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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of resolution on the relative importance of numerical and physical diffusion in atmospheric composition modelling" *by* M. D'Isidoro et al.

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We acknowledge the comments made by the anonymous referee #3 that helped us to formulate part of the paper in a more convenient way in order to clarify the formulation and the definition of the terms used.

In particular, the Section 3 was completely restructured because it generated some confusion. Introduction was also deeply revised, trying to rigorously define the terms used in the paper.

Improvement on readability that are expected upon the revision of the text by an English editor, should also increase the rigor of the formulation.

The Rerefee's comments also pushed us to stress the meaning of non-dimensional quantities and, in particular, the resolution.

Detailed answers to all specific comments follow:

[1] A major trouble with this paper is that it lacks rigor and clarity. Certain words used are not suitable for a scientific journal. For example, on Page-22866 ": : :.. competition between physical diffusion ..", and on Page-22868, first paragraph: "spread of cloud .." , and ".. spread is referred to as ..", sound very weak. The terms diffusion (spread!), ...

Terms lacking rigor and clarity have been eliminated and text has been edited by an English editor to make it clearer.

Moreover, there should be a clear explanation about the difference between "physical diffusion" and "numerical diffusion." In the manuscript these terms are introduced in a confusing manner..

The concept has been clarified in the Introduction giving clear definition of terms and eliminating other terms that created some confusion.

[2] On Page 22866: Many practical transport schemes (including WAF) employ monotonic (non-oscillatory) limiters to control spurious oscillation (also to remove small scale structures). Did you mean the "smoothing" as an effect of limiting or filtering? It is not clear in the manuscript, please make it clear.

Some sentences in the Introduction were rather unclear. The section has been rephrased to avoid confusion.

[3] Equation (4) is the advection equation in non-conservative form, however, the Toro's WAF scheme you are using is based on conservative (flux) form. Then a nondimension version of advection equation (5) is introduced and an idealized solution is derived in terms of variance. However, authors fail (or ignore) to explain what is the purpose of this, and how this procedure is incorporated in to the WAF-based model. A more direct

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discussion should be given in the revision.

The non-dimensional version of the discretised advection equation was made to generalize the problem, finding the parameters relevant to our analysis. Then, the numerical simulations were made using the WAF scheme, whose solution is consistent with modelling an advection equation. Finally the results were compared in terms of the non-dimensional parameters. We think this point arose because of a misleading organization of the section "Remarks on numerics" in the text. We tried to make this part clearer by reorganizing Section 3 and rephrasing some sentences.

[4] On Page-22871: For the benefit of the readership, please plot the initial conditions and the final simulated solution for the numerical experiment. Also the quality of the figures should be improved (numbers and axis markings etc.) What is "BOLCHEM"? Don't expect every reader is an expert of this model, please provide more details.

A plot with initial conditions and final simulated solution has been added as suggested (Fig. 2). More details and references about BOLCHEM have also been provided.

[5] In the conclusion it is given that "numerical diffusion depends on only two parameters: resolution and Courant number". But the resolution (dx) and the Cournat number (U dt/dx) are dependent; then why not say 'numerical diffusion depends on only the resolution and the wind field (U)', which are independent. A justification is needed.

Here, the referee probably confuses the grid resolution (Δx) with the resolution ρ introduced in Section 3, which is the ratio between the source size R and the grid resolution Δx.

The Smagorinsky (1963, Mon., Wea. Rev, Vol.96, 99-165) type "physical" diffusion is widely used in many high-resolution meteorological models and engineering applications, and it appears very promising. Is it possible to apply such a diffusion scheme in your model?

As the referee says, a description of sub-grid turbulence should be adopted in high-

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resolution simulations. We added a comment about this in the Conclusions.

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