

Review of *"The Eulerian urban dispersion model EPISODE. Part II: Extensions to the source dispersion and photochemistry for EPISODE-CityChem v1.2 and its application to the city of Hamburg"* by Matthias Karl et al.

The submitted work describes the EPISODE model with the CityChem extension. When I read the manuscript was part 1 of the article series not available, which makes it somewhat difficult to assess the need and value of the the CityChem extension. The reader of this particular manuscript is also left with an incomplete description of the EPISODE-CityChem system.

EPISODE provides a promising set up with sub-grid scale Gaussian models embedded into a Eulerian model with relevant photochemistry. As understood from the present manuscript does the CityChem extension offer three improvements compared to the original EPISODE model:

1. Modification of the sub-grid photochemistry scheme
2. Modification of the line source emission model
3. Modifications to the plume rise calculations

The three improvements are explained and visualised by presenting results from idealised sensitivity studies. In my view do the authors not quite get to the point when the respective improvement is presented and visualized.

-Section 2.1 discusses a number of chemical schemes. From my reading of section 2.1 can I not tell what photochemical module(s) is (are) actually used in EPISODE-CityChem. Section 3.2 compares the performance of the updated photochemical scheme in the EPISODE-CityChem system with the standard PSS assumption. The conclusion (not shown in any graph!) is that the new scheme (EP10-Plume) behaves very similar to the original PSS-scheme. This is an interesting and valuable finding, but does not provide support for introducing an alternative photochemical scheme in the original EPISODE model.

-I found section 2.2.1 -describing the improvements of the line source emission model particularly difficult to follow.

-Section 2.2.2 details the calculation of wind speed at the actual plume height, and is a good reference for documenting how winds are calculated in the EPISODE-CityChem model although it is basically a standard similarity approach. As part 1 of the article series is not yet available it is impossible for the reader to know whether the "WMPP" is the sole improvement to the original EPISODE model or if the whole concept of plume rise calculations (Briggs, 1969; 1971; 1975) are also part of the development.

The set up of the Hamburg domain is illustrated with a figure of the domains (Fig. 6) along with the text in section 4.1. It is still very difficult to understand the whole modelling chain of EPISODE-CityChem (and the other models participating in the comparison). For boundary concentrations of air pollutants there appears to be 2-3 CMAQ nests inside the global FMI APTA chemical transport model. Similarly, is meteorology dynamically downscaled from ERA5 in, at least, five nested TAPM domains. An impressive undertaking, indeed, but rather bulky and inconvenient for most applications.

The location of the Hamburg study domain in the Southwest corner of the 4km x 4 km CD04-CMAQ domain does not make sense. The CD04 values at this point will hardly be any different than in the parent CD16 model, as the boundary is only 20-30 km away (less than an hour with a typical wind speed). I do not agree with the authors that: "This is considered to be sufficient to avoid that concentrations in the study domain are affected by domain border effects" (page 18-19, lines 34 and 1, respectively).

Many of the results presented are based on 1-2 week simulations (including, for example, section 4.1.2 and 4.1.3 and the final ~30 rows of section 4.2). I would be reluctant to draw any major conclusions from such limited data series.

The description of the “second experiment” involving the TAPM air quality model (P21, L6-17) is difficult to follow. Why only evaluate 14 days? I also lack a brief discussion on why the model results differ. Is it due to different emissions or - for CMAQ, the seemingly best model- different meteorology? For the full 12-month evaluation (Figs 8,9,11 and Tables 8-10) is only EPISODE-CityChem compared against observations. Why is not the three-model comparison (Table 7) extended to the full 12-month period?

In this section 4.1.2 the high-resolution EPISODE-CityChem is compared with two coarser-resolution models for a two-week simulation over the city of Hamburg. As far as I can judge from the presentation is CMAQ superior to EPISODE-CityChem, which is interesting, since CMAQ is used as boundaries for EPISODE-CityChem. The test is not a good promotion for EPISODE-CityChem.

The model performance evaluation of EPISODE-CityChem using FAIRMODE DELTA Tool is brief, but difficult to follow (Figure 9 and lines 1-19 on page 26). It could perhaps be dropped (or moved to the Supplement as a stand-alone entity), in favour of a more focussed presentation.

The manuscript is rather long but at the same time superficial. I would like to recommend the authors to focus and streamline the presentation.

There are several annoying features that distracts the reader from truly appreciating the presentation. For example:

- a. As already pointed out is part 1 of the article series, not yet available. Still, the text frequently refers to Hamer et al. for explanations of background and details omitted in the presentation.
- b. The manuscript is long and the average reader lose focus after a while. Consider shorten and remove some parts:
 - TWOSTEP is unnecessary mentioned and explained several times.
 - There is no need, in my opinion to describe, in great detail, the interpolation and file format conversion of the boundary data to the EPISODE model (section 2.3.1).
 - What is the rationale for testing the EMEP45, EmChem03-mod and EmChem09-mod in this presentation. Doesn't that belong to the standard EPISODE model (part 1 of the article series?).
 - The section of comparing the new and standard K(z) profile (4.1.3) is not suited for this manuscript as the new K(z) method is described in part 1 of the article series. It also distracts the reader from the CityChem extensions -which should be the focus of this manuscript.
- c. I don't think it is good practice to discuss results in the main text that are only presented in the Supplement. This happens throughout the manuscript.
- d. The order of how items are presented and discussed is stochastic and confusing for the reader. Check, for example, section 4.2 where the different species are discussed in a seemingly random order, not even following the order in which they are presented in the figures.

- e. An overwhelming number of acronyms are introduced and used throughout the manuscript. Acronyms admittedly decrease the length of the presentation but also decrease legibility when these are first introduced and when the reader needs to go back and search for the explanation of a particular term. Would it be possible to put all explanations in a common table, for easy reference?

Some acronyms are not described the first time they are introduced. For example w_{sc} and L_{max} on page 8; TAPM on page 9; CMAQ on page 11; ...

Minor issues / typos:

1. Page 1, 2nd sentence: "... lower latitudes ...". Why open with this in the Abstract? Hamburg is hardly lower latitudes. For most readers Hamburg clearly classifies as belonging to "...northern European cities."
2. P2, L20: "...150 km² in size...". Unrealistically small domain if you use a 1 x 1 km² resolution.
3. P3, Bullets: Change place/numbering of item 1 and 3. In the following sections you discuss modifications to photochemistry first (Section 2.1), then the street canyon model (Section 2.2.1) and finally the extension that provides realistic winds at actual plume height (section 2.2.2). Please also introduce the three modifications in the same order in the abstract (it is currently photochemistry, plume rise, line source).
4. P4, L16: "... fine particulate matter with (PM_{2.5};...". The sentence is not correct.
5. P4, L18-19: "... NO₂, the major pollutant in many cities of northern Europe". This statement is opposite to what is said in the introduction (P1, L21-23), where PM and ozone is pointed out as the main air pollution issues in Europe!
6. P6, L21-22: "... NO_x are often below 1 ppbv." This is a too strong sentence. NO_x is rarely below 1 ppbv in "rural and sub-urban areas" of northern Europe.
7. P8, Eq 3: σ_{w0} should likely be squared.
8. P9, L13-14: "...the average turbulence of the hypotenuse of the trapezium (slant edge towards the opposite street side)." Can this be explained so the un-initiated understands it?
9. P9, L15: $(L_{base}/2)$ should likely be squared.
10. P10, L1: Consider renaming the header of this section as it mainly goes through the method of deducing windspeed at height z . The "plume rise model" is only described by referring to Briggs.
11. P10, L20: I do not understand the +0.01 term. Can it be explained?
12. P12, Eq10: " $C_{point,s}$ " should likely read " $C_{point,p}$ "
13. P14, L12: "... the ratio 10:1; ...". Why not indicate this line in Fig. 2a?
14. P15, L22: "Despite a slightly ...". This sentence is not complete.
15. P17, L7: What is the difference between "actual plume height" and "final plume height" ?
16. P17, L25: "...roughly proportional to ..." -> "...roughly inversely proportional to ..." (?)
17. P17, L26: "...40.5 m (neutral) to 32.4 m ...". Can you really defend three significant digits?
18. P19, L18: "...factor of 2.8 ...". Why not "...factor of 8 ..."?
19. P20, L22: "But considerations ...". Strange start of sentence.
20. P23-28: Section 4.2 is much longer than any of the other sections. Can it be divided into sub-sections for increased legibility?
21. P23, L3: "...when on the following days." -> "...than on the following days." (?)
22. P24, L1: "26.56". Can you really defend four significant digits?
23. P24, L11-12. Sentence including "..., modelled and show ..." is not correct.

24. P25, L7: "bias is within +/- 25 ug/m3". From Fig. 8b it is more like +/- 10 ug/m3
25. P25, L12: "...summer (JJA) mean...". Summer mean values are not shown in the manuscript.
26. P25, L19: "...diurnal cycles...". Not shown in the main manuscript. Avoid discussing details only shown in supplement.
27. P25, L27: "... underestimate observed PM2.5 and PM10...". From Fig. 8e,f, these are excellently reproduced!
28. P27, L5: "... in some distance..." -> "... at some distance..." (?)
29. P29, L5: "...EMEP unified model". I believe "unified" (as of combining photochemistry and acid deposition in the same model) has been dropped since several years.
30. P29, L9,L14: Is there a reason why you spell out "photochemical steady-state" on line 14, but abbreviate to PSS on line 9?
31. P30, L23: "..., yet unregulated pollutants, in cities ..." -> "..., yet unregulated, pollutants in cities ..."
32. P35, L7: "The statistical ...". The sentence is not correct.
33. P53, panels (d)-(f): Shouldn't the values to the left of 0 on the abscissa be negative?
34. P58: Spell out what version of TAPM (D4?) and CMAQ (CD4?) this refers to.
35. P61, Table 7: It takes a while to realise that TAPM also yields air quality results. In the main text TAPM is only mentioned in terms of dynamical downscaling of meteorology.
36. P61, Table 7: Why doesn't this table list the stations in the same order as in Tabs. 8-10? Why is station 54BL missing in Table 7? Why is Table 7 presenting absolute bias while Tabs 8-10 present relative bias?
37. P61-P62, Tables 8-10: Spell out in the table legend that the presented model performance statistics is for EPISODE-CityChem.