Geoscientific

Model Development

Discussions

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comment

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

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This manuscript provides a thorough presentation of Enviro-HIRLAM representing one of the first serious development efforts towards implementing a fully online coupled meteorological and chemical weather model. It contains detailed descriptions of methodology selected and implementation followed, including some coverage of less well-defined aspects of online coupling and performance evaluation. The paper is well written and contains a large amount of information. A section on model applications provides additional insight on the extremely important aspects of evaluation and vali-

dation.

As the overall assessment of the present referee, the paper successfully describes the remarkable effort that has been devoted to the development of a state-of-the-art online meteorological and chemical weather model. It is adequately referenced and contains detailed explanations of the main physical mechanisms and selected parameterisations. It also highlights some of the more promising aspects of the coupling idea, both in the area of aerosol-radiation treatment and in cloud microphysics.

The only weak point in the manuscript is the rather sketchy discussion of the extent to which the explicit introduction of all effects will lead to improvements in model performance. Section 4 of the manuscript represents of course an honest attempt to summarise what we know on the effect of coupling in model performance for different applications. The authors are encouraged to provide more explicit comments in this respect. This should be combined with a more thorough discussion on how all parameters required in the various process parameterisations could be fine-tuned (for instance, expanding the comments made in the last four lines of the paper).

In the below listed specific comments references are made to specific lines in the text.

1. Methodology and modelling system structure

a. The model coupling implements aerosol impacts on radiation (direct and semi-direct effects) and on clouds (first and second indirect effects), l. 110-111. It appears appropriate to include an explicit reference to COST action ES1004 in the framework of which these effects were extensively discussed.

b. The cloud feedback module includes some rather advanced approximations (l. 287-293); the reader would welcome more remarks on the extent to which this complex cloud model has been validated.

c. The present HIRLAM NWP model core is based on the hydrostatic approximation (l. 127) which could be a serious limitation over complex terrain (l. 508) and/or in cases of

nesting down to urban areas. A plan for a transition to a new, non-hydrostatic platform (e.g. HARMONIE, l. 135) is mentioned, but more information in this respect would be helpful.

d. The atmospheric chemistry modules implement a wide array of new parameterisations and numerical schemes (page 4, l. 141-192). Although these were obviously validated separately, their combined implementation in a coupled model definitely needs further validation. Did the authors take already actions in this direction, and if not, what are their plans?

e. The aerosol dynamics model introduces a very interesting classification of particles depending both on particle size and particle composition per emission source. This could allow, in theory, a separate per-source type treatment of particles throughout the chemical mechanism. But is there such a procedure (with potential applications in source apportionment) really implemented or planned?

f. Specific emission models for anthropogenic biomass burning (e.g. wildfires) are included (section 2.5). These are based on satellite or other inventory-estimates of yearly fluxes that are temporally disaggregated using pre-defined temporal profiles. Are the latter site dependent, and which is the origin of the coefficients used by the authors?

g. The model contains several "urbanisation" features (section 2.7), including a subset of previously proposed urban parameterisations (Martilli, Dupont, Masson, Grimmond et al.). This is an interesting and original approach, but there are several concerns on how it is implemented (please see comment 2c below).

h. In l. 372-378 some aspects of the so-called locally mass conserving semi-Lagrangian (LMCSL) transport scheme are described. The description emphasizes the approximate mass-conserving properties of the algorithm for 1st neighbour cells, but one could ask whether and how is mass consistency ensured in the larger scale. In l. 389-390 it is stated "[. . .]Enviro-HIRLAM is not formally wind-mass consistent re-

garding tracer transport". The authors should discuss possible consequences of this failure.

2. Model applications and validation

a. Sensitivity studies on the model response to aerosol effects do indicate some strong "signals" (difference between coupled and uncoupled runs), e.g. l. 418. But these do not necessarily imply an improved model performance, and the authors should state this clearly in the manuscript, cf. l. 420 "[Korsholm (2009)] found a marginally improved agreement[. . .]", and l. 464-467 "However. . .it is too early to make conclusions about the improvement of precipitation forecasting by implementation of the indirect aerosol effects, because of large uncertainties in parameterisation . . . and due to adjustments of such effects. . .and constants".

b. This referee believes that careful tuning is needed in view of the large number of parameters in the complex feedback modules, especially with regard to cloud effects. It is not obvious how and to what extent this could be achieved only by comparing final simulation results (i.e., without a further quantitative study of the cloud physical mechanisms themselves).

c. An evaluation application for the urbanisation modules was performed for the cities of Paris and Bilbao. There are several issues regarding this application that are neither explained in the text nor in the referenced publications:

i. A domain spatial resolution of 2.5 km appears to be insufficient for such an application.

ii. The resolution of the BEP dense sub-grid is not mentioned. Is it also 2.5 km?

iii. The authors seem having assumed only four urban classes, cf. Figure 10. Such a classification would ignore the important role of green urban areas in UHI evolution.

iv. Is the 2.5×2.5 arc-minute resolution ($\sim$5km) of the AHF data adequate for assessing UHI effects in an urban scale? In the Bilbao case it appears that the entire urban area

is covered (and classified) in only 16 cells!

v. Are AHF data constant during the day, or do the authors assume an intrinsic diurnal profile?

vi. Values of 40 or 60 Wm-2 for the AHF are mentioned. Is this a mean annual value or a daily estimation following a seasonal profile?

vii. Concerning the validation process, it is unclear whether a combination of statistical indicators is used or just the correlation coefficient. Not much evidence is presented (e.g. in form of figures or tables) that the model reproduces satisfactorily the mesoscale features.

viii.It is well documented in the literature that the Paris UHI is expanding just after midnight, but not that this expansion lasts until 11 UTC, especially during a summer period. Comments by the authors would be welcome.

ix. Confusion is caused by the fact that in the second paragraph of section 3.2 the authors claim that the model was applied for July 2009, while in the last paragraph of the same section they write "...showed that under calm conditions during summer and winter...".

d. Enviro-HIRLAM is operationally used for birch pollen forecasting in Denmark. This appears to be one of the more mature applications of the model, with rather advanced emission, deposition and scavenging modules. However, no mention is made on the effect of online coupling (and the relevant feedbacks) on these simulations. In the conclusions it is mentioned that feedbacks are not important in pollen forecast (l. 711-712). How did the authors reach this conclusion?

e. Section 3.4 attempts an evaluation of the feedback effects on air pollution forecasting. It is mentioned that online coupling improves the forecast skill, however without referring to specific applications, as for instance the MEGAPOLI Paris campaign.

From a technical point of view, the paper is excellent. Yet, the authors should check it

again for inconsistencies (e.g., both "online" and "on-line" are found in the manuscript).