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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the use of mass-conserving wind fields in chemistry-transport models" *by* B. Bregman et al.

Anonymous Referee #1

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General Comments

This paper addresses an important issue for chemical transport models – the conservation of mass. If chemical transport models are to be used for seasonal or longer integrations it is essential that the mass fluxes used by the advection scheme are consistent with changes in surface pressure so that atmospheric mass is conserved and spurious sources or sinks of tracer amount do not arise. The authors discuss the main causes for mass imbalance in numerical models. They go on to describe a method which calculates the mass imbalance by integrating the continuity equation over each grid cell and then using the residual to adjust the horizontal mass fluxes across the cell faces so that balance is ensured. They evaluate their method by examining tracer age from a 20 year integration and also the results for an ozone tracer with prescribed production and loss rates. These results are compared with observations from aircraft and ozone sondes. Their method produces results much closer to the observations than obtained by a previous massflux–correction method (Heimann and Keeling, 1989).

It is clear from the results, especially the comparison between MOZAIC ozone and the ozone tracer, that the "old" massflux–correction method resulted in ozone concentrations that were much too high in the winter UTLS region and also a mean age that was much too young in the extratropical stratosphere. The new method corrects these biases, although tracer age is still too young. However, the authors do not show whether the the application of massflux corrections (7) gives better results than if no correction had been applied at all (assuming that the mass fluxes used by the tracer advection scheme are still given by (2)–(6)). In other words, is it sufficient to ensure that mass fluxes are calculated in a manner consistent with the GCM providing the winds and surface pressure to the CTM, or are the mass flux corrections essential?

Before acceptance by ACP I recommend that the authors should show the case where no flux corrections are applied on Figures 1, 5 and 6 or explain why this is not necessary.

Overall the paper is well written, the equations are correct and the comparison between their ozone tracer when using the new correction method and the MOZAIC data is impressive.

Specific Comments

1. In the introduction five causes are given for mass imbalance. I recommend changing them to three points as follows:

   Point one is the most fundamental and applies to any primitive equation GCM or CTM using GCM output directly. One point that is not mentioned is that the GCMs (like the ECMWF model) do not conserve mass over multi-month integrations for this reason and a small correction is applied to maintain global average surface pressure.

Point five seems likely to result in the largest mass imbalances for offline CTMs.

Given that vertical mass flux is always diagnosed from the continuity equation in hydrostatic models it is not clear to me that the assimilation by itself would result in massflux imbalance in an analysis (point 4). However, the change in surface pressure between two consecutive analyses may be less consistent with mass balance than the change in surface pressure between two model dumps *from the same forecast* which would exacerbate point 5. This could be combined as the same point.

Points 2 and 3 are not as general and seem to refer specifically to offline CTMs driven by ECMWF model output. It would be better simply to replace them with a statement that the calculation of mass fluxes to drive an offline CTM should mimic the fluxes used by the GCM as closely as possible, cutting down on truncation error. In the case of the ECMWF model the flux form of the continuity equation in hybrid $\sigma - p$ co-ordinates should be used to obtain the fluxes, first transforming spectral divergence, vorticity and surface pressure directly into grid point U, V, D and p_s and then following Simmons and Burridge (1981) (hereafter SB81) and Segers *et al.* (2002).

2. I found the definition of $\delta_h.\Phi_h$ confusing. You could simply describe it as net flux difference across the grid cell for the fluxes in the zonal and meridional directions, just as you state that $\delta_l \Phi_w$ is the net vertical mass flux across layer l. Alternatively you could write $\delta_h = x.\nabla$ where $x = (R\cos\beta\,\Delta\lambda, R\,\Delta\beta)$ is the vector describing the horizontal extent of the grid cell in spherical co-ordinates and $\delta_l = \Delta\eta\,\partial/\partial\eta$.

3. Equation (4) describes the grid cell average of surface pressure rather than a spectral transform. It could be omitted.

4. Equations (5) and (6) are essentially the same as Eq. (3.3) in SB81. Since the ECMWF model uses the SB81 continuity equation you should cite their paper and note that your method matches that used in the ECMWF model.

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5. You should note that $m_l = A\Delta p_l/g$ is the grid cell mass, rather than the column integral in Eq. (1).

6. The three peaks in vertical mass flux variance at 50N appear to be over the Canadian Rockies, Mongolia (rather than Tibet) and the Atlantic storm track. The first two illustrate the role of orography in exciting vertical velocity but the last must be associated with weather systems. Does the Pacific stormtrack not appear because it lies further south than 50N?

7. Section 3.3, last para and Figs. 7, 8 and 9: Why not show vertical mass flux variance at 500 hPa rather than 100 hPa? As you state, when the η–levels tend to pressure levels the difference in the mass flux correction method will become small. Also vertical velocity is greatest in the mid-troposphere (especially in stormtracks).

8. p.1777, l.25: I suggest changing the last sentence to, "It takes into account the imbalance of the mass budget for every grid cell and corrects it through adjustments to the horizontal mass fluxes."

Technical Corrections

1. p.1769, l.19: between the grid CELL boundaries.

2. p.1770, l.11: over the grid CELL boundaries ...

3. p.1770, l.14: ...the pressure DIFFERENCE over a model LAYER k ...

4. p.1771, l.18: independently

5. p.1771: It would be more consistent with the earlier equations to use k as the level index rather than l.

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6. p.1772, l.25: model layers
7. p.1773, l.3: Do you mean that the age spectrum reaches a stationary solution (rather than the tracer concentration itself)?
8. p.1775, l.10 and Fig. 7: yearly → annual
9. p.1776, 1.3: significantly less than the old winds at middle and high latitudes.
10. p.1777, l.4: Omit last sentence of this paragraph as it is too speculative.
11. p.1780, l.13: implementing

References

Simmons, A. and Burridge, D. (1981). An energy and angular-momentum conserving vertical finite-difference scheme in hybrid vertical coordinates. *Mon. Weather Rev.*, ***109****, 758–766.*

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