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Geoscientific Model Development Discussions

Interactive comment on "Using an antidiffusive transport scheme in the vertical direction: a promising novelty for chemistry-transport models" *by* Sylvain Mailler et al.

Anonymous Referee #1

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The authors use idealized tests to quantify the performance of the Després and Lagoutière (DL99) advection scheme for the calculation of vertical constituent transport in chemistry-transport settings, comparing it to both classical schemes such as Van Leer and (relatively) modern schemes such as the Piecewise Parabolic Method (PPM). They find that DL99 may be able to produce a more accurate simulation of the transport of thin atmospheric layers than the standard PPM or Van Leer approaches, in spite of being formally lower-order in accuracy. This is an interesting and potentially highly significant result, due to the known difficulties of simulating such layers in the atmosphere and the computational cost of increasing the vertical model resolution – the only serious solution yet suggested elsewhere in the literature.

The central question addressed by the authors, of how to efficiently address the issue of numerical diffusion in CTMs, is important and timely. Their use of an idealized test to complement their earlier tests in a "real-world" situation is appropriate, both in scientific terms and for GMD. Their conclusions are well supported by the data, with the exception of one comment (see below). The identification that an efficient, first-order accurate vertical advection scheme might be able to help address the long-standing issue of numerical diffusion is a significant advancement in the field.

Overall, I found this manuscript to be significant, well-written, and concise. I therefore recommend it for publication, once a small number of issues are addressed. I have sorted my comments into major, minor, and superficial.

Major (substantive) comments:

1. While I understand that the Upwind and DL99 schemes are first-order, it seems like an unnecessary confounding to use different operator splitting methods for these two methods in comparison to the VL and PPM schemes. It would be useful to see a comparison where all schemes are using (e.g.) the Strang operator splitting. Although this is not expected to yield an improvement in the Upwind and DL99 results, it would at least verify that the improved performance is not because of the operator splitting approach. Given the performance characteristics of DL99 (i.e. low sensitivity to small CFL numbers) one would hope that Strang's scheme also would not compromise its accuracy, although it might incur an unnecessary expense.

2. It seems like an oversight to not invoke Godunov's theorem (Godunov, 1959), especially on lines 306-308. It is a known result that any higher-order scheme cannot exceed first-order accuracy in the vicinity of a sharp gradient, so it is not true that "higher-order schemes are expected to reduce numerical error at any given resolution".

3. On line 327, the authors state that "if model resolution is fine enough to represent properly the plume, then higher-order schemes are still a better choice", but I am not

sure this is true (or that this manuscript even supports that conclusion). An important point they raised is that the DL99 scheme does a good job even in the situation of low CFL numbers (line 140), and it seems that such conditions are likely to be common when considering vertical movement in the atmosphere. I would recommend that this conclusion be removed or at least made more precise to account for the fact that it may only be true under certain conditions. This is hinted at through the final sentence of the discussion (line 318) but the authors are understating the importance of this point. The implication that increasing vertical resolution may be an inefficient solution for even higher-order methods is a potentially significant finding.

I also have some minor comments:

1. Figures 1 and 2 would be improved by using the same color scale for all panels (i.e. 0-20 ppb for Figure 1, and 0-100 ppb for Figure 2)

2. I would suggest that the authors consider replacing “promising novelty” in the title with, say, “promising and novel solution to ”. I think that “novelty” makes the work sound unimportant, whereas I found this work to be intriguing and of high value.

Finally, I tried to make a note of any typos or grammatical errors I found. However, I would suggest that the authors make an additional sweep for grammatical accuracy:

1. Line 9: “an important direction into improvement” doesn’t quite make sense. Perhaps “necessary step in the development”? 2. Line 17: “too much observations” should be “too much compared to observations” 3. Line 72: “permit” should be “ensure” or similar 4. Line 73: there is a spurious space between the closing bracket and comma. 5. Figure 3 caption: “shox” should be “show” 6. Line 309: “teh” should be “the” 7. Line 328: “enaugh” should be “enough” 8. Line 336: “adress” should be “address” 9. Throughout: “1d” should be “1D” or “1-D” 10. Throughout: some language is somewhat nonscientific (e.g. “spectacular” on line 298 is hyperbolic)

References

Godunov, S. K.: A difference method for numerical calculation of discontinuous solutions of the equations of hydrodynamics, Matematicheskii Sbornik, 47(3), 271–306, 1959.

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