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Geoscientific Model Development Discussions

Interactive comment on "Description of the uEMEP_v5 downscaling approach for the EMEP MSC-W chemistry transport model" *by* Bruce Rolstad Denby et al.

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REVIEWER 1

Thanks to reviewer 1 for their very detailed review. The manuscript has definitely been improved because of these comments. Here follows the authors answers/comments to the review

- Line 46: the Authors could mention the OSPM model as an example of a streetcanyon

model to complement the overview of local scale models

* The two examples given are of urban modelling systems. OSPM is a street canyon model, not a system for modelling whole urban regions. It is part of the THOR forecast system as the last part of the cascade. We did not include it in this line but it is implicitly include via the THOR reference.

- Line 95: clarify if this option implies that emissions in the EMEP grid cell are not consistent with the ones in the sub-grid cells

* This is clarified with the text. 'The independent emissions do not need to be consistent with the EMEP gridded emissions in this case.'

- Line 101: some discussion on the implications of using such inconsistent chemistry treatments in uEMEP and EMEP would be appreciated. As uEMEP is intended for applications over wide regions with significantly different chemical regimes, the simple chemistry may perform better in some environments than others

* This we have commented in Section 3.4 and 3.5 where this is discussed. In Section 3.4 we have added 'Comparisons with EMEP NO2 calculations show that this chemistry scheme matches the results obtained by EMEP over longer time periods.' and in Section 3.5 with 'This empirical relationship will vary from region to region, largely due to differences in O3 concentrations and photolysis rates that are not included as part of the parameterization. If used over large regions, for example Europe, then the uncertainty in the NO2 conversion will increase.'

- Line 116 and 140: the term Csg_nonlocal(i,j) is the more complex to understand. Perhaps, an equation describing how is computed would help the reader. I appreciate the effort of the Authors to explain the method with Figure 1 and 2 and Section 2.3, but it is still confusing how the local and non-local contributions of the EMEP grids are used in the computation of the Csg_nonlocal term.

* We are aware that the non-local, local and the moving window concepts may not be

as clear as we would like but we have tried to explain this as best we can. These are geometrical considerations that are not easy to express in words or even equations and best explained on whiteboards or with pen and paper. However, the reviewer has pointed out an oversite in our text. In actual fact Csg_nonlocal (Equation 1)is equivalent to Cg_nonlocal derived in Equation 10, since it is the EMEP contribution to the non-local subgrid. This was not explicitly mentioned but Equation 10 has now been updated, along with the text, to indicate this.

- Line 147: More details on Wind et al. (2020) methodology would be appreciated in the manuscript. Considering that the local fraction estimate links emissions with concentrations, the Authors could clarify how the chemistry is treated once the tagged emissions are dispersed in the EMEP grid cells. Are tagged primary pollutants emitted as inert tracers or limited chemistry is considered? The details are provided in Wind et al. (2020), but the reader would appreciate some further descriptions of the method and limitations in the present manuscript.

* We have included the following sentence concerning chemistry 'Tagged species are assumed to be inert species, primary PM and NOX, for the downscaling application as chemical reactions are not included in the tagging.'

- Line 152: Provide which fraction of the total contribution is missed in the local fraction estimate when using few EMEP grid cells.

* The authors perhaps did not understand how to answer this question. If all EMEP grids for the local fraction, not just 5 x 5, were used then 100% would be included. With less grids more will be part of the EMEP non-local contribution and less a part of the uEMEP local calculation. We attempt to address this in the sensitivity tests given in Sections 5.2 and S5.2. There we show for example that when increasing the moving window size from 4 to 8 EMEP grids then the local contribution increases by just 4% for PM10 and for NO2 this is 7%. There is no single answer to the reviewers question so none can be given in the text. The reader is already referred to this sensitivity study

in the text.

- Line178 Eq. 6: Why this is not divided by the sum of the weights? Following the example in Fig. 1, you use more than 9 EMEP grid cells (adding their concentration) to obtain the local contribution of the moving window over the i,j sub-grid cell. This results with a local contribution overestimated somehow if nmw>1. For the case nmw=1, the expression seems good as the sum of the weights would be 1.

* The local fraction calculation from EMEP specifies in the 5 x 5 grids surrounding each grid how much that grid contributes to the central grid (Cg_local). So if all the weights (w) were 1 we would simply get the sum of all the contributions to that central grid within that area. The weighting is just to account for when a part of the grid is included in the moving window. Having read the reviewers question we see there may be some confusion concerning the notation. The terms Cg_local(I,J) refers to the contribution to any one EMEP grid from the surrounding grids. We left off this index to avoid over indexing, though this indexing is included in the Wind article. We will put this additional indexing back into the Cg_local(I,J,I_lf,J_lf) where I,J refer to the grid and I_lf, J_lf refer to the local fraction grid associated with each I,J grid and where I_lf and J_lf are indexed from -n_lf/2:n_lf/2. We thank the reviewer for this comment that, though indirectly, corrected a misunderstanding in the notation.

- Line 233 Eq. 10: Why Cg(i,j,s) is divided by nsource if it is already the concentration of a specific source?

* In Eq. 10 Cg(i,j,s) is the sum of the moving window total concentations calculated for each source, i.e. Cg(i,j,s)=Cg_local(i,j,s)+Cg_nonlocal(i,j,s). This was not specified in the paper so Equation 10 has been rewritten to reflect this. As written in the text the non-local and local contributions can be different for each source when using the emissions for weighting and each source will have a nonlocal component contributed from the other sources. So after doing this source specific calculation these must be recombined into a single nonlocal concentration. This is done in Equation 10 by

taking the average of all the source specific total concentration calculations Cg(i,j,s) and then subtracting the total local contribution to get the final non-local value. This is rigourously correct when using the area weighting but is only a very close estimate when using the emission weighting. The authors realise that including the emission weighting makes this section much more complicated than otherwise required if only area weighting was used. To clarify what is being done the paragraph before Eq. 10 has been altered to read 'These local and non-local calculations are carried out for each emission source individually so the non-local contribution is also dependent on source and the non-local component for any particular source will also contain the local contributions from the other sources. This makes creating a final non-local contribution complicated. To solve this all the source specific Cg_local + Cg_nonlocal contributions are averaged and the sum of the Cg_local source contributions are subtracted to obtain the final Cg_nonlocal. The final non-local contribution at each sub-grid Csg_nonlocal, Eq.(1), is equivalent to the EMEP non-local Cg_nonlocal contribution and is calculated by '

- Line 295: I suggest introducing in this section the meandering and traffic term described in the supplementary material. Some variables in the equations are not defined just before or after presenting the equation. It would help the reader to introduce all the terms after the equations and specify which ones will be further described in subsequent sections

* We have included the following paragraph in this Section 'In addition to the parameterizations presented here uEMEP also includes parameterizations, provided in the supplementary material, for plume meandering and change of wind direction (Sec. S3.4.1), traffic induced initial dispersion (Sec. S3.4.3) and road tunnel internal deposition and emissions (Sec. S3.4.5).' and have defined all variables included in these equations.

- Line 330: Mention the floor value of the wind speed imposed in the model in this part of the manuscript. Some details are only presented in the supplementary material.

* THe following sentence has been added 'A minimum wind speed of 0.5 m/s for all dispersion calculations has been imposed.'

- Line 414: a table with the sigma_init_y values per emission source would be appreciated.

* The sigma_init_y, as mentioned in the text, is defined almost exclusively by the grid size, rather than the physical process but we have included the additional text 'traffic and 5 m for shipping, heating and industry'

- Line 518: an order of magnitude of the maximum distance allowed in the dispersion of the Gaussian model would be appreciated (i.e., 250 m).

* It is not clear to the authors what the reviewer is refering to here as there is no mention of this in this line. We do not know which 'maximum distance allowed' the reviewer is referring to. The distance the plume can travel is defined by the size of the moving window, if that was what is meant here. If the reviewer is referring to the sub-grid size then there is no numerical limit but we have never applied the model to a larger sub-grid than 500 m.

- Line 583: Is ozone also a product used from uEMEP? Is there any evaluation done for this pollutant?

* Ozone is a product and this is also assessed but we simply did not include it here. There are very few ozone stations in Norway where the model was assessed. This link shows extensive evaluation, also for ozone, but is in Norwegian (https://www.met.no/prosjekter/luftkvalitet/evaluering-av-luftkvalitets-modellen)

- Line 631: Some discussion about the improvement in the daily cycle of the uEMEP results compared with EMEP would be appreciated. Local models use to improve the traffic peaks but also may inherit issues with the temporal profiles and the boundary layer evolution. The validation section could be improved introducing some discrimination between types of sites (rural, industrial, suburban, urban). I suggest presenting all

the material of subsections 5.1.1, 5.1.2 and 5.1.3 under section 5.1 as those sections consist only in a single paragraph.

* We have reduced Section 5.1 to a single section, as suggested by the reviewer. As listed there are a limitted number of monitoring sites in Norway, with 90% being traffic stations. There is 1 urban site, 3 suburban sites, 2 rural sites and 1 industrial site. This lack of representation does not warrant individual selection and presentation. The EMEP model run in Norway uses the same emission data as the local scale uEMEP, only aggregated to grids. So the traffic variation is exactly the same, if this is what the reviewer is refering to. In general we have tried to keep the validation to a minimum as this will be more thoroughly assessed at a later date. The validation is intended to show that the model works, rather than a detailed analysis. The paper is intended as a model description primarily.

- Line 653: What missing processes could explain the remaining bias during the summer period in both PM10 and PM2.5?

* We believe a large part is secondary organics, but that is currently just speculation so this was not taken up in the article. We intend a more detailed evaluation of many more years in a later article.

- Line 700: it is counter-intuitive having more non-local EMEP contributions with smaller moving windows. Could the Authors clarify this in the text? If less EMEP grid cells are used in the moving window, less non-local contributions would be expected.

* Non-local contributions come from outside the moving window. The larger the moving window the less comes from outside so this is intuitively correct. Said differently, the larger the moving window the more local contribution as well. This is decribed in Section S5.2.

- Line 796: There are still some street-canyon processes that uEMEP cannot represent, particularly in compact cities with high street aspect ratios. The Authors should mention

this in this last concluding remark.

* We have added the text to this line 'It can also represent concentrations down to street level, though not street canyons, '. It was pointed out before that it is not obstacle resolving but it does not hurt to mention this a second time.

- Line 29: the acronym CTM is used several times in the manuscript but defined in Line 71. Please, define the acronym already in the introduction and use directly the acronym in the rest of the manuscript.

* Done

- Line 51: use coma instead of a semi-colon in the reference

* Done

- Line 58: the reference Wind et al. (2020) is not provided in the reference section.

* That was strange, but inserted.

- Line 154: fix the Section number. Here and in other parts of the manuscript, the number of the reference to specific sections is 0.

* Due to automatic reference system that stopped working. This is now fixed

- Line 245: Use Eq. instead of Equ. in the Figure caption.

* Done

- Line 362: Monin–Obukhov is mistyped in different parts of the manuscript.

* Done

- Line 362: the Monin-Obukhov length and the surface roughness have already been used before in the manuscript. Define them there only once.

* Done

- Line 371 Table1: please, use consistent notation for the boundary layer height and Monin-Obukhov length. Both have been introduced before as H and L

* Done

- Line 407 and 574: fix the section number that appears in the reference Sect. 0.

* Done

- Line 646: the statistics presented in panel (a) should be introduced in the caption specifying for which model are computed. In panel (b), the Authors could remove the shipping and industry labels in the legend as no information is shown in the figure.

* The contribution from industry and shipping is present but very small, in this case. Since these were calculated we will keep them in the legend. We have added in the figure caption that the statistics in panel (a) refer to the uEMEP model and we have reorganised the statistics text in the plot itself to better reflect this.

- Line 661: There is too much information in Figure 11. I suggest presenting the non-local contribution of EMEP and not the detailed composition of it. Though of interest, it is impossible to appreciate EMEP4NO line and some artefacts appear as the white contribution above EMEP PRIMARY blue fraction.

* We agree that the plot was very busy and we have now aggregated all species into the non-local EMEP, as suggested by the reviewer.

- Line 679: avoid using subsections that consist of a single paragraph.

* The authors used this form to have consistent cross referencing to the supplementary material but understand it would seem a little strange in this context. We have removed the numbering but have kept the headings to delineate between the different sensitivity studies.

- Line 719: to be consistent with the supplementary material the coefficient of determination of the station mean time series of uEMEP should be 0.79, not 0.80. Harmonise

the number in both documents

* Done

- Line 728: I suggest merging Sections 6 and 7.

* The authors would like to keep these as two seperate sections, as is often the case for discussion and conclusion.

- Line 13: Use section S1 instead of S3 and number accordingly the rest

* As explained in the text we use this numbering so it is easy to cross reference between supplementary material and the main document. Given the nature of the supplementary material, that it provides extra details on particular sections in the main document, the authors feel this method of numbering is more appropriate and will keep it. This is already stated at the start of the document.

- Line 106: It should be Eq. (15a).

* Done

- Line 242: Why the inverse of the wind speed is used instead of wind speed?

* In dispersion calculations it is the inverse wind speed that is multiplied with emissions and the dispersion intensity to determine the concentration, Eq. 11. When averaging then it is the mean of the inverse of the wind speed that should be used, rather than the mean of the wind speed.

- Line 314: In the figure caption, it should be Fig. S4 instead of S2.

* Done

- Line 328: The observation measurement could be provided in Fig. S6.

* The comparison with observations has already been made in Figure 10a in the main paper. Since the major aim of this sensitivity study was to assess the dependence on resolution the authors do not think it is necessary to repeat that comparison here, in an

already crowded plot. We do not include the observations again here.

- Line 368: Why Figure S8a is different from Figure 10b? The caption describes the same results.

* The reviewers are correct that these two plots should be the same. This has now been fixed. The discrepency was due to the way concurrent measurements and model results were selected. The reviewers will also note that the mean of scatter plots is not always the same as the mean of the time series. This is also due to the selection criteria for annual means requiring 75% coverage per station whilst daily mean plots of the average of all stations do not have the same requirement.

- Line 385: A value of 0.1 would likely provide an even closer fit to observations.

* A lower value than 0.15 would probably give a better fit but this was not assessed as it lay outside the expected NO2/NOx emission ratio range for vehicles in Norway.

REVIEWER 2

Thanks to reviewer 2 for their comments and time. Some aspects of the modelling, particularly how the local window local/non-local works are difficult to explain but I have tried to improve on this. Reviewer 1 also commented on this. Here follows the answers to the reviewers questions and improvements.

- line 33 / “near street level modelling”: What is then the ambition of the model? Is it supposed to represent concentrations at roadside monitoring sites or background sites?

* It will represent concentrations at roadside monitoring sites, and the validation for NO2 confirms this, when using a resolution of 25 m. Even so the sensitivity tests to resolution, Section S5.3, show that good results are still obtained at 100 m. It does

not however well represent street canyon sites as the Gaussian model used has no obstacles. One would then expect an underestimate at these sites.

- What is the meaning of resolution <100m when there is no local topography modelling involved? Wouldn’t building layout, air flows in the street canyon etc need to be accounted for at these very local scales?

* Without including obstacles the increased resolution allows the concentration gradients at roadside to be better described. The reviewer is right to point out that, if this was to be done properly at < 100 m, then buldings need to be included. However, uEMEP is intended for application over country scales and that level of detail is not achievable.

- line 85: Which source sectors are included in the uEMEP downscaling calculations? Traffic, residential, any other? Should be mentioned somewhere in Sect 2.1

* The downscaled sectors depend on the application so this is not expllicitly named until the application is defined in Section 4.2. However, we have included the following text in Section 2.1, line 100 ’Typical source sectors downscaled using uEMEP include traffic, residential combustion, shipping and industry. The sectors addressed will depend on the availability of high resolution data for distributing them’

- line 150. “neighbour cells” sounds as if only +/- 1 in each direction but I understand from the next sentence that the local fraction region can be quite large. Please clarify in the text.

* We have changed that sentence to read ’The local fraction region extent is then limited.’

- line 153. Perhaps I missed it but it would be good to have a paragraph somewhere that explains the difference between the different domains (uEMEP vs local fraction vs moving window) as it is a bit confusing to the reader

* Moving window and EMEP local fraction region are described in the text seperately but these are also visualised in Fig. 1. That was indeed the intention of Fig. 1. We

believe this is sufficient explanation.

- Sect 2.3-2.4: These sections are difficult to follow, I would suggest restructuring 2.3 and 2.4 into one (The second sentence of 2.3 already refers to 2.4)

* We would prefer to keep these as two different sections. The first (2.3) applies to the local fraction calculation from EMEP and the second (2.4) the moving window calculation in uEMEP. Though the second utilises the first, they are two distinct calculations. Reviewer 1 also commented on this section and as a result additional text and a change in formulation of the equations have been implemented. We belive this has helped to clarify these sections.

- Sect 2.4: This is rather complicated to follow for an effect that is probably secondorder. How much is gained by the complicated moving window calculation of nonlocal contributions at sub-grid resolution? With a reasonably big local fraction tracking domain, the difference between sub-grid and grid level non-local contribution should become negligible?

* The reviewer is correct, first that it is complicated and second that it would not matter if the local fraction domain and moving window were sufficiently large. This is also stated in the text. However, there will always be an edge somewhere to the moving window domain and we consider it necessary to implement a method that can deal with this limit properly, especially when just 1 large EMEP grid, for example 15 km, is used.

- Line 214-216 are a bit confusing, please explain better why this method (as opposed to the area weighting) gives different total (local?) concentrations

* We have tried to clarify this in the text with an extended explanation 'The resulting total concentration, using this method, may be higher or lower than the original EMEP concentrations because it reflects the impact of moving the EMEP grid in space. This is easiest to visualise if the moving window is the same size as the EMEP grid. If

the moving window were centred on an area with concentrated emissions, that are in reality spread over two EMEP grids, then when using the emission weighting the new EMEP local contribution would be higher, the non-local lower and the total would be different, see Fig. 2. The opposite is also true if the moving window were placed over a region with low emissions, the local contribution would be lower and the non-local higher. Due to this, it is not possible simply to subtract the local EMEP contribution from the total to get the non-local EMEP contribution, as detailed in Eq. 5.'

- line 218: non-local contributions do not have any associated emission: that is considered in the uEMEP. In general I assume they do have an associated emission. Do s refer to all source sectors in the EMEP model or only those considered in uEMEP?

* We are refering only to the sources that are downscaled using uEMEP but we have reworded to make this clear. 'The first term is the non-local contribution for a particular source and is calculated with the area weighting distribution since non-local contributions, those outside the local fraction region, do not have any associated emission or local fraction for weighting.'

- line 220/ Eq 9 is confusing to me. It should be possible to slightly rephrase the paragraph before to clarify why this needs to be done and what is done here. Also, is there an inconsistency between Eq 6 and Eq 9 regarding the source grid range, Eq 6 has I-nmw/2 . . . I+nmw/2 but here it runs from I-nmw . . . I+nmw

* The reviewer is correct, there is an inconsistency between Eq. 6 and 9. Thank you pointing this out. We have corrected Eq. 9. Reviewer 1 also commented here and some additional updates of the Equation indexing has been made. We have rephrased this paragraph to read ' An additional correction term, second term in Eq. (9), accounts for the non-local contributions from local contributions on the EMEP edge grids, those parts of the EMEP grids that are outside the moving window and not included as a local contribution in Eq. 6. In those cases the local EMEP contribution outside the moving window must be converted to a non-local contribution and subtracted from the

calculated non-local value, first term in Eq. 9.'. These are geometrical arguements that are difficult to explain with words and equations but we hope the concept has become clearer.

- Eq 10: Why the division by nsource?

* This was also commented by reviewer 1 and we have rewritten the text and reformulated Eq. 10 to make this clearer. The term n_source was used to average CG(i,j,s) since this value contains non-local contributions from other sources as well, each source having it's own local and non-local contribution. In addition the term CG(i,j,s), in Eq. 10, was actually never defined (it is CG(i,j,s) = CG,local(i,j,s) + CG,nonlocal(i,j,s)). The text now reads, 'These local and non-local calculations are carried out for each emission source individually so the non-local contribution is also dependent on source and the non-local component for any particular source will also contain the local contributions from the other sources. This makes creating a final non-local contribution complicated. To solve this, all the source specific CG,local + CG,nonlocal contributions are averaged and the sum of the CG,local source contributions are subtracted to obtain the final CG,nonlocal. The final non-local contribution at each sub-grid CSG,nonlocal , Eq. (1), is equivalent to the EMEP non-local CG,nonlocal contribution and is calculated by ...'

- line 291: This is the first occasion that time is explicitly mentioned, worth a sentence of explanation since so far everything was stationary.

* We have made this clearer by writing 'pollutant travel time (t) from source'.

- Section 3.2: Annual mean with rotationally symmetric Gaussian plume – As the authors state, the condition of homogeneous distribution of wind speeds in all directions is typically not met. A calculation with wind roses would not add too much in complexity but would avoid this assumption

* Yes, wind rose calculations could be made but this does require use of the hourly

meteorological data at every point in space within the model domain. This is much more complicated and time consuming than this simple analytical methodology. Comparisons with hourly calculations, Section S5.1, show the assumption works quite well.

- Line 510: traffic emissions are often described as line sources in emission inventories. What is then the appropriate uEMEP subgrid size?

* We find 25 m is sufficient resolution (around the width of a multi-laned road) and little is gained by higher, or even slightly lower resolutions, see Section S5.3. We use 25 m for receptor calculations though 25 m is prohibitive for large scale map making.

- Which source sectors are included in the uEMEP for Norwegian forecasts?

* This is stated in Section 4.2

- Section 5.1: Are all station types included in the validation? How different is the performance of uEMEP, does it work equally well for street canyon stations as for urban background sites? It would be interesting to indicate the station types in Fig 10a.

* All stations are included. In Norway there are very few traffic stations that could be called 'street canyon', around 3 of these. The rest are in fairly open road situations. There are also very few urban background sites, around 3 of these as well. Since this article is primarily a model description we tried to reduce the validation to a minimum. We believe that a more detailed assessment is more appropriate for a seperate paper which includes many more years of data and a more thorough investigation. For the moment the authors think the current validation is sufficient.

- Section 5.1.2: While the agreement is clearly better than with EMEP, still the correlation is quite low and there is a low bias. What is the authors' explanation, given that emissions are provided at quite high resolution? In particular for the low bias in summer, which is also seen in PM2.5 (factor 2!) – is this a regional issue (also seen in EMEP validation against background sites) or a problem in downscaling?

* Without having direct proof we believe the low values in the summer are due to too

low estimates of secondary organics in EMEP and not a problem with local sources. This is being looked at. The spatial correlation for PM2.5 annual mean is rˆ2=0.49, which is significantly better than EMEP and given the complexities of PM emissions and processes a reasonable result. Low correlation for PM10 is dependent on all the same PM2.5 uncertainties but in addition, in Norway and other Scandinavian countries, road dust emissions have a significant impact on PM10. This emission source is very difficult to model, though we do use the NORTRIP road dust model for this which is currently state of the science. As mentioned in the previous answer we will be investigating these problems in a later study that concentrates on the results rather than the model.

- line 66 typo: provided

* Done

- line 130 replace then with comma

* Done

- line 135 the same

* Done

- line 141 add comma after (I,J) to increase readability

* Have added a comma on both sides, I think that is correct.

- line 154 correct reference

* Done

- line 167, 176 the same

* Cannot find this line reference

- line 250 insert comma after ‘this’ to increase readability

* Done

- Line 305: Define u*.

* Done ’friction velocity’

- Line 406 references missing

* Done

- line 574 reference missing

* Done

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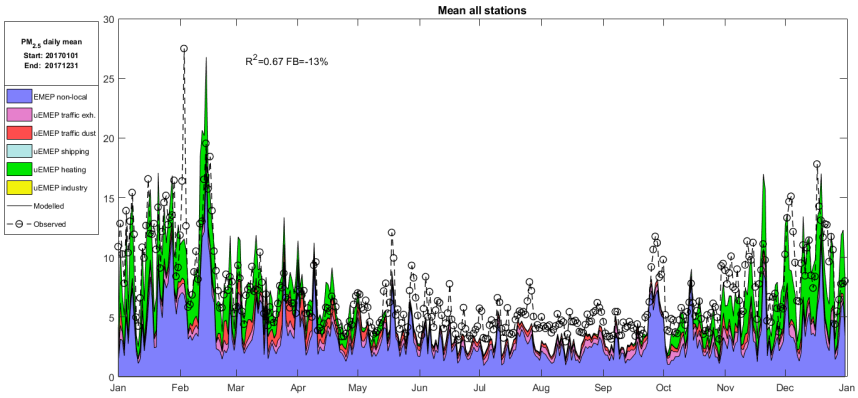

Fig. 1. Update of figure 12b

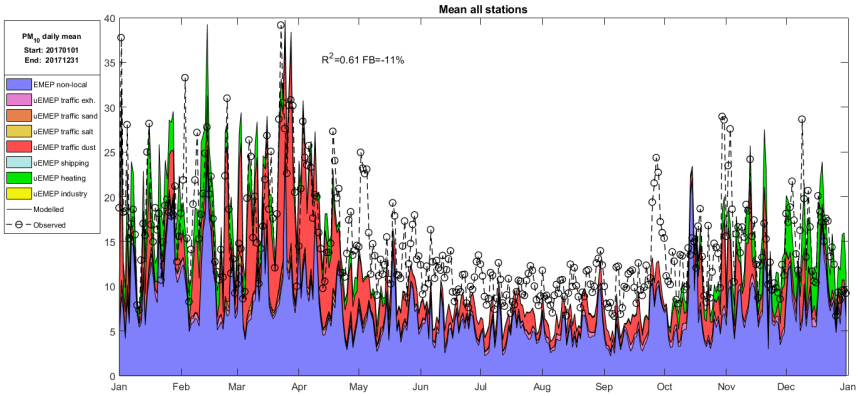

Fig. 2. Update of figure 11b