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Interactive Comment

Interactive comment on "European air quality modelled by CAMx including the volatility basis set scheme" *by* G. Ciarelli et al.

Anonymous Referee #2

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The paper describes the application and evaluation of the CAMx model with the Volatility Basis Set scheme used for formation of secondary organic aerosols. The study includes several sensitivity simulations varying the volatility and emission parameters of the organic species. In-general, the study goes along the same lines of several existing applications, some quoted by the authors in the introduction. In that sense, I found little new or innovative pieces in the paper. From the other side, such evaluation exercises are useful for collecting experience with the VBS approach. Till now, it falls short of demonstrating a major breakthrough in the models performance as a reward for high complexity and bulkiness.

The paper is comparatively well written except for the results section 3.

I however noticed a few omissions, some with potentially heavy consequences, which should be brought up.

General comments

The most-important omission is the analysis of the emission dataset. A potentially woeful problem, for instance, is seen from Figure 8, the S1 scenario. The concentration map evidently reproduces the emission distribution, which almost completely misses half of the countries. This is a major caveat of the input dataset, which, if confirmed by the explicit emission analysis, would disqualify the whole exercise: the authors would have to switch to another emission dataset.

From the other side, the authors fell to a frequent modeller's trap of blaming emission for poor model performance, often with thin supporting analysis. Some of these blames may be justified, some may be not. For instance, I found it hard to believe the long discussion in p. 35657, where the authors try to explain the strong systematic NO2 under-estimation – and blamed emission. I found an alternative and much simpler potential explanation: nitrates are strongly over-estimated in most of cases, which would probably make-up for the deficit and suggest problems in the model chemistry rather than emission.

Another weakly presented component is the comparison with other studies. The TNO-MACC emission, EURODELTA, EUCAARI and Airbase archives are usual sources of information for numerous model exercises, not to mention MACC project itself, which covered the considered period with the ensemble of seven models and performed a detailed evaluation against the same Airbase. Numerical results and model scores are available. How does CAMx compare to these? In a couple of places, the authors mention conclusions of other studies but it has to be in a numerical form and made much more systematic.

Among smaller things, I am missing the time correlation coefficient in the list of parameters. It is not only the absolute level that is to be verified, the expensive and complicated VBS mechanism is supposed to deliver better representation of the processes,

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thus improving the patterns and their evolution. The temporal correlation coefficient is arguably the best parameter to reflect it. Fractional error is good but less straightforward and intuitive parameter, also affected by bias.

The naming convention is confusing. The base case is usually number one, from which the sensitivity cases are made. It may look like a small thing but while reading I had to again and again remind myself that S3 is, in fact, the base case.

Section 3 is the problematic one from the presentation standpoint. The text is not structured, subsections are routinely comprised of just one huge paragraph without much logic. I would strongly recommend heavy editing of this section.

Specific comments

The title does not reflect the paper content. This is the model evaluation exercise, not the AQ assessment.

p. 35647, l.15. I found it strange to praise the model for PM2.5 score, which, as shown already in the next lines, is a result of error compensation (l.20).

p. 35648, l.1-3. No, it does not. The only piece shown is that the model appeared sensitive to scaling of the biogenic emission fluxes in one case and anthropogenic in another. The residential combustion is a hypothesis of the authors not directly supported by the study. It still sounds plausible and can be brought up in discussion but not in the abstract and not in the so categorical form.

P. 35651, l.12-13. I did not understand: were the CAMx levels the same as the ones of IFS or not? If they were different, I would challenge the idea of neglecting the interpolation from the IFS levels. The issue should be clarified and explanations provided.

P. 35652- 35653. The emission discussion is unstructured and difficult to comprehend. Splitting the paragraphs to “main” species available from TNO-MACC, biogenics, etc, would help.

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P.35652, l.14-17. How was the split made? As follows from the rest of the paper, amount of organic matter is one of the primary parameters of the study. This vague sentence is part of the most-important weakness of the paper mentioned above: the emission dataset is not analyzed and, as follows from this sentence, is not even presented properly.

P.35653, l.1-10. I did not understand: did the authors run MEGAN themselves, including preparation of the land use specifications, emission factors, etc? From the text it seems so ("were prepared for this study") but then, what was wrong in the native MEGAN setup? And how the changes suggested in this study modified/improved its performance? Did the authors make this analysis?

P. 35654, l.4. "Further aging" from what stage? And why was the ageing stopped? Just because then the model over-estimates the SOA, as stated in the paper? But this cannot be the reason, it is artificial and model-dependent. Is there any physical/chemical ground or hypothesis?

P. 35655, Statistical methods. These formulas are from textbook. One can put them to appendix for the sake of completeness but this sub-section definitely should be eliminated from the main paper.

P. 35656- 35658. Almost two pages of plain unstructured text, all in one (!) paragraph. I tried several times and still had problems in pushing myself through it.

P. 35656- 35658. It also looks like the authors do not really pay attention to the physical and statistical meaning of the metrics used. As said in the general comments, mean error is heavily controlled by bias when the latter is large. An independent quantity would be correlation coefficient.

P. 35659, l.13. Another praising the model for meeting totals by a mere error compensation. Not sure if this is a big achievement.

P. 35659 – 35661. . . and another 2.5 pages in a single-paragraph of unstructured text.

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P. 35665, l.20. . . .and again “total PM2.5 was modelled very well“, for a change without a reference to error compensation. I have strong difficulties with such presentation style.

Figure 6: what panels are for what parameter? The axis font is much too small to figure it out.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 35645, 2015.

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