

Interactive comment on “Prediction of source contributions to surface PM₁₀ concentrations in European cities: a case study for an episode in December 2016 using EMEP/MSC-W rv4.15 – Part.2 The local urban background contribution” by Matthieu Pommier

Anonymous Referee #1

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General comments

In this paper, the author presents a methodology to predict the source contribution to surface PM₁₀ concentrations for 34 European cities, focusing on the contribution from urban emissions. It is based on a set of EMEP model simulations run with a range of emission scenarios. This paper has a companion paper, Part 1 that is already published, focusing on the country contribution using a similar methodology and two

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models. The present paper shows that the proposed methodology gives good results. In particular, it gives a detailed analysis of the possible effects of non-linearities on its performances. This methodology proves its robustness and is clearly of interest for discriminating the sources in case of regional PM10 events. The paper is well in the scope of the journal. The main weakness of the paper is the lack in some places of clear explanations or references to well support the methodology and associated finding.

Specific comments

Although this paper is the Part 2 of a series of papers and it is recommended to read Part 1 first, the understanding of Part 2 should be possible without. This is not the case because some important pieces of information from Part 1 are missing in Part 2 for a full understanding (see suggestions below).

In the title and in many other places in the paper, the adjective 'local' is used and sometimes together with background, which is somewhat contradictory. Since the EMEP model is run with a horizontal resolution that is coarse with respect to the urban scale, the model simulations cannot provide very local information (as discussed in the paper when compared to observations). I find the use of 'Local' is misleading, in particular in the abstract. I would recommend to use 'urban' rather than 'local' in the paper and remove local of the title.

Line 76: Here and in other places in the paper, there are references to the WHO exceedance limits which are limits related to human exposure. Because of the model resolution, the EMEP simulations do not represent the variations of the pollutant concentrations at the small scale within cities and therefore also not the actual exposure. Nevertheless, the methodology proposed on the basis of the EMEP model is useful to for characterizing regional air pollution events and the contribution of cities to these events. This needs to be clearly explained.

Lines 81-84. Secondary aerosols are generally not the main contributors to PM10

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because they are mainly small particles. It would be useful that the author provides references on the relative contributions of secondary aerosols to PM₁₀ in European cities, since secondary inorganic aerosols are largely discussed in the paper.

Line 84-85: 'these PM₁₀ are essentially. role'. This sentence needs to be supported by references. PM₁₀ particles also undergo significant sedimentation due to their weight and dry deposition.

Introduction, 3rd paragraph where the Copernicus forecasting apportionment product and Part I paper are introduced. This paragraph is not clear: what means the different components? What the Part 1 paper is about and its link to the Copernicus product? How Part 2 is complementary to Part 1?

Introduction, 4th paragraph. An example is given of two methods assuming a linear relationship. Are there other methods not making this assumption?

Line 107: 'in cities in Europe' is too vague. Please, give a short description of the set of cities chosen.

Lines 112 to 120: The model resolution and its justification should be discussed first in this paragraph, before introducing the issue of the definition of city domain. About the resolution, I recommend to include a few sentences on the limitations related to the EMEP resolution for the present work but also for the previous work cited using similar model resolutions. Regarding the 3 definitions of the city domains, it is stated that they have been used in the companion paper but there is no information on what were the results from Part 1 on this particular point. More generally, a way to improve the introduction would be to give a short summary of the Part I objectives and results. This would be useful to understand what the present paper (Part 2) has in common and/or adds to Part 1.

Line 130: Could the author give a short description of the main updates?

Paragraph beginning line 131. The natural aerosols represented in EMEP need to be

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included in the model description.

Lines 142-144: Same remark on wet deposition as in the introduction. Why is there a lot of details on wet deposition while there is no information on precipitation occurring during the pollution event?

Line 146-147: To understand the paragraph, change 'This estimate . . . precipitation' to 'This estimate is derived from large scale precipitation and convective precipitation accumulated at surface.'

Line 149-150: What is the value set for the precipitation intensity? On which basis this value is chosen?

Paragraph from line 168 to 174. The logical choice would be to use GADM which should represent the real extent of the cities. This is not clear to me why a 1 or 9 grid points should be tested. This needs to be argued. The '1 grid point' is obviously too small to represent most cities. The '9 grid points' assumes that the cities have an extension following a square shape which is not fully satisfactory, in particular for cities close to the seaside which then are assumed to extend over the sea.

Section 3.2: I am not sure to understand the first sentence. I think this is the chemistry (and/or possibly other processes) that causes non-linearities leading to errors in the method since the method assumes that the response to the change of anthropogenic emissions is linear. I also do not understand what the author means in the last two sentences. This section needs to be written more clearly.

Lines 225-226: What do the negative contributions correspond to? This is explained but later in the text. Figure 3 is a complex figure that requires detailed explanations.

Line 235: Is the 31st country not already one of the in the 30 European countries? Please clarify.

Line 242: Information on the results of the companion paper is needed here so that it makes easy to understand the combined results from Part 1 and Part 2.

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Section 4.2 2nd paragraph: The author shows that H₂O concentrations are impacted by the non-linearity. There is no explanation on what H₂O refers to (relative humidity, concentration, vapour or liquid), except at the end of the paper where it seems that H₂O is linked to relative humidity. In the EMEP simulation, the meteorological parameters come from IFS. In the troposphere, H₂O mixing ratios are mainly driven by meteorology. Therefore, how H₂O concentrations are affected by the change of anthropogenic emissions? Since changes associated to chemistry should be negligible with respect to the uncertainties in the meteorological water vapour field, where do the non-linearities come from? The treatment of H₂O in EMEP model needs to be clearly explained in the model description in order to understand the analysis of the results.

Section 4.2 last paragraph: It would be useful to compare the uncertainties from the non-linearities to the model uncertainties that can be drawn from the comparison with the observations. This comparison has been done in Part 1 but this information has not been given in the present paper.

Section 5.2: The choice of the two cities (Paris and London) needs to be justified at the beginning of this section. Also, it would be useful to have the emissions for these cities for a more thorough interpretation of the results.

Lines 316-318: The author's analysis is consistent with an air quality report for Paris. Could the author explain what is the information in this report that supports his analysis?

Line 321: Reference to policies. The author may be more careful regarding policies since the present study assumes that the reduction of emissions applies to all sectors and with the same magnitude. In reality, policies on emissions during pollution events cannot be applied to all sectors and with the same level of regulation.

The conclusion section gives a summary of the methodology and results but little discussion on the other sources of uncertainties of the method than the non-linearity (for instance the meteorology or the parameterization of wet deposition) and also very few

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prospects of extension of the methodology (for instance to PM2.5 or to the contribution of anthropogenic emissions by sector).

Technical corrections

Line 95: replace 'few' by 'a few'.

Line 148: Replace 'Precipitations are' by 'Precipitation is'.

Line 149: Replace 'precipitations occur' by 'precipitation occurs'.

Line 165: Replace 'data set' by 'dataset'.

Figure 1 is small and it is very difficult to see the domains. More generally, most figures are small and uneasy to read.

Line 215: Replace 'was developed' by 'occurred'.

Line 299: Replace 'larger' by 'largest'.

Line 309: 'the list of country contributors are' to be replaced by 'the list of country contributors is'.

Line 310: Change 'correspond' to 'corresponds'.

Line 338: Change 'larger' by 'a larger'.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-242>, 2020.

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