Authors response to comments of the Referee #2

We thank the Anonymous Referee #2 for the interesting and important comments on our manuscript. All the individual comments are addressed below in red.

General comments:
It would appear that the primary objectives of the presented manuscript were to introduce, document and promote a 'fit-for-purpose' application of the Enviro-HILRAM model.
The Eviro-HILRAM model is well established in the community. It is being used and developed trough a broad international collaboration. It is important that a proper reference to this valuable tool is provided. The Authors made an effort to present the origin and evolution of the model over the years. Also, a short description of model components and applications was provided. Specific comments and suggestions are given in the next section.
In the manuscript, the Authors advance terms and concepts of "online coupling", "fully online integration", "seamless meteorology-chemistry modelling", "two-way interacting", "on-line integration". The use of these terms is not consistent and confusing.

Response: Thanks. The terminology is harmonised/corrected in the revised version.

Also, the concept of a meteorological/NWP model with chemistry was proposed, implemented and published earlier than the provided reference to Grell at al. (2005).
Coupled chemistry-climate models were developed and used in the 1990s, cf. Steil et al., 2003 (doi:10.1029/2002JD002971), Austin and Butchart, 2003 (Q. J. R. Meteorol. Soc., 129, 3225–3249), de Grandpré et al., 2000 (J. Geophys. Res, 105, 26,475–26,492), among other publications. Thus, a proper historical and scientific perspective should be preserved, especially in a paper that presents "strategy and methodology" and dedicates several paragraphs to model evolution and origin.

Response:
Thank you for the comments. The references are included in the revised version. However, more comprehensive historical overviews of coupled chemistry-meteorology models were done e.g. by Zhang (2008), Kukkonen et al. (2011), Baklanov et al. (2014).

The Authors introduced the term "biological weather". It is the understanding of the reviewer that this term refers to birch pollen modelling. However, the meaning of the term is unclear and probably misleading.

Response:
The "biological weather" term is defined in Klein et al. 2012 as "the short-term state and variation of concentrations of bioaerosols".
Thus in the current paper, biological weather refers to birch pollen modelling.
The reference to Klein et al. 2012 is included for clarification.

It is not evident, from the presented model description that it is a multiscale or a wideband atmospheric model. In most of the presented examples, the model domain covers the European continent. Application of the model to urban scale with a resolution of 2.5 km in a hydrostatic mode is rather problematic. The Authors should further comment and justify its use at the said resolution (cf. Lines 508-509).

Response:

Yes, the hydrostatic approximation of the model was a limitation to increase the resolution to perform the urban simulations. However, sensitivity tests demonstrated that the 2.5 km was the optimal resolution allowing at the same time to obtain satisfactory reproducibility of the large scale processes and to explore the urban effects at local scale without being diminished due to a coarse resolution, for a medium size city (even possibly can be considered for a small size city). For other metropolitan areas such as Paris, Rotterdam, St. Petersburg, Shanghai - a similar resolution was chosen, although for Copenhagen (with its flat terrain) the highest possible/ suitable resolution tested was 1.5 km and provided reasonable verification results. Within a selected metropolitan area there could be only a few grid cells having 100% representation of the urban fraction, but taking into account all urban grid cells, the boundaries of the cities (number of cells) could be substantially larger. Moreover, it should be noted that most of existing developed parameterizations in the physics core of any existing NWP model might need a revision when resolutions of 1 km and finer are used.

The Authors provided references to all model components and applications. However, this paper should explicitly provide all 'vital model information' such as vertical structure, horizontal resolutions (with clearly stated limitations), numerical methods and approximations employed in different modules (components), modularity and scalability of components, examples of integration time and computer topology used for benchmarking.

Response:
Vertical structure and horizontal resolutions of the model are flexible. Limitations, e.g. due to the hydrostatic approximation, are provided (min 1,5 km for flat terrains, e.g for Copenhagen). Corresponding information, as requested, is included in the revised version.

What is the required computer power, maximum number of computational cores, can the model be run on a heterogeneous architecture with GPUs? All these characteristics should be addressed and tabulated with appropriate references and notes.

Response: The model is parallelized with both OpenMP and MPI technics, but it cannot be run on heterogeneous architectures with GPUs. The parallelization algorithm performs 2D decomposition of a modeling domain. The Enviro-HIRLAM can be run on Linux/Unix clusters and CRAY XT5/XC30 high performance computers.
We have not heard of tests where effect on scalability of introducing chemistry, aerosols etc. have been made.
Changes in manuscript:
L427: The Enviro-HIRLAM modelling domain with horizontal resolution of 0.15^{o} x 0.15^{o} having 310 x 310 grid cells, and 40 vertical hybrid sigma levels extending to pressures less than 10 hPa, covers Europe, North of Sahara, and European Russia. The modeling domain was partitioned into 120 CPU cores and the model was run with time step of 300 seconds.

In several sub-sections, the Authors included a description of earlier versions of the model. Thus, it is not clear to the reader which parameterizations are used in the current version of the Enviro-HIRLAM model. It would be advisable to move these paragraphs to an appendix presenting development stages and perspective of the Enviro-HIRLAM model.

Response:
More concrete info about parameterizations used in the considered case studies and in the current version of the Enviro-HIRLAM model is provided in the revised version.

In Section 3 (Modelling system applications), the Authors refer to several earlier publications. It is not clear if the presented manuscript contains any results that were not published. It would be advisable to add a table (in Section 3) with a list of presented experiments and model versions used for simulations together with appropriate references.

Response:
Most of results presented in the paper are new (used only in technical reports). We include more accurate references to appropriate papers, if some experiments were considered in previous publications, in the revised version. However, it is difficult to provide such information in a table form.

Also, if a figure is adopted from an earlier publication, a proper reference should be included in a figure caption.

Response: Thanks, checked and done.

Pollen module description should be moved from Section 3.3 (Pollen forecast) to Section 2 (system description).

Response:
Pollen applications require specific parameterisations of pollen emission sources and other characteristics, so it is more relevant to describe in the section 3.3.

Sub-section 3.4 should be moved and inserted as 3.1

Response:
Section 3.1 focuses on the effect of weather while 3.4 is about air-quality forecasting. Although these are two distinct subjects which seem reasonable to address individually.

Overall, the justification of advantages of the on-line approach is not sufficiently demonstrated.

Response:
The advantages of the on-line approach were discussed in details in the previous EuMetChem paper (Baklanov et al., 2014).

Verification aspects should be included in a more coherent way. Presented experiments refer to relatively short periods (one summer month). Results for the gas phase chemistry are not discussed.

Response:
Yes, we agree that many additional verification and sensitivity experiments are needed for different applications (long-term validation, chemistry mechanisms, etc.). We are working with some of them and they will be in following papers.

The Authors should restructure the manuscript to emphasise the overall modelling philosophy and future directions of the proposed model development and applications.

Response:
Thanks. We modified the concluding sections correspondingly in the revised version. However, the overall modelling philosophy and future directions of coupled meteorology-chemistry model development were subjects of our previous papers of EuMetChem, CCMM, etc. (see corresponding references in the paper). Here we focus on the Enviro-HIRLAM model description and its applications.
However, we'd prefer do not change the papers structure dramatically, especially keeping in mind that two other reviewers have found that "The manuscript is well structured and provides a comprehensive presentation of Enviro-HIRLAM development ….".

Specific comments:
The presented comments are in a sequential order and refer to the line numbering in

the presented manuscript.
L22: “Online integrated passive pollutant transport” - the same term should not be used for the simplified approach.

Response: Thanks, agree. We mean the online consideration of tracer equations together with other equations at the same time steps (without feedbacks). We modified the sentence.

L27: What is “effective chemistry”?

Response: Thanks. Changed to ‘cost-efficient’.

L35-36: The section title is too long and awkward.

Response: Thanks. The title is shortened.

L68: The style of Figure 1 does not conform to a convention used in scientific publications.

Response:
Yes, it might be not the standard/ most common way of the material presentation, but the Figure 1 presents the overall structure of the modelling system, its research development, technical realisation, science education and potential application areas. All these elements are necessary main building blocks in elaboration and maintenance of the modelling system and it is important/useful to present them in such a graphical form.

L108: “current new version” – should be either “current” or “new”?

Response: Done.

L128: “main meteorological fields” – please define.

Response: It is specified in the text.

L142: How long are the “long-term runs”. Please explain and justify.

Response: Done: up to one year.

L175-185: The whole section on photolysis rates is confusing and misleading.

Response:
For the simplicity of photolysis rates estimation we used the following:

1. For the simple reactions, we estimated the Photolysis rates as a function of number of parameters such as meteorological and chemical inputs including altitude, solar zenith angle, overhead column densities for O3, SO2 and NO2, surface albedo, aerosol optical depth, aerosol single scattering albedo, cloud optical depth and cloud altitude.
2. For the complex reactions, we estimated the Photolysis rates as lookup table using the Tropospheric Ultraviolet-Visible Model (TUV) developed by Madronich and Flocke (1999) and a pseudo-spherical discrete ordinates method (Stamnes et al., 1988) with 8 streams. We run TUV offline and calculated a lookup table of the Photolysis rates, and then we implemented this lookup table under different weather conditions inside our model.

L177: Please explain how the ozone column is set above the model top.

Response:
We used the climatological chemical boundary conditions from MOZART chemical transport model using a monthly average of years 2000–2007 (Horowitz et al., 2003; Emmons et al., 2010). The model

top (50 hPa, corresponding to the lower stratosphere) uses a climatological ozone concentration based on interpolated MOZART ozone fields.
Therefore, the model top layer contains ozone concentrations comparable to the stratosphere. Indeed, we implement the climatological values for computational efficiency during model development and test simulations.

L181: The assertion that the 8-stream method is "the most accurate" should be justified.

Response:
The 8-stream method is used and justified in TUV model system, developed by Madronich and Flocke (1999):
Reference: "Madronich, S. and Flocke, S.: The role of solar radiation in atmospheric chemistry; in: Handbook of Environmental Chemistry, edited by: Boule, P., Springer-Verlag, New York, 1–26, 1999"

L282: In Figure 4 X-axes have different units.

Response:
Both the left hand plot and the right hand plot in Fig. 4 have x-axes, showing the electromagnetic wavelength. Since the left hand plot shows SW wavelengths, these are given in units of nm, while the LW wavelengths in the right hand plot have units of μm. It is common practice to use these units for SW and LW wavelengths, respectively.

L343: What is "traditional" SL? Please provide a reference.

Response: Thanks. We provided the reference to the "traditional semi-Lagrangian" scheme:
Robert, A. 1981. A stable numerical integration scheme for the primitive meteorological equations. Atmos.-Ocean 19, 35–46.

L382: Figure 6: The presented figure alone does not prove that the model can deal with sharp gradients.

Response:
Detailed model tests of the ability of ILMC to reproduce sharp gradients are described in Sørensen et al. (2013), in particular Figure 3 and the accompanying discussion in that paper.
The text in the revised version is corrected to avoid confusions.

Line 389: What is TR4?

Response: Thanks. It is a mistyping. TR4 should be Eq. (4). Corrected.

Line 390: The mental jump referring to "formal conservation" should be explained.

Response:
We have already answered this question to Reviewer 1.
We have added a sentence to clarify that mass-wind inconsistency is a minor problem. The traditional HIRLAM is (at least in principle) wind-mass consistent. In Enviro-HIRLAM, where all moisture fields are transported with the LMCSL scheme, there is no formal consistency, yet, since precipitation is very similar to that in HIRLAM (except for individual convective systems that are chaotic/unpredictable in their nature), the mass-wind inconsistency is small in practice.
A more careful discussion on the issue of mass-wind inconsistence in atmospheric models would require a rather extensive addition. In principle, no monotonic transport schemes can be mass-wind consistent, since the monotonic limiters formally destroy the consistency.
We also add a reference to the paper: Jöckel, P., von Kuhlmann, R., Lawrence, M. G., Steil, B., Brenninkmeijer, C. A. M., Crutzen, P. J., Rasch, P. J., and Eaton, B.: On a fundamental problem in

implementing flux-form advection schemes for tracer transport in 3-dimensional general circulation and chemistry transport models, Q. J. R. Meteorol. Soc., 127, 1035–1052, 2001.

L407: The title is confusing, and the whole section is too long. Half of the first paragraph refers to urban applications, which are discussed in the next section.

Response: In the revised version we modified the title to 'Applications for Numerical Weather Prediction' and slightly shortened the text.

L497: It is wrong to assert that higher correlation implies that the model is "closer to observations."

Response: We modified the text; the statistical analysis showed that the urban simulation had a reduced bias with respect to observations than the control simulations.

L505: The ability of a weather prediction model (i.e. HIRLAM) to reproduce meso-scale processes at the regional scale should not depend on the use of an urban parameterization. The presented conclusions do not belong in Section 3.2.

Response:
Yes, the ability of a weather prediction model to reproduce meso-scale processes does not depend on the use of an urban parameterization. However, since the hydrostaticity of the model was a limitation for increasing the resolution to study the urban impacts, several sensitivity tests demonstrated that the 2.5 km was the optimal resolution allowing at the same time to obtain satisfactory reproducibility of the large scale processes and to explore the urban effects at local scale without being diminished due to a coarse resolution (as fraction of urban areas in grid cells of coarser resolution became very diluted).

L654: The calculations were analysed for one month (July 2010) only. Thus, the sentence is too general.

Response: Thanks. The sentence is changed in the revised version.

L656: "crude model resolution" – what does it mean?
The use of the English language:
The Authors should pay particular attention to the use of articles, prepositions and tenses in the revised manuscript. Also, the Authors used words that do not exist i.e. Line 255: 'to split' is an irregular verb – the simple past tense is 'split', or words in a wrong context i.e. Line 187 'Heterogenic chemistry' should be 'Heterogeneous chemistry'.

Response: Thanks. We checked and corrected the language in the revised version.

Recommendation:
In the opinion of this reviewer, the presented manuscript could constitute an important contribution documenting the Enviro-HILRAM model. The paper should be published after major revisions.

Response: Thanks a lot. We do our best for that.