Page 6288, line 27: Change “We output” to “We show”.

113. Page 6289, line 7: Remove “to total aerosols”.

114. Page 6289, line 8: Change to “the model is biased low”

115. Page 6289, line 9: Change “quasi-nul” to “insignificant”.

116. Page 6289, line 11: Please clarify why CAT is mentioned here.

117. Page 6289, line 17: Change “load” to “extinction”.

118. Page 6290, line 5-6. Please explain why this S curve shape is not observed in the free-running simulation.

119. Page 6290, line 12: Change “correct” to “good”.

120. Page 6290, line 12-14: Please also mention that the agreement is bad in all other regions.

121. Page 6290, line 17: Please change “the climate model” to “the CNRM-CM global climate model”.

122. Page 6290, line 18: Change “aerosol AODs” to “aerosols”.

GMDD

7, C2366–C2381, 2014

123. Page 6290, line 21: Change “from 2005” to “since 2005”.

124. Page 6290, line 23: Change “12 bins” to “twelve tracers” (or “12 tracers”, if you prefer).

125. Page 6290, line 26: Change “Large” to “Large-scale (advection)”

126. Page 6291, line 1: Change “transports” to “transport”.

127. Page 6291, line 5: Change “(1995); Kok” to “(1995) and Kok”.

128. Page 6291, line 7: Change “as a common” to “as is common”.

129. Page 6291, line 16: Change “site as from” to “site, from”.

130. Page 6291, line 19-24: In this paragraph, it should also be mentioned that there is a problem with the re-evaporation in the free-running simulation.

140. Page 6291, line 27: Change to “by 14 using the new scheme”. Correct “dependant” to “dependent”.

141. Page 6292, line 1: Change to “The spatial distributions”.

142. Page 6292, line 4: Change “static” to “prescribed”, and remove quotation marks.

143. Page 6292, line 9: Change “simulation” to “model”.

144. Page 6292, line 13-17: Change to “underestimates the coarse fraction over continents, except over dust emitting areas.” Remove the next sentence (see main comment above).

145. Page 6292, line 18: Change “the various aerosol types” to “total AOD”.

146. Page 6292, line 22-23: Change to “a systematic low bias”.

147. Page 6292, line 23: Change to “This seems to”.

148. Page 6292, line 25: Change “close to 0” to “small”.

GMDD
7, C2366–C2381, 2014

149. Page 6293, line 4-5: Change to “comparing for summer and winter total and dust extinction”.

150. Page 6293, line 8-9: Change to “However, most regions”.

151. Page 6293, line 9-10: Remove “but there ... large”, since this is no excuse for the discrepancies found.

152. Page 6293, line 11: Change to “this simple prognostic aerosol scheme is promising”. Given the deficiencies in the descriptions of both natural and anthropogenic aerosols, I don’t think it is fair to say that the current scheme is suitable for aerosol-climate studies (see main comment above).

153. Page 6293, line 14-18: Change sentence to “Over the continents, there is room for improvement in the modelling of SOA, and the inclusion of a simple sulfur cycle, using prescribed monthly distributions of oxidant fields (e.g., OH, O3, and H2O2), could improve the description of sulfate, which is of primary interest to climate.”.

154. Page 6293, line 14-18: Please also mention that the current scheme does not describe nitrate, which is expected to be of growing importance (see main comment above).

155. I would propose to re-order the Tables and Figures following the order in which they appear in the text.

156. Table 1: Please change to “dust emission scheme”.

157. Table 2: Change to “Prescribed emission totals”. Remove “Range” after “1993-2012”. Correct “litterature” to “literature”.

158. Table 4: I suggest to include the full variable descriptions in the table entries, rather than in the caption. Change “Efficiency for scavenging” to “Efficiency for in-cloud scavenging”, and include “by” before “rain” and “snow”. Please explain the meaning of dust emission potential in the main text. Please indicate that the hy-

GMDD

7, C2366–C2381, 2014

drophylic/hydrophobic ratio is applied to emissions only. This should also be clarified on page 6271, line 6. Please also indicate on page 6269, lines 9-12, that ageing of OM and BC is included using a constant conversion rate from the hydrophobic to the hydrophilic fractions, and refer to the table for the value assumed for this. What is the unit of this conversion rate given in the table? Please check it.

159. Figure 1: Sea salt seems to be more sensitive to the meteorology than mineral dust. Is this explained in the text? Are the differences between the simulations and the MACC reanalysis consistent with the previous evaluations of the MACC reanalysis? Please comment in the text.

160. Figure 4: Change “Mean 2004 dust AOD” to “Mean dust AOD for 2004”. Is it explained in the text why the dust AOD pattern obtained with the new emission scheme is much more inhomogeneous than with the old scheme (this Figure) and the MACC reanalysis (Figure 5)? If not, please do so.

161. Figures 11, 12 and 14: If it is not too much work, can the underscores be removed in the station names?

162. Figure 14: Change “good performing” to “well performing”.

163. Figure 15-17: Please indicate the full names of the regions, and increase the size of these Figures.

164. Unless specified otherwise above, please change “specificities” to “characteristics”, “specific features” or similar words, and “outputs” to "output" throughout the paper.

Interactive comment on Geosci. Model Dev. Discuss., 7, 6263, 2014.