Response to the referee comments for the paper by Sofiev et al,

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

Taking this chance, we would like to express our gratitude to both referees for their comments and efforts. They were all carefully considered during the preparation of the revised paper version. Below, we provide detailed answer to each critical comment.

Main changes in the paper

Following the reviewer's comments, several sections of the paper have been significantly modified:

(i) the title changed to "Construction of an Eulerian atmospheric dispersion model SILAM v.5 based on the advection algorithm of M.Galperin". The term "dynamic core" has been replaced to "transport module" throughout the paper.

(ii) section 3.2 is reformulated and extended to include the formulations for the 2- and 3-D cases; we also added a section positioning the scheme among others

(iii) separation of the transport modules v.4 and v.5 has been removed since the scheme formulations stay the same. This also resolves the confusion between the model versions v.5.4 and v.5.5.

(iv) we separated the improvements of the transport module from its connection to other model units. Both sections were reviewed in light of the reviewer's comments. In particular, section 4.3 on aerosol size spectrum treatment has been removed but shape preservation issues and meteo-to-dispersion interface are explained in more details

(v) the improvements of the scheme in SILAM are linked to the assessment by Petrova et al. We presented the exercises similar to those of Petrova et al to demonstrate the improved performance of the refined scheme version. Also, new test is added with real-life 3-D wind field from ERA-Interim meteorological archives.

Comments of the Referee 1

This article presents a scalar advection scheme, of which there are already very many, as shown in the introduction. In particular, this Galperin scheme is already well documented in the several referenced papers of Galperin.

In fact, the Galperin's advection scheme has never been presented in open peer-reviewed literature in any level of details – as we said in the paper and as is also stressed by the Referee 2. Therefore, we disagree with the comment.

The paper mentions the development of the SILAM dynamical cores, the model's boundary conditions, the need to modify the scheme to be useful over complex topography or near strong gradients and fronts, vertical advection, modeled diffusivity and turbulence mixing, and aerosol size spectra advection, but the only results shown are for idealized 2D scalar advection tests, and does not demonstrate any potential advantages for this advection scheme, other than that it performs modestly well in these tests and converges at only first order.

We again have to disagree: the model shows quite rare skills in preserving delta-function-type fields (figure 5) and other sharp-gradient patterns. This is of utmost importance for various emergency-type applications, which resulted in Lagrangian models usually applied to those tasks: Eulerian ones are too diffusive. Secondly, the scheme converges faster than the first-order, as shown in Figure 10. The only exception is the slotted-cylinder test and the L-infinity norm, which are not even shown in other publications related to this test case.

The only test that demonstrates the advantages of the new scheme is in Figure 4, showing an improvement over the original Galperin scheme. Given that this scheme has been implemented in a full 3D dynamical core and a full physics atmosphere model, it should be possible to demonstrate all of these things, rather than showing numerous traditional test cases which do not illuminate the improvements made (presumably with much effort by the authors) to this scheme needed for realistic model simulations. Since there are so many existing scalar advection schemes, the authors need to show what distinguishes this scheme from the others, especially once it is coupled to a dynamic model.

We followed the standard procedure that required evaluation with the basic tests to demonstrate the overall credibility of the approach before going into the complicated cases. Secondly, these simple tests have analytical solutions that can be used as the references.

• *Are there any demonstrations of this revised scheme in either a 3D dynamical core or in a full model?*

In the revised paper, we added the full-3D case study demonstrating the scheme consistency.

• *How well does this scheme maintain a given aerosol size spectrum? How much improvement is found by using the method in section 3.3?*

Following also the criticism of the Reviewer 2, we removed this section

• *How well does this scheme perform near complex topography?*

The new real-life test case shows that: it is global and the results show no error growth in vicinity of any topographic inhomogeneities.

• *First-order advection schemes may be too diffusive for many realistic applications. Are there any plans to extend this scheme to better than first-order accuracy?*

We did not understand the criticism: the scheme is completely non-diffusive and converges faster than the first-order, as pointed out in the paper.

Comments of the Referee 2

We thank the reviewer for the detailed and constructive comments and hope that the below modifications resolved the issues

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.*

We added: (i) the detailed presentation for the multi-dimensional scheme implementation, (ii) a section positioning the scheme against the existing developments, first of all, the schemes of Egan & Mahoney and Prather. The review by Rood is now included. The wording of the corresponding sections has been critically reviewed.

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.

The title has been revised as stated above and the word "dynamical" removed throughout the paper to avoid misunderstanding. We think, however, that the name of the original scheme developer should be clearly associated with it. Since the original scheme is one of the major developments of Prof. M.Galperin, this seems to be a right way to reflect the contribution of this scientist, who was well-known in Russia but unfortunately less recognized in Europe.

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.

The aerosol dynamics section has been removed. Diffusion and dry deposition sections have been reformulated to provide clear links to the advection scheme. The importance of this section is indeed in the sub-grid information taken for the diffusion and dry deposition implementations – as provided by the advection.

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

We expended the existing tests, added the shapes used by Petrova et al and showed reduction of the problems and improvements of the shape preservation.

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.

The sub-grid information topic received more attention in the revised paper, in particular, in connection with the emission, diffusion and dry deposition processes.

Specific remarks

P 2905: Change title according to 2) above

The title has been reformulated

P 2906 l1: The paper does not discuss the differences between v4 and v5 to justify mentioning it in the abstract

Removed

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

Several sections have been removed (see above and also below responses) to concentrate on the main message.

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.

Thank you for the suggestion! The Lauritzen reference was actually mentioned in the paper, now we pushed it up and made more visible, together with Rood reference, which previously was indeed not included. The introduction was also shortened.

P 2907 l15: Please quote for example Williamson and Rasch (1990) or Machenhauer (2008) for a list of the advection scheme requirements.

The references have been added

P 2908 l6: Kukkonen et al. (2012) is a more review of regional CTMs, they would not be called "atmospheric models".

Corrected

P 2908 l16: 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.

Added

P 2909 l1-13: consider shortening or omitting

The paragraph is strongly shortened

P 2909 l25: 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.

A dedicated sub-section is added after the description of the Galperin's algorithm.

P 2910 l1: 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.

The section is removed

P 2910 l7-29: I recommend removing this part as you do not give enough room to this topic in the paper.

P2910 l10: 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.

The sub-section is removed from the introduction.

P 2911 l1: Point out the difference between the two versions. Please discuss the difference in the text as well. It does not make sense otherwise.

Since the difference between the two versions is only in the implementation of ideologically the same algorithm (the v.4 is defined in metric space, v.5 is in mass space, with corresponding units for grid cells and wind speed), the description has been unified.

P 2911 l11: Consider removing the list of the units here.

The units were added following the request of the Editor at the initial-submission stage

P 2911 l15: Please explain the differences in the formulation and show the impact in the paper.

The description has been unified (see above)

P2911 l24: Please use the same (case, type) letter phi as in formulae above.

P2911 l24 please clarify what is meant by pollutant (Gaussian the same (case, type) letter phi as in formulae above.

Corrected

P2911 l23 (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?

Sinks due to transformation are a function of available mass, linear in formula (1), whereas emission is independent from it. Therefore they have to be distinguished. Secondly, emission cannot be considered as a boundary condition because in many cases it happens into the upper layers of the model (buoyant fluxes from fires, aircraft emissions, dust storms, high stacks, etc).

P2912 l17: 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.

Removed

P2912 l20: I did not manage the look into the 2000 paper but the other contains very little information about the scheme. This mean 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).

Indeed, as we stated in the introduction, the scheme is practically not presented in the literature. The reference to Petrova et al is added.

P2913 l14: Is U defined for the grid box centre or at the borders. Make statement about grid-staggering between Ui and the other variables.

In the original scheme, U is in the centre, our improved version uses staggered wind. Clarifications are added.

P2913 l17: (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 xi +/- 0.5 ? What interpolation (if any) is used?

Phi is defined for every x, and gives a piece-wise continuous representation of the concentration in terms of the slab functions Pi(x). However, pointwise evaluation of phi(x) is not needed, but only the integrals over grid cells. The integrals, in turn, are straightforward to evaluate, since phi(x) is a sum the slab (box-shaped) functions Pi(x). We reformulated this section to avoid confusion and to make the extension to multi-dimensional case. Since the phi is a piece-wise continuous function of x, no interpolation is needed.

P2915 l4: Please comment on the fact that the position Xi does not coincide with the grid box centre locations of grid box i.

P2915 l4: A bit more detail on the remapping procedure is needed here, in particular when the algorithm is used in 3D.

The extension to 3D case is added at the end of the section, more details of the remapping procedure are provided.

P 2915 l5: Please show that the scheme is mass conserving and that it is numerically stable for high Courant numbers.

Clarification is added at the corresponding places of the scheme description

P2915 l10: Please provide more detail on the 3D implementation.

The procedure is added

P2915 l17: It would be helpful to explain the spatial discretization of SILAM (tracers, wind) at this point.

The discrete representation of tracer and wind are clarified in the corresponding places. Since our modifications of the scheme changed wind form non-staggered to staggered, it would be too long to write it here.

P2916 l18: Please show how Px/y/z relate to Mxyz.

Did not understand the request: the place contains no P or M notations.

P 2917 l8: Include year after Ghods et al.

Corrected

P 2917 l17: 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

Clarification added. This is the very improvement suggested to the scheme: using two wind values at the cell borders instead of just one in the cell centre as in the original scheme.

P 2918 l3ff: 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.

The section is deleted

P 2920 l10: Strange title, consider omit "Vertical axis"

Corrected

P 2920 l12: Is the Galperin scheme used also for the advection related to dry deposition?

Clarification added. The dry deposition is considered as a combination of two types of processes: the surface uptake (the actual deposition) and, for aerosols, gravitational settling, which is volumetric. Near the surface these two processes are combined as described in Kouznetsov & Sofiev, whereas in the upper model layers the sedimentation velocity is added to the vertical wind species-wise – and thus treated by the Galperin's scheme.

P 2920 l15: How does the scheme deal with time varying vertical coordinates (hybrid)

A short clarification added in the meteo-to-dispersion interface. The changes of the grid cell volumes are treated explicitly and reflected in the continuity equation strictly enforced in each grid cell.

P 2921 l3: Interesting point but please provide more detail.

This section is expanded, as well as other interfaces between the advection and other modules of SILAM. The use of sub-grid information received more attention in the revised paper.

P 2921 l13: 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.

This section is expanded and clarified, as well as other interfaces between the advection and other modules of SILAM. The use of sub-grid information received more attention in the revised paper.

P 2921 l18-28: I am not sure this is of interest for the paper, which focuses on the advection.

The paragraph is removed

P 2922 l7: please summarize the findings of Petrova et al. (2008), in particular the problems with the advection of non-point sources (cones etc.)

Added

2922 l12-18: 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.

The discussion is added

P 2923 l3: No need to repeat the test setup here, refer to the original paper, consider removing section 4.2.1 and 4.2.2

The section has been shortened leaving only pieces needed for the further discussion

P 2925 l21ff: 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.

There seems to be a mis-understanding: the numbers are directly compared with the HEL and CSLAM schemes just here. The text is refined to avoid confusion. A short parallelization discussion is added.

P 2925 l16: You do not discuss in any way the differences between v4 and v5.

The dual-version approach has been removed since the difference between the versions is only technical.

P 2925 l18: Please list the challenges and discuss them.

Added.

P 2926 l3ff: This is more or less true for any SL advection scheme. It is not specific for the Galperin scheme.

Well, not quite: backward schemes may have problems with out-of-grid transport; point-wise schemes are not inherently mass-conservative, as well as some of the schemes using non-linear approximations (e.g. Bott). All these schemes require special efforts to reach the mass conservation and accountability (e.g. renormalization in Bott scheme). For the Galperin's scheme, these features follow from the very basic formulations and do not require any additional effort. The sentence is reformulated to highlight this peculiarity.

P 2927 l24 ff: Please discuss the figures in more detail. Please try to compare the performance to other schemes as done Petrova et al. (2008) .

Done

P 2927 l19: Avoid using "tough"

Reformulated

P 2927 l21: Was this discussed earlier? More detail is needed how the SILAM scheme compared to other schemes presented in L14 and Kaas et al. (2013)

The discussion is expanded

P2928 l7: What do you mean by "high optimal", please explain.

Reformulated

P 2929 l17: 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.

The section has been removed, with some pieces moved upwards to illustrate the challenges of the scheme implementation.

P 2929 l18: omit v4 and v5

Done

P2929 l22-l26: The aspect of the sub-grid scale is not sufficiently discussed to justify this paragraph in the summary.

The sub-grid topic has been extended in the revised paper