Geosci. Model Dev. Discuss., 3, C598–C603, 2010
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Geoscientific Model Development Discussions

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Interactive Comment

Interactive comment on "Mass-conserving tracer transport modelling on a reduced latitude-longitude grid" *by* D. Belikov et al.

Anonymous Referee #2

Received and published: 13 December 2010

General comment

The authors describe the implementation of a flux-form advection scheme in the global transport model NIES TM. The manuscript points to all issues that are important here, such as mass conservation and resolution of the driving meteorological data. Different implementations are compared using standard tests or real-life applications.

The described methods are not new in the sense that they introduce new idea's or applications in the field of global atmospheric transport. But the manuscript could certainly serve as a reference for future use of the NIES-TM model, and would therefore be suitable for publishing in GMD. However, the current manuscript could strongly benefit from minor or even major revision.

Main comment is the focus of the paper. The title suggests that the paper either proposes a new method for implementing tracer transport, or provides a very broad overview on the subject. None of these is the case however, since the paper mainly describes how the new implementation for NIES-TM is made, and how this compares to the previous implementation. It would be much better if the title should reflect this, for example "Comparison of semi-Lagrangian with flux-form transport schemes in NIES-TM" . The paper would then clearly be a reference paper for the NIES-TM model, and in that way serve as an example of how global transport could be implemented and what users should be aware of when implementing transport into their own model.

In addition, some issues that are important for the implementation but also for the interpretation of the results are not described well, which makes it difficult to judge the results. For example, since the title now suggest that the mass-conservation is an important issue throughout the paper, I would expect that at least eq. (3) is explained in more detail (or be less confusing, see specific comments). Also, it is not clear how the data from the two meteorological models are mapped to the transport model. This should be clarified in more detail to let the community fully benefit from this paper.

Specific comments

page 1740, lines 15-24. The need for high resolution depends strongly on the gradients present in tracer concentrations. Rather smooth concentration fields such as found for methane will benefit less from higher resolution than chemical active species such as NO_2. Please comment on this.

page 1742, line 23. "... spectral fields, which are not available ..." Why are these not available ? On page 1743, line 11, the authors mention actually that they derive mass fluxes from spectral data, and the preamble of section 2.3 tells that both driving models are spectral. So if the authors would have wanted to use spectral fields, they could have been obtained I assume. But available or not, to compute mass fluxes through cell interfaces from spectral fields one needs to evaluate them on a high resolution

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lon/lat grid anyway. Thus, having the spectral fields available is not essential if high-resolution lon/lat fields are available.

page 1743, line 16. "... m_s denotes the mass in the cell, ..." Shouldn't this be "the mass in the column" ?

page 1743, eq. 3. Don't understand the algorithm. Is a different "\delta F_c" computed for each layer "l" ? Shouldn't it be that F_c is computed for the entire column, e.g.: \delta F_c = - [\sum_{l=1,N} \delta\Phi_h(l)] - dm_s/dt followed by some distribution of F_c over the individual layers ? Please clarify this in detail.

page 1744, line 15. "... the semi-Lagrangian algorithm cannot meet this requirement". Why not ? Is the problem efficiency or mass-conservation ?

page 1744, line 18. "... the function on the edge of the control volume" . What function are we talking about here ? Just the tracer concentration ? For those that are not familiar with the Van Leer schemes, what does the 3rd order Van Leer scheme assume for the inner-cell concentrations ?

page 1744, line 23 and further. I'm confused. To my understanding, the second-moment scheme simply expresses the inner-cell concentrations in terms of a 2nd order polynomial. The actual concentrations at any point in the cell are then described by a number of coefficients; in 3D the number of coefficients is 1 for the zero-order moment (average), plus 3 for the first-order moments (slopes), and another 6 for the second-order moments. This makes the scheme more expensive in terms of storage and cpu since now 10 coefficients have to be updated. If this is correct, what implications would that have to splitting advection into the 3 directions ? Decomposing the advection into x, y, and z is related to operator-splitting, which is necessary for all flux-form advection schemes, not only for second-moments. Therefore I don't understand why on line 1 at page 1745 the statement "each advection step is divided into four etc" would hold only for the second moment scheme. Isn't' this simply a requirement from the CFL check, that one needs more steps in x-direction because the dominant wind direction

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is longitudinal ?

page 1745, sections 2.3.1. What is the original spectral resolution of the GFS model ? Couldn't higher resolution fields than 1x1 be obtained ?

page 1745, sections 2.3.1 and 2.3.2. How are the meteorological fields interpolated to the model grid ? Just bi-linear interpolation to the cell center, or averaging over the grid cell volume ? And how are they mapped to the cell interfaces ? This could be very important for the model performance!

page 1746, line 14. Where is the HPBL field actually used for, turbulent diffusion ? Is that enabled in the experiments described actually ? If so, is it done in the same way in the semi-Lagrangian versions and the flux-form versions ? Should be mentioned when discussing the applications to CO_2 etc which processes other than advection are included in the model too.

page 1747. The authors discuss the effect of mountains that is visible in the high-resolution data, but doesn't that vanish if you average over the coarser grid cells of NIES ? On coarse resolution, both the orography and the wind fields should be smoother than on high resolution. Orography then reflects the average effect of the topography on the flow.

page 1747, line 18. "... 300 mb ...." Better use "hPa" .

page 1747, lines 16-25. Confusing. The description almost suggests that the vertical grid is pure sigma below 300 hPa and pure pressure above, is this what is implemented ? The driving models probably use sigma-pressure coordinates $p=a+b*ps$, isn't NIES use this too ?

page 1747, line 29. When is the resolution doubled in the current implementation ? A plot with the grid size as a function of latitude would help here.

page 1748, line 3. Type error in "375.5 E" ?

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page 1749, line 3. What about F_k, what is contained in this ?

page 1749, lines 6-8. How does this field look like for the chosen beta, rotating over the globe from pole to pole ?

page 1750, lines 16-19. CPU and memory can be very implementation and machine specific. Better explain them in terms of formula, for example: memory = base + (data + concentrations) * number_of_cells and then explain which of these numbers is different for the various schemes.

page 1752, line 1. Is there also a figure with the observations available ? Now it is difficult to judge which scheme performs better.

page 1753, lines 9-10. "... is comparable to those of other established transport models ..." How can that be concluded from this figure ? One needs to see results for other models to decide on this.

page 1756, lines 24-25. "Because of the diffusion procedure employed in solving equations of transfer, ..." Please explain this; do you mean there is also an explicit diffusion operator implemented in the model ?

page 1757, lines 2-3. A mark for these cities in the figures would help the interpretation.

page 1757, lines 16-17. "... because of the diffusive advection algorithm or flow distortion associated with the flux-correction procedure" This statement should be qualified with an appropriate test. The second-moments scheme should be less diffusive than the VL scheme, so from the difference between these two one might judge on what the problem is here.

page 1757, lines 23-24. "... no adverse effects associated with implementation of the reduced grid". Also this statement should be qualified with an appropriate test, for example by comparison with a run with less reduction.

page 1758, line 11. " ... this version of the model still has problems with convective mix-

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ing ..." Was convective mixing implemented in the same way for the semi-Lagrangian version and for the flux-form versions ? I could not find this in the current manuscript.

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