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Geoscientific
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Discussions

Interactive comment on "Description and evaluation of GMXe: a new aerosol submodel for global simulations (v1)" *by* K. J. Pringle et al.

Anonymous Referee #2

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General comments:

The paper presents the aerosol submodel GMXe which is a very comprehensive aerosol module for use in global models. GMXe is very flexible to be adjusted for applications with different scientific and technical objectives. The current version of the model allows for particularly detailed descriptions of both bulk aerosol species and complex inorganic solution aerosols. The paper provides very detailed descriptions of relevant aerosol processes. The quality of the model is demonstrated by a large number of comparisons of model results with observations allowing for a thorough model evaluation.

Since the development and evaluation of an atmospheric model is the focus, the paper is well suited for publication in GMD. The paper is generally of good technical quality.

Model concepts and evaluation results are clearly presented. I recommend publication of the manuscript after the following comments have been addressed by the authors.

Major comments:

Comparability aspects of the model and the observations have to be considered in more detail. In particular the following aspects have to be addressed:

1. If measurements during specific episodes are compared to the model output, the specific episodes should be extracted from the simulations. Comparing annual means from two specific years to data from shorter time periods (of possibly other years), as presented in Figure 3, could lead to a systematic bias since the respective episodes might show specific dynamical or chemical features which might not be consistent with the two-year mean. The authors argue that the comparison is only qualitative in such cases but that does not lead to a robust evaluation and any interpretation of the results might be meaningless. The authors should therefore think about presenting only those comparisons which can be interpreted from a quantitative point of view.

2. Also the comparison to the mass spectrometer data (section 4.4) suffers from comparing with observational data from years which are not covered by the simulations. The authors could extend their simulations either to cover the times of observation or generate climatological information from the model showing a representative state of the aerosol. Means and variability gained from a longer term simulation (e.g. over 10 years) could be compared even to observations from time periods not covered by the model. In such a case, the observed values should be covered by the model variability. Only in cases where a low inter-annual variability of the considered quantities can be expected, the comparison as presented in the paper might be justified, but this has to be discussed in the paper and the authors should explain why only the two years were simulated. It should also be taken into account that changing emissions could cause discrepancies when different time periods are compared. This can be particularly important also in the case of the AEROCE data comparison where different decades are

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considered (section 4.5).

3. Sea-salt and dust emissions are chosen according to offline emission data sets. Are these data consistent with the specific observation periods considered? The authors should discuss in section 4.2.1 whether this could be a source of systematic bias. If so, alternative simulations over a longer time period using the on-line emission approaches might help to evaluate the uncertainties inherent in the dust and sea salt simulations.

Minor comments:

1. Model name: What is the meaning of the 'e' in the acronym GMXe? The model name would rather suggest the acronym GMeX.

2. The use of the term 'sea spray' instead of the commonly used 'sea salt' has to be explained.

3. Introduction section: The authors highlight the comprehensive representation of nitrate in their model. It could also be mentioned in the introduction, that ammonium is considered explicitly, since this is another advantage over other models which only consider sulfate and refrain from explicit considerations of nitrate and ammonium.

4. Section 2.1: The years which have been simulated should be specified here.

5. Page 573, line 24: Write '(initiated by both. . .' instead of '(both . . .', since in-cloud scavenging leads to wet deposition only when precipitation occurs.

6. Section 2.4.1: The authors should explain why they refrain from calculating dust and sea salt emissions on-line. Are the uncertainties of such on-line calculations larger than those of the applied emission climatologies?

7. Page 576, line 5: Provide reference for this kind of splitting.

8. Page 576, line 16: refer to section 3.2.1 and Table 2.

9. Section 3.2.1: The mathematical definition of the lognormal distribution should be

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provided here since it represents the basic concept of the model, at least a reference should be included.

10. Section 3.2.2: The relative humidity might also be a driving parameter, in addition to temperature and sulfate concentration.

11. Page 577, line 8: The authors should discuss why sigma can be fixed. Is it justified since sigma shows a much lower variability than number and mass?

12. Page 579, line 8: Replace 'aqueous species' by 'aqueous solutions'?

13. Page 579, line 14: Provide example for an 'aerosol metastable, inverse/forward problem'.

14. Page 579, lines 17,18: Specify which of the studies (references) deal with global, regional, and urban-scale modelling.

15. Page 581, line 24: A quantitative example for the coarse mode fraction of total nitrate should be given as a motivation.

16. A reference for the n-monolayer thresholds should be provided. Are there any laboratory studies which would support such assumptions?

17. Page 585, line 4: mention that also EQSAM was applied as an alternative.

18. Page 585, line 15: 'zonal mean annual average'.

19. Section 4.1: it should be discussed in more detail why the evaluated number concentrations by Stier et al. can be used as a reference point here. What are the differences to Stier et al.? Can they be neglected? The authors only quantify the largest differences (coarse mode). For evaluation purposes it might be useful to quantify the maximum deviations in the other modes.

20. Section 4.2: Explain why using different metrics (GMR, AMR, ...). Which specific conclusions can be drawn from the specific metrics?

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21. Page 587, line 1: ‘percentage of model point where the respective species concentrations deviate from the observations by less than a factor of two’.

22. Section 4.2.1, discussion of Fig. 6, Tab. 3: The modelled BC concentrations over North America show nearly perfect agreement with the IMROVE observations. This is remarkable regarding the comparatively high uncertainty inherent in current BC emission data. It is also remarkable since many IMPROVE stations are located in rural areas. Hence deviations from the modelled mean concentrations (including also concentrations in highly polluted areas) must be expected. The authors therefore should discuss that the perfect agreement might not indicate highest achievable model quality.

23. The presentation of comparisons with large-scale observation network data in section 4.2.1 is inconsistent with the title of section 4.3. Hence the title should be changed.

24. Section 4.2.2, last paragraph: discrepancies can also arise from different representation of precursor chemistry and different NOx emissions.

25. Page 589, line 19: Provide examples for such problems and/or a reference.

26. Page 589, last paragraph: Add sentence ‘The details of the evaluation are discussed in the following.’

27. Section 4.3: Information on the time periods covered by the observations from the specific networks and the time periods considered for model evaluation should be included.

28. Page 590, line 2: insert ‘concentration’ behind ‘sulfate’.

29. Page 591, line 7: explain why dust is important here.

30. Section 4.3.3: It should be discussed whether uncertainties in nitrate could also result from uncertain gas phase precursor chemistry or NOx emissions.

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31. Page 592, lines 19, 20: Even a perfect agreement of model and observations might not attest high model quality since differences might have to be expected, regarding the differences in meteorology.

32. Page 596, lines 16, 17: Is this in agreement with the changes in sulfur emissions which have been large during the last decades?

33. Section 4.7: Since the results from EQSAM and ISORROPIA mostly agree, it would be worthwhile to discuss also technical aspects like the computational expenses or technical advantages/disadvantages of the modules.

Editorial changes:

1. Page 577, line 7: '(the latter of each component)'.

2. Page 579, line 15: Replace 'option' by 'options'?

3. Page 580, line 12: Replace 'for' by 'form'.

4. Page 581, line 2: delete 'there'.

5. Page 582, line 25: delete 'when'.

6. Page 587, line 4: delete 'and 5' since figure 5 is discussed in section 4.2.2.

7. General: write South (North) America instead of S. (N.) America

8. Page 587, line 8: over India.

9. Page 588, line 9: (2005, 1.1??)

10. Page 588, line 15: delete 'also'. Insert 'tropospheric' (column).

11. Page 588, line 18, '>2': insert unit.

12. Page 594, line 5: 'For the latter, we take values ...'

13. Page 594, line 9: ' and Tsigaridis . . .'

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14. Figure 2: Plots should be enlarged. Caption: Replace 'at STP' by 'converted to STP conditions, i.e. ...)'

15. Figure 3, caption: mention that concentrations are given for surface level.

16. Figures 4,5, captions: mention that annual means are shown.

17. Figure 6, caption: no triangles and crosses are visible.

18. Figure 7a: Replace 'Annual mean' by 'Simulated annual mean'.

Interactive comment on Geosci. Model Dev. Discuss., 3, 569, 2010.