

# ***Interactive comment on “Construction of an Eulerian atmospheric dispersion model based on the advection algorithm of M. Galperin: dynamic cores v.4 and 5 of SILAM v.5.5” by M. Sofiev et al.***

## **Anonymous Referee #2**

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Review of

Construction of an Eulerian atmospheric dispersion model based on the advection algorithm of M. Galperin: dynamic cores v.4 and 5 of SILAM v.5.5

by Sofiev et al.

The paper is mainly a description of the tracer advection scheme of the Eulerian dispersion model SILAM. The advection scheme algorithm was first presented in Galperin (2000, in Russian, and 1999, conference proceedings). The paper hopes to provide a reference of the scheme in the peer-reviewed (English) scientific literature. The paper further discusses improvements of the scheme necessary for its application in SILAM.

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The performance of the scheme is demonstrated in idealised test cases, some of the test cases follow Lauritzen et al. (2012).

In my opinion the paper does not succeed to adequately inform the reader about the advection scheme in SILAM. It does not contain enough information on the implementation details and the originality of scheme. Occasionally, the paper suffers from lack of sufficient evidence about the statements made in abstract and conclusion.

#### General remarks

1) The advection scheme developed by M. Galperin is not adequately described in its 3D implementation.

The authors convey the impression that the advection scheme is somehow unique without any similar approaches presented in the literature. I don't think this is the case and the authors should make a bigger effort to explain the origins of the scheme. Although the authors include a literature overview of advection schemes they do not relate it to their scheme well enough. I suggest to focus in the literature review only on similar (i.e. volume and Lagrangian) advection schemes. The presented scheme by Galperin seems to have a lot in common with the "scheme of moments" by Egan and Mahoney (1972) using only first order moments. The authors should discuss much better how their scheme relates to Egan and Mahoney (1972), which has often been cited, and similar approaches of "slab schemes" discussed for example in Rood (1987, Review of Geophysics).

Although the 1D implementation is presented by formulae too little effort is made to explain the three dimensional formulation as this seems to me not as easy as the authors state ("Generalization of the above 1-D algorithm to 2- and 3-D spaces is straightforward: slabs become rectangles or cuboids and 1-D integrals are replaced with 2- and 3-D formulations, respectively.") In particular the remapping to the grid point presentation of SILAM after the advection seems to be not trivial.

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2) The title of the paper (as well as the running title) is not adequate.

A title such as “The tracer advection scheme of SILAM v5.5” would reflect better the content of the paper. The title is not adequate because of the following reasons: (i) “Dynamic core” suggest that SILAM solves the “dynamical” equation for evolution of wind and temperature. Instead, only the tracer advection scheme (and loosely also the diffusion scheme) using off-line wind fields is presented. (ii) The paper only mentions in one sentence the differences between v4 and v5 of the advection scheme but it does not discuss in any way the differences between the two. (iii) I find it unusual to mention a name in the title (M. Galperin), which is not very well established in the scientific literature.

3) The sections on aerosol dynamics, diffusion and dry deposition do not fit well in the existing structure of the paper.

The above mentioned sections do not relate well enough to the overall purpose of the paper to document the advection scheme. More importantly, they contain too little detail and no evaluation at all to justify them in a peer-reviewed scientific paper. They should preferably only be included in connection with the specifics of the advection scheme. The section on aerosol dynamics is an unhelpful diversion in particular in its current position. The diffusion scheme description is too general and to little connection to the advection is made. The interesting aspect of exploiting the sub-gird scale information of the scheme should be given more space and its benefits should be demonstrated.

4) The testing of the scheme needs to be discussed in more detail and inter-compared with the performance of other schemes.

Currently, the Lauritzen tests are predominately presented in Figures and too little effort is made to compare the performance with other schemes. The reader wants to know how well the scheme performed. The only inter-comparison of the Galperin scheme seems to be carried out by Petrova et al. (2011). They concluded that scheme by Galperin is very good for point sources but has serious issues with 3d advection prob-

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lems (i.e. cones, gauss-shape etc.) The authors should start from this study and demonstrate any progress made.

5) The summary contains statements about the importance of the sub-grid scale information of the advection scheme but the paper contains too little to support this.

The second paragraph of the summary on the sub-grid information of the scheme is interesting but I did not find enough information about this aspect in the paper. It is only briefly mentioned but would need to be treated with much more detail in the paper to deserve a full paragraph in the summary. Also, the abstract needs to better reflect the actual content of the paper. The link between advection and the other modules is not well discussed in the paper.

Specific remarks

P 2905: Change title according to 2) above

P 2906 I1: The paper does not discuss the differences between v4 and v5 to justify mentioning it in the abstract

P 2906 Introduction: Try to be more focused on the main topic of the paper, i.e. the SILAM advection scheme.

P2907 Introduction: there are already good overview papers (e.g. .Rood et al 1987, Lauritzen 2012) which deal with many of these issues. You should refer to them.

P 2907 I15: Please quote for example Williamson and Rasch (1990) or Machenhauer (2008) for a list of the advection scheme requirements.

P 2908 I6: Kukkonen et al. (2012) is a more review of regional CTMs, they would not be called “atmospheric models”.

P 2908 I16: Give examples for forward schemes (the Galperin scheme seems to be one), point out that most SL schemes are backward schemes. Discuss the pro and cons of forward and backward schemes.

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P 2909 I1-13: consider shortening or omitting

P 2909 I25: Please explain in much more detail how the Egan and Mahoney scheme relates to the Galperin scheme. What is the same and what is new in Galperin? It is good scientific practise to explain with much detail the novelty aspect of the new scheme with respect to other papers.

P 2910 I1: The reference to the aerosol dynamic modelling is very confusing. You either explain in more detail the communalities between transport and aerosol modelling or omit this statement here.

P 2910 I7-29: I recommend removing this part as you do not give enough room to this topic in the paper.

P2910 I10: Smagorinsky (1963) is essential for LES studies but it would be not correct to claim his seminal work as the origin of all diffusion schemes in numerical models. There much more ways to solve the vertical diffusion equation in numerical models.

P 2911 I1: Point out the difference between the two versions. Please discuss the difference in the text as well. It does not make sense otherwise.

P 2911 I11: Consider removing the list of the units here.

P 2911 I15: Please explain the differences in the formulation and show the impact in the paper.

P2911 I24: Please use the same (case, type) letter phi as in formulae above.

P2911 I24 please clarify what is meant by pollutant (Gaussian the same (case, type) letter phi as in formulae above.

P2911 I23 (formulae 1): Why do you distinguish between emissions and “transformation sink and source processes”? Should the Emissions not be treated as boundary flux conditions and therefore not appear in this local formulation?

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P2912 I17: I suggest omitting the whole introduction about “the first time in the international literature”. It would be OK to make such a statement if you refer to the paper of someone else but it is not common to praise your own paper in this way.

P2912 I20: I did not manage to look into the 2000 paper but the other contains very little information about the scheme. This means I could not get any information and it is the job of this paper to explain the scheme in a comprehensive way. The most convincing, although also very short description could be found in Petrova et al. (2008).

P2913 I14: Is  $U$  defined for the grid box centre or at the borders. Make statement about grid-staggering between  $U_i$  and the other variables.

P2913 I17: (formulae 6) Is  $\phi(x)$  a continuous function in  $x$ ? How is this dealt with numerically as  $\phi$  will only be available at grid point centres?

P2914 (formulae 7 and 8) How are the integrals evaluated at the point  $x_i \pm 0.5$ ? What interpolation (if any) is used?

P2915 I4: Please comment on the fact that the position  $X_i$  does not coincide with the grid box centre locations of grid box  $i$ .

P2915 I4: A bit more detail on the remapping procedure is needed here, in particular when the algorithm is used in 3D.

P 2915 I5: Please show that the scheme is mass conserving and that it is numerically stable for high Courant numbers.

P2915 I10: Please provide more detail on the 3D implementation.

P2915 I17: It would be helpful to explain the spatial discretization of SILAM (tracers, wind) at this point.

P2916 I18: Please show how  $P_{x/y/z}$  relate to  $M_{xyz}$ .

P 2917 I8: Include year after Ghods et al.

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P 2917 I17: I thought the wind was defined at the grid box centres. Please clarify if the centre or the border of the slabs are advected according to the equations in section 2.2

P 2918 I3ff: The whole section 3.3 seems not correctly placed in the overall structure of the paper and should best be deleted for the sake of brevity.

P 2920 I10: Strange title, consider omit “Vertical axis”

P 2920 I12: Is the Galperin scheme used also for the advection related to dry deposition?

P 2920 I15: How does the scheme deal with time varying vertical coordinates (hybrid)

P 2921 I3: Interesting point but please provide more detail.

P 2921 I13: Please discuss the link to the wind fields as part of a description of the scheme. These issues are not trivial and need careful consideration.

P 2921 I18-28: I am not sure this is of interest for the paper, which focuses on the advection.

P 2922 I7: please summarize the findings of Petrova et al. (2008), in particular the problems with the advection of non-point sources (cones etc.)

2922 I12-18: Please discuss these Figures. There is no point in just mentioning them here. If the figures are not properly discussed, they should not be included in the paper.

P 2923 I3: No need to repeat the test setup here, refer to the original paper, consider removing section 4.2.1 and 4.2.2

P 2925 I21ff: The whole paragraph makes only sense if you relate to the performance of a different scheme. It cannot be concluded from these performance numbers that the scheme is efficient or not. Some statements about the parallelism of the implementation would be helpful.

P 2925 I16: You do not discuss in any way the differences between v4 and v5.

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P 2925 I18: Please list the challenges and discuss them.

P 2926 I3ff: This is more or less true for any SL advection scheme. It is not specific for the Galperin scheme.

P 2927 I24 ff: Please discuss the figures in more detail. Please try to compare the performance to other schemes as done Petrova et al. (2008) .

P 2927 I19: Avoid using “tough”

P 2927 I21: Was this discussed earlier? More detail is needed how the SILAM scheme compared to other schemes presented in L14 and Kaas et al. (2013)

P2928 I7: What do you mean by “high optimal”, please explain.

P 2929 I17: The title of section 5.3 is not very descriptive. Also. I don't believe this need to be include in the discussion as it is too general. Consider omitting the whole section 5.3. The multi tracer approach could be mentioned elsewhere.

P 2929 I18: omit v4 and v5

P2929 I22-I26: The aspect of the sub-grid scale is not sufficiently discussed to justify this paragraph in the summary.

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Interactive comment on Geosci. Model Dev. Discuss., 8, 2905, 2015.

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