

Interactive comment on “Regional-scale modelling for the assessment of atmospheric particulate matter concentrations at rural background locations in Europe” by Goran Gašparac et al.

Anonymous Referee #1

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The manuscript deals with interesting topic of aerosol modelling and presents results of simulation of two modelling systems and high number of stations. The review recognizes that plenty of work has been done with processing of all data. On the other hand, many serious errors occurs in the manuscript and its current state absolutely does not respond to the ACP level. The most serious is the wrong use of statistical variables (see below), nearly no comparison with previous studies, sometime wrong or not described methodology. Also the presentation quality is not well, the text is hard to understand, sentences are often wrong arranged and mistakes in English occur (in/definite articles, commas, word order, braces). The number of technical errors is very height (see below). The manuscript have to be fundamentally improved or otherwise rejected.

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Specific major comments:

1) Fig. 7: Application of the same Equation 1 for BIAS to wind speed, temperature and surface pressure is not a good idea. E.g., a small hPa BIAS have great consequences, but percentage BIAS is only slight. For temperature, it depends on a choice if temperature in Celsius or Kelvins is filled. Generally, these variables are evaluated usually by simple BIAS as only the difference between model and measured value. Similarly, also NMSE, NMSE_{sys}, NMSE_{unsys} are dependent on Celsius/Kelvins, therefore not appreciate for temperature evaluation. Further, in every case, it is not possible to compare used statistical variables for comparison between meteorological variables. For this reason, it is also necessary to modify sentences in p. 12/l. 8-22.

2) Chap. 3.3.1: In general, evaluation of EMEP meteorology means evaluation of IFS model, EMEP is only the chemical transport model. This should be taken into consideration and discussed.

3) P. 13/ l. 17-23: It is not reasonable to conclude that the overall performance of models was good, due to low correlation of PM₁₀ concentrations and no comparison with other similar modelling studies. The comparison with other studies is relevant also for temperature and other meteorological variables evaluated. The comparison with previous studies has to be added to the paper.

4) Chap. 3.3.2: The text of the chapter is more a synoptic situation description than the model evaluation. There is no numeric comparison and model vs. observed spatial distributions of variables are in different figures, moreover partly with different scales. Please enable better comparison of modelled and observed values (figures including modelled and observed values, some statistics focused on the episodes).

5) Chap. 3.3.3: There is no information about sounding measurement, e.g. source, temporal resolution of data, etc. In case that only daily means are available, it is necessary to compare also only model daily means. White colour stands for RiB > 1 (not 0.25 as in text), statically stable conditions are for RiB > 0.25. It would be appropriate

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to have a better colour scale clearly pronouncing the limit of $RiB=0.25$. Further, why did not use direct output of boundary layer height from models? And again, the modelled and observed data could be in the same figure to easier comparison and reduction of total number of figures in the paper.

6) Chap. 4: Comparison with other previous studies belongs rather to sections Discussion (or Results), not in section called Summary or Conclusions.

7) Chap. 3.2, p. 9/ I. 23-27: Weather in western Europe should be also described, due to region of above-average PM concentrations.

Other comments and technical corrections:

1) P. 2/ I. 33-34: WRF-Chem includes chemical reactions in gas-phase mechanism used.

2) AQMEII and EMEP could be referenced by citation.

3) P. 3/I. 29: Braces in braces.

4) 4/33: Uncertainties are calculated but not used, it is not necessary to write it.

5) Chap. 2.2: Please write the reason for using of specific statistical methods and what they describe (at least for less frequent ones)

6) 2.2: There are two mistakes in IOA definition (see e.g. <https://www.rforge.net/doc/packages/hydroGOF/d.html>)

7) 2.4: first paragraph belongs rather to introduction – sentence 6/16-18 is unclear.

8) 2.4.1: Which type of chemical mechanisms (gases, aerosols) is used in EMEP model?

9) 2.4.1: What horizontal resolution has IFS?

10) 2.4.1: Which PBL parametrization is used in terms of meteorological model?

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11) 2.4.2: Why is not used the same domain for both models?

12) 2.4.2: Please add version of WRF-Chem model.

13) 2.4.2: Add the reason why is used NCEP analysis (resolution of 1 deg) for meteorological ICBC and not ERA-int reanalysis (0.7 deg).

14) 8/14: Which differences are meant? Description of results belongs to Chap. 3.

15) 8/18-24: Sentences in the whole paragraph are unclear and should be written better.

16) Chap. 2: There is not written any time extent of performed simulations or no information about spin-up interval.

17) 3.1 9/5: Analysis of variance should be shortly described or referenced. What does mean abbreviation ANOVA and $p=0$?

18) Fig. 2: The format of time axis (MM.DD.) is misleading due to fact that the paper concerns also to episodes. Someone can understand it as episode between January 1st and 12th. (Fig. 9+10 have time format DD.MM.)

19) 3.1: Secondary Inorganic Aerosols (SIA) – please reference it or describe more.

20) 10/8: There is maybe any missing text or reference to Fig. S2.

21) 10/23: Poland borders with eastern parts of Germany, so the onset could be rather in Poland and north-eastern Germany.

22) Fig. 3+4: Stations with temperature between 5-10 °C are not well visible.

23) Fig. 3+4: It seems that mountains stations indicate lower surface pressure, it would be appropriate to explain it.

24) 11/25: No significant difference between models and measurement below 75 m – that is not true (Fig. 6, on the left)

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25) Fig. 9: Please explain WS.

26) 14/32-33: It is not evident from Fig. 4 that there were increased values of PM10 in Pannonian basin, only few stations occur in this area. It should be well discussed or not written.

27) Fig. 11: Obviously wrong description.

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