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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol simulation applying high resolution anthropogenic emissions with the EMAC chemistry-climate model" *by* A. Pozzer et al.

Anonymous Referee #2

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General comments:

The paper reports on the application of a novel high resolution global anthropogenic emission data set in the global EMAC model system. Global simulations are discussed, with a particular focus on the model representation of atmospheric aerosol. The results of a detailed comparison of the model results with observational data as well as an analysis of global and regional budgets of specific aerosol components are presented. In addition, the effects of applying emission data with monthly temporal resolution are analyzed.

Uncertainties in emission data are one of the major limitations in global aerosol mod-

elling. Hence, the application of highly resolving sate-of-the-art emission data in global simulations is an important step. The analysis of the global and regional aerosol budgets presented in this study provides important new information since the contributions of different particle sources and sinks to such budgets are currently not well known. Unfortunately the paper misses a clear focus and some of the methods applied need to be improved.

The paper deals with three major subjects. i) A model set-up including the new emission data base is evaluated, ii) global and regional aerosol budgets are analyzed, and iii) effects of resolving the seasonal cycle of emission fluxes are discussed. As revealed by the title and the conclusions the main subject is intended to be the application of the highly resolving emission data. Hence I would expect that the key objective would be the demonstration whether using these emission data has a benefit over applications of conventional emission data. This, however, is not really demonstrated because a comparison with model runs using conventional emission data is missing. The only step in this direction is the quantification of the effects of considering the seasonal cycle of the emissions. This, however, suffers from a missing analysis of statistical significance (see below). The analysis of the budgets does not necessarily require the new emission data and is not considered in the title and not even discussed in the conclusions. So it seems somewhat displaced here, although it is the only part of the paper which provides new results with regard to atmospheric processes.

The application of highly resolving emission data is, in my opinion, not very well suited as the major objective of a publication in ACP (probably better suited for GMD) since technical model improvements are the focus. The quantification of global and regional aerosol budgets would be much more appropriate as the main objective since it focuses on atmospheric processes, rather than technical model innovation. Another main focus could be the detailed analysis of the aerosol seasonal cycles, which is already a subject of section 5, but which could be highlighted in much more detail, for instance, by including figures showing seasonal variations or even discussions on the seasonal

ACPD
11, C11274–C11282, 2011

variations of the budgets.

I would, therefore, suggest that the major focus of the paper will be shifted towards these issues and that the highly resolving emission data would be highlighted as a technical improvement to enhance simulation quality, particularly with regard to the aerosol seasonal cycle. To keep the paper balanced, the model evaluation part should be shortened, particularly by removing some plots, for instance, the scatter plots or even the Taylor diagrams since the spatial distribution can also be analyzed on the basis of the horizontal distribution plots.

With regard to the benefit of using the new emission data, some clarifying discussions should be included. It is not clear to me why it is an advantage to use 0.1°x0.1° resolving emission data in a model with 1°x1° model resolution. The additional information of the highly resolving emissions is lost and conventional emission data providing 1°x1° resolution may be sufficient for the model. Hence the key improvement achieved with the new emission data seems to be the consideration of the seasonal cycle. The demonstration of this improvement, however, seems to be inappropriate as outlined below.

Since the model is not a pure CTM (couplings with dynamics via aerosol effects on radiation), the application of simplified (temporally averaged) emission data could have feedbacks on the dynamics causing significant deviations from the reference simulations. Even in a nudged simulation such feedback effects occur in the form of changed meteorology, since the model still generates its individual dynamics. Hence, the differences between the two experiments NT and ST can also be due to these feedbacks, rather than being directly related to the changed emissions only. The size of the feedback effects depends on the degree of nudging and can (in extreme cases) be as large as the interannual variability of the reference simulation. Hence, if the differences between the NS and ST simulations are smaller or on the order of the interannual variability of the parameters considered, they cannot be interpreted as pure emission effects. Instead, they could just be related to changed 'weather' and could average out

ACPD
11, C11274–C11282, 2011

if a longer (multi-year) NS simulation would be analyzed. Even if the differences found here are larger than the variability they can still be affected by feedbacks and a robust quantification of the effects of temporally resolving emission data cannot be achieved. This needs to be urgently admitted in the paper.

The standard way to demonstrate that the differences between the two simulations are statistically significant and probably related to the modified emission data rather than to model noise would be the application of statistical methods as the student's t-test. This, however, cannot be done with simulations of a single year only. The best solution would be to perform a longer (multi-year) simulation NS and prove statistical significance of the differences between NT and ST by statistical means. If this is not achievable, the authors should remove section 5 from the manuscript.

Specific comments:

Title: Replace 'emissions' by 'emission data'.

Introduction: use '(e.g., reference)' if references are just examples and do not represent unique studies on the specific subject.

Page 25210, line 8, MECCA: The chemistry scheme should be roughly characterized. Which sets of species and reactions are considered (inorganic, organic, tropospheric and/or stratospheric chemistry, . . .)?

Page 25210, line 29: Replace "with" by "within".

Page 25211, line 21: Skip '. The references used are'.

Page 25212, line 6: Which volume is used for weighting?

Page 25212, line 14: Explain how the wavelength bands are weighted.

Rage 25212, line 18: Which coefficients are weighted and how is the number used for weighting? Are the number concentrations of the different modes used to weight the optical properties obtained for the individual modes? The optical thicknesses of the

ACPD

11, C11274–C11282, 2011

Interactive Comment

modes should just be added and not averaged.

Page 25212, line 27: The acronym CIRCE should be explained. The project number is probably not relevant here. The Doering et al references should be included directly behind the explanation of CIRCE.

Page 25213, line 7: It seems somewhat critical to me to evaluate the emissions with other emission data, since also these data are uncertain. I would rewrite it towards something like 'The EDGAR-CIRCE data has been compared .... These comparisons reveal ...'.

Page 25213, line 10: include 'data sets' between '2002)' and 'and'.

Page 25214, line 16: This sentence should be included in the above paragraph.

Section 2.3: As for the satellite data, it could be mentioned where the data has been downloaded, if appropriate. References for the EMEP and EANET networks are missing. The data sets gained from these networks should be described in more detail. In particular, the species covered should be listed.

Page 25216, line 22: Replace 'solve' with 'solving'.

Section 3.1: The satellite observations are interpreted as the truth in this comparison. The satellite data products, however, can have large uncertainties. These uncertainties need to be discussed in this section and the conclusions about model quality should be drawn more carefully facing the data uncertainties.

Section 3.1 and Figure 1: In addition to the differences, the model or satellite AOD itself should be shown in Fig 1. Otherwise the reader cannot easily estimate the relative change or the relevance of the deviations. The relative deviations should also be discussed in the text.

Page 25218, line 22: Include 'is' behind 'what'.

Page 25219, lines 10-11: Replace 'more closely to' by 'better with'.

ACPD

11, C11274–C11282, 2011

Page 25219, lines 19-20: It should be explained how the standard deviation is calculated here. Is it the standard deviation calculated from the set of temporal means (over each month) at the different grid points?

Page 25219, line 27: skip 'very'.

Page 25219, line 27: insert 'mostly' before 'below'.

Page 25220, lines 10-11: It should be explained how the correlations were calculated (based on which temporal resolution: monthly, daily, ...?).

Page 25220, line 15: Write out 'w.r.t'.

Page 25221, line 1: Skip 'the'.

Page 25221, line 10: write 'aerosol sulfate, nitrate ...' instead of 'sulfate, nitrate ... aerosols' since the components are internally mixed.

Page 25221, line 13: Which scatter plots are refered to? (Not mentioned yet). Specify or rewrite as 'scatter plots discussed below' or similar. It could be helpful to include a short description of all figures of this chapter before explaining some figures in more detail.

Page 25221, line 16: Which Taylor diagrams are meant here? (Figures should be specified).

Page 25222, line 4: The number of stations in Asia might be too low to draw robust conclusions about the gradients.

Page 25222, lines 9-11: I don't see why the small standard deviations indicate an underestimation of the concentrations. The standard deviations reveal that the variability is lower than observed but a general underestimation cannot be concluded since the Taylor diagrams are based on centered root mean squares and leave out the deviation of the overall means.

ACPD

11, C11274–C11282, 2011

Page 25223, line 3: Even over Europe and the USA the overestimation is quite significant. I wouldn't say that this is a minor deviation. Hence, the sentence should be rewritten.

Page 25223, line 6: Include 'eastern' before 'USA'.

Page 25223, line 14: Replace ';' accordingly.

Page 25223, lines 18-24: I have doubts that a poor correlation necessarily indicates an overestimation. An overestimation can cause poor correlation but poor correlation can also have other reasons. The Taylor diagrams should be interpreted more carefully.

Page 24224, line 2-3: Replace 'NH4 distribution is highly concentrated' by 'high NH4 concentrations occur particularly'. The purpose of the Clarisse et al reference is unclear here. The sentence describes Fig 8, which is not part of the other paper. Perhaps write 'This agrees with the findings of Clarisse..., who demonstrated ...'.

Section 3.2.3: In the section 3.2.2 possible uncertainties in the NH4 measurements are discussed. These uncertainties should be considered when evaluating the model on the basis of the observations here.

Page 25224, line 14: The authors should admit that large deviations occur also over Europe.

Page 25225, lines 20-22: I cannot see in Figure 10 that sodium is overestimated at almost all stations. At many stations the modelled values are lower than observed, particularly over Europe. This is not consistent with Fig 11 (which is not even discussed in the text). Figure 11 reveals that the model shows larger values than observed by EMEP in nearly all cases. What is the reason for this discrepancy?

Page 25225, line 23: replace 'East' and 'North' by 'eastern' and 'northern'.

Beginning of section 4: It should be specified in more detail what kind of budgets are the focus here (e.g. contribution of different processes to total aerosol production and

ACPD

11, C11274–C11282, 2011

loss within specific areas).

Section 4, regional budgets:

1) A figure with a map indicating the specific regions should be included.

2) The information about the budgets is presented only within Table 5, which is quite hard to overview. I would suggest to additionally present the information provided by this table in terms of figures (bar plots). I would include one figure for each of the most essential aerosol components. These plots could show different coloured bars representing the different processes. For each region, sets of such bars could be presented. This would highly facilitate the interpretation of the budgets.

3) The discussion of the budgets should be rewritten by laying more emphasis on the most essential contributions. For instance, the fact that East Asia is a net importer of NH3 is less important regarding the high local emissions, which are not mentioned. Hence, the relevance of each statement should be discussed and most essential points should be highlighted. Obvious features of the budgets, for instance, that Europe is an importer of dust, shouldn't be discussed in detail. New results, as the different dust deposition mechanisms in the different regions, should be highlighted as new findings. It should be emphasized what can be learned from the budgets, beyond the obvious mechanisms.

4) The conclusions should be rewritten by including the major findings of the budget analysis. The discussion of findings from applying the new emission data could be shortened.

Figure 2: The comparison between MODIS and MISR is not discussed in the text. What can be learned from the triangles shown in the figure? The triangles could also be skipped, if appropriate. If they are kept, they should be interpreted. It should also be explained which of the data sets represents the ideal point.

Figure 5: The respective network names should be indicated in the plots, not only in

ACPD

11, C11274–C11282, 2011

the caption, since the captions of Figs 7 and 9 do not directly provide this information.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25205, 2011.

ACPD

11, C11274–C11282, 2011

Interactive Comment