

Interactive comment on “The Enviro-HIRLAM online integrated meteorology–chemistry modelling system: strategy, methodology, developments, and applications” by Alexander Baklanov et al.

Anonymous Referee #2

Received and published: 6 February 2017

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 Enviro-HILRAM model is well established in the community. It is being used and developed through 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

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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. Also, the concept of a meteorological/NWP model with chemistry was proposed, implemented and published earlier than the provided reference to Grell et 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.

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.

It is not evident, from the presented model description that it is a multiscale or a wide-band 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).

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. What is the required computer power, maximum number of computational

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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.

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.

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

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

Sub-section 3.4 should be moved and inserted as 3.1

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

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

Specific comments:

The presented comments are in a sequential order and refer to the line numbering in the presented manuscript.

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L22: "Online integrated passive pollutant transport" - the same term should not be used for the simplified approach.

L27: What is "effective chemistry"?

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

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

L108: "current new version" – should be either "current" or "new"?

L128: "main meteorological fields" – please define.

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

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

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

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

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

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

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

Line 389: What is TR4?

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

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.

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

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observations.”

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.

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

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’.

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.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-319, 2017.