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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sources of organic aerosols in Europe: A modelling study using CAMx with modified volatility basis set scheme" *by* Jianhui Jiang et al.

Anonymous Referee #2

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General comments The paper "Sources of organic aerosols in Europe: A modelling study using CAMx with modified volatility basis set scheme" by Jiang et al. deals with a very interesting topic for modelling science. Indeed, the modelling reconstruction of Organic aerosol fraction remains a challenging issue due to the relevant number of species and processes involved. The paper can surely provide a very interesting contribution to the scientific knowledge in this field, particularly in the European context and therefore fits the scope of ACP. The paper is well written, with concise and clear statements, and it does not require any substantial review of syntax and language.

However, before publication, there are a few issues that should be addressed by the

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Discussion paper

authors, that are detailed in the following:

1) One of the key aspect of the paper concerns the implementation of modified parameterizations in the 1.5 VBS scheme, however from section 2.2.3 is not clear what modifications have been actually introduced beyond the split of the original 5 basis sets into 11. From the text it seems that just two modifications were introduced: a. Setting SOA yield for DN to 0. b. Enabling oxidation of SOA from Biomass burning Is it correct? If yes, the modifications introduced by authors are surely reasonable and interesting, but limited only to a few aspects of 1.5 VBS scheme and this should be better clarified in the text. Conversely if other modifications have been introduced they should be better describe it (maybe introducing a table comparing BASE and NEW VBS parameterizations)

2) Authors point out that one of added values of their work is the evaluation of model results over a long term period. However both meteorological and air quality model performance evaluation (for chemical species other than OA) is limited only to a winter and summer month. Though interesting, such analysis is not fully adequate to evaluate the CAMx performance over the whole year. Moreover, in most cases the selected months (February and July) do not overlap with the observation periods of OA measurements (see table 1). An yearly based analysis of AQ and meteorological model performance should be added. Moreover, considering the observation periods covered by OA measurements a seasonal based analysis could be added too. The latter would also be coherent with several results presented by authors in sections 3.1.2, 3.2, 3.3 and 3.4

3) The “NEW” simulation includes two main modifications, the first one concerning the VBS scheme and the second one related to the estimation of SVOC emissions. It would be very interesting introducing an intermediate simulation where only one the two modifications is implemented (e.g. only the modified VBS scheme either only the chance in SVOC emissions). This could help in better quantifying the contribution of every change to the total concentration variation. The results of such analysis could be

introduced in Table 2 as well as in figure 2 and 3.

4) The analysis of the obtained results allows the authors to conclude that the “NEW” introduces an overall improvement of CAMx performance. I fully agree with their conclusion, however SOA performance still highlights a general underestimation , differently from HOA and BBOA that are reproduced fairly well in “NEW” run. Could this result be specifically related to a possible underestimation of some key precursors such as IVOC? Any comment/additional analysis of this issue?

Specific comments P7 R15-16 Were coarse PM emissions split into EC, Na and SO4=? The default CAMx aerosol scheme (CF) includes only CCRS and CPRM species for the coarse fraction. In case, what aerosol scheme was used?

P8 R5-8 SVOC emissions play a key role on OA processes both in terms of total mass as well as with respect to their volatility distribution. Did the authors introduce also a different volatility distribution in the “NEW” run, beyond increasing the total emissions by a factor of 3?

P8 R22-24 Authors correctly point out that NO2 is underestimated suggesting that the observed discrepancy could be related to a corresponding underestimation of NOx emissions. As NOx emissions are mostly related to road transport could other emissions of the same sector be underestimated too? (e.g. NMVOC. . .). Any possible influence on OM results?

P9 R24-30 CAMx performance in reproducing SO2 are rather poor. Modelled concentrations are strongly overestimated (MFB is higher than 75% in February) and substantially uncorrelated to observed values (IOA is around 0.1). Considering that SO2 emissions are mostly related to “Other anthropogenic sources” do authors think that such overestimation could influence also other species (e.g. NMVOC, PM) and, therefore, also the contribution of this sector to OM concentrations? (see for example conclusions P16 R5-9)

P10 R1-2 Table 2 is a bit misleading because it summarizes the model performance at all ACSM/AMS stations, which cover very different temporal periods. The different rows should be grouped somehow, for example separating stations covering the whole year from sites covering only winter periods, summer periods, etc.. Moreover, it could be useful adding, for each site, the number of available observations, as well as the observation period (though already reported in table I)

Technical corrections P19 R18 Table 1 ?

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