We thank the reviewer for the positive and constructive comments on our manuscript.

The feedback from the reviewer has improved the quality of the manuscript. The reviewer's specific comments (shown in italics) are addressed below.

Comment 1.

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.

Answer 1.

We agree with the referee that the paper mainly describes how the new implementation for NIES-TM is made, and how this compares to the previous implementation. We suggest formulating the title a little bit more detailed as follows: "Mass-conserving tracer transport modelling on a reduced latitude–longitude grid with NIES-TM"

Comment 2.

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 NO2. Please comment on this.

Answer 2.

Even for long lived tracers like carbon dioxide and methane high spatial contracts are observed downstream of strong anthropogenic emitters, where model simulations have numerous problems including low resolution. Availability of high-accuracy flux data and high-resolution meteorological date (e.g., global 1 × 1 km fossil fuel CO_2 emission inventory; (Oda and Maksyutov, 2010) and high-resolution meteorological data (MERRA, Bosilovich et al., 2008)) allows calculation of tracers transport using more detailed grids and resolve many smaller-scale phenomena explicitly. In addition, high-resolution has advantages in simulation over regions with significant heterogeneity of topography, sources and sinks even in case if tracer has smooth horizontal gradients in general (Patra et al., GBC 2008).

Comment 3.

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

Answer 3.

For some datasets only postprocessed data are available to general users to use in off-line model simulations as input data. Thus we are limited in choice of the methods of flux correction.

Comment 4.

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

Answer 4.

No, this is correct. For more details please see answers for comments below.

Comment 5.

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

Answer 5.

In original mass-flux correction method (Heimann and Keeling, 1989) additional flux $\mathbf{F_c}$ is computed for the entire column using the mass in the column. In our modification we implemented this method for every vertical level l using the mass in the cell $m_s(l)$. Nevertheless the referee is right to point out errors and inaccuracies, which are corrected as follows:

"The conservation of mass requires that the vertically integrated air-mass convergence equals the surface pressure tendency. The horizontal mass fluxes, $\vec{\Phi}_h = \left(\Phi_u, \Phi_v\right)$, derived from meteorological dataset are balanced with the surface pressure tendency by adding correction fluxes, $\vec{F}_c$, which are determined as follows:

$$\vec{\delta}\left(\vec{\Phi}_h(l) + \vec{F}_c(l)\right) = -\frac{\partial m_s(l)}{\partial t}, \quad l = 1,..,N \qquad (2)$$

where $\vec{\delta}$ is a horizontal difference operator between opposite boundaries of a grid cell, l represents the vertical grid layer, and $m_s(l)$denotes the mass in the cell, defined as the product of pressure $p(l)$ on the current level and the grid cell area A divided by gravitational acceleration g ($m_s(l) = p(l)A/g$). The pressure changes are the multiplication of sigma by the surface pressure changes which are calculated from two pressure fields at different time points. Equation (2) is first solved for the single correction flux $\vec{F}_c$, which is valid for the entire column:

$$\vec{\delta}\vec{F}_c(l) = -\vec{\delta}\vec{\Phi}_h(l) - \frac{\partial m_s(l)}{\partial t}; \qquad l = 1,..,N \tag{3}$$

The correction flux is calculated by transforming Eq. (3) into a Poisson equation, which is solved with a discrete 2-D Fourier transform. This procedure is performed N times for each vertical grid layer l independently, yielding the corrected air mass flux, which is subsequently added to the vertical flux."

Comment 6.

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

Answer 6.

The semi-Lagrangian algorithm (if not implemented in flux-form way) such as by *Rash (MWR, 1994)* usually has the mass-conservation problem, which requires use of a mass fixer to conserve the global trends and tracer budgets in long-term simulations.

Comment 7.

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?

Answer 7.

Here, "function" should be replaced with "tracer concentration".

The 3rd order Van Leer scheme assumes a second order polynomial to represent the inner-cell concentration.

Comment 8.

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 is longitudinal?*

Answer 8.

Operator splitting in case of 2-nd order scheme greatly simplifies the algorithm implementation allowing to avoid explicit implementation of the cross-directional terms. The referee is right to point out benefit of operator-splitting for VL method, yet we don't use splitting advection into the 3 directions for the implemented 3rd order Van Leer scheme, at the expense of less accurate approximation of the cross terms in numerical scheme.

Comment 9.

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?

Answer 9.

Information about spectral resolution of the GFS model are added as follows: "The current GFS has a spectral triangular truncation of 382 waves (T382) in the horizontal (equivalent to nearly a 35 km Gaussian grid), and a hybrid sigma-pressure finite differencing system in the vertical with 64 layers (Moorthi et al., 2010)."

GFS model output data is interpolated to prepare a data set available with resolutions 0.5 and 1 deg. We use 1 degree as input data for NIES TM because a longer record is available. Obtaining the original output data of a meteorological model would be difficult we suppose.

Comment 10.

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!

Answer 10.

We agree, way of meteorological fields interpolation and mapping should be described (after line 16, page 1746) as follows: "The meteorological fields are interpolated to the model grid using bi-linear interpolation to the cell center. Mass fluxes are interpolated to the center of cell interfaces."

Comment 11.

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 CO2 etc which processes other than advection are included in the model too.

Answer 11.

We agree, way of HPBL field implementation should be described (after line 16, page 1746) as follows: "Turbulent diffusivity is similar to described by Maksyutov et al., (2008): above the PBL top approach used by Hack et al. (1993), below PBL top, the turbulent diffusivity is set to a constant value of 40 m^2/s. The 3-hourly planetary boundary layer height is taken from the GFS data or ECMWF Interim Reanalysis (Simmons et al., 2006/2007). The same scheme of turbulent diffusivity was implemented for semi-Lagrangian and flux-form versions."

Comment 12.

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.

Answer 12.

The increased horizontal resolution in the meteorological model grid leads to the increased amplitude of vertical velocity. It is true that this effect can be flattened due to averaging over the grid cells of NIES TM in case of test simulation with coarse resolution (2.5deg). However it can neither be vanished nor smoothed in case when the transport model resolution is close to resolution of the meteorological data, which is our target resolution for model applications.

Comment 13.

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

Answer 13.

We agree. We replaced "mb" with "hPa".

Comment 14.

*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?*

Answer 14.

We agree with the referee. Lines 16-25 should be replaced with: "A hybrid vertical grid has the properties of sigma coordinates in the lower part of the atmosphere (up to 300 hPa), where strong vertical mixing occurs, meaning that extra mixing due to the implementation of a correction method for horizontal mass flux and the use of high-resolution meteorological data has no unintended consequences and cannot disrupt the structure. In the upper part, above 300 hPa, pressure levels are used (Kalnay, 2002)."

Comment 15.

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.

Answer 15.

Reduced grid is employed to keep $\Delta x > 0.5\Delta y$, here Δx, Δy are longitude and latitude step correspondently. If this condition is violated, then Δx is doubled. Dependence of the nesting level on latitude for resolutions of 1.25 and 2.5 deg are presented in the figure A1.

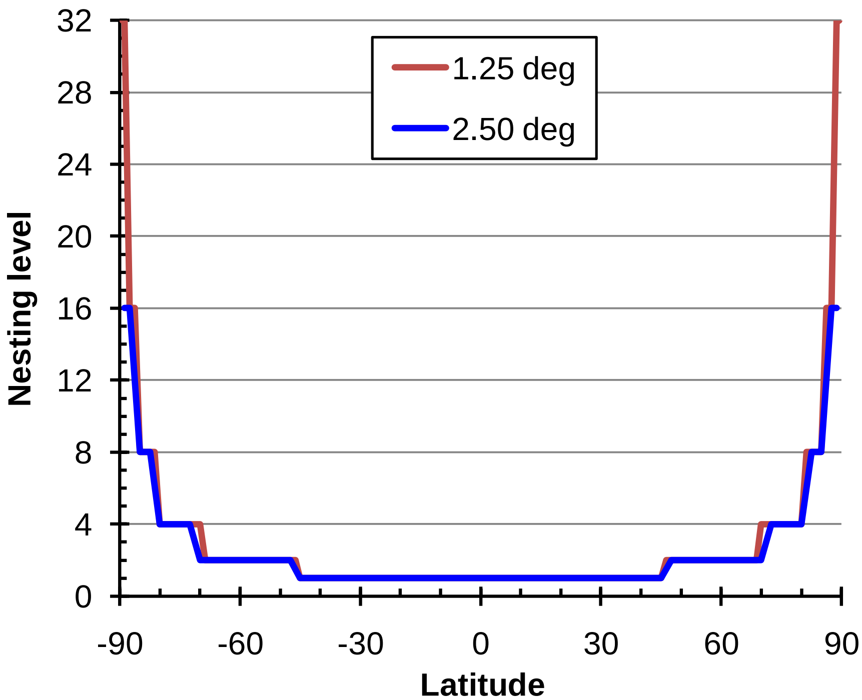

Figure.A1. Dependence of the nesting level on latitude for resolutions of 1.25 and 2.5 deg

Comment 16.

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

Answer 16.

Yes, this is type error. 375.5 E should be replaced with 357.5 E

Comment 17.

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

Answer 17.

This comment is quite reasonable. Here is misprint. In line 3 “or” should be replaced with “and”.

Comment 18.

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

Answer 18.

For the chosen angle beta two tracer fields are rotating over the globe from pole to pole. Tracer field goes along meridian 90ºE from the North to the South Pole, and goes back along meridian 90ºW.

Comment 19.

*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.*

Answer 19.

It is true that CPU and memory can be very implementation and machine specific, but we did not set a goal to study in detail the property of numerical algorithms presented in this work, since Prater’s second moments and few numerical scheme based on formulas of van Leer were thoroughly investigated by Peterson et al. (1998). We assessed CPU time and memory demand to choose numerical scheme suitable for long-time and high-resolution simulation using specific machine available for simulation. Considerable attention was paid to evaluating the impact of reduced latitude-longitude grid on the schemes performance in order to be sure that errors in tracer simulations in near the poles regions are small.

Comment 20.

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

Answer 20.

We agree with the referee. In accordance with the referees' wishes, we have now changed Paragraph 2 in Section 3.2.1 as follows:

"In Figure 2, summer and winter average concentrations calculated by the NIES TM model are compared with observation data reported in (Jacob et al. 1997). In a previous investigation (Maksyutov et al., 2008) it was pointed out that the model semi-Lagrangian version underestimated the ^{222}Rn concentration near the surface and upper levels both in the summer and winter seasons. The flux-form version has significantly improved results for upper and surface layers for the summer season. However, a comparison with results obtained by Zhang et al., (2008) (not shown here) suggests that the concentration at the top of the troposphere is underestimated, as the model does not take into account the cloud penetrative convection that provides tracer transfer up to the tropopause level."

Figure 2 should be replaced with:

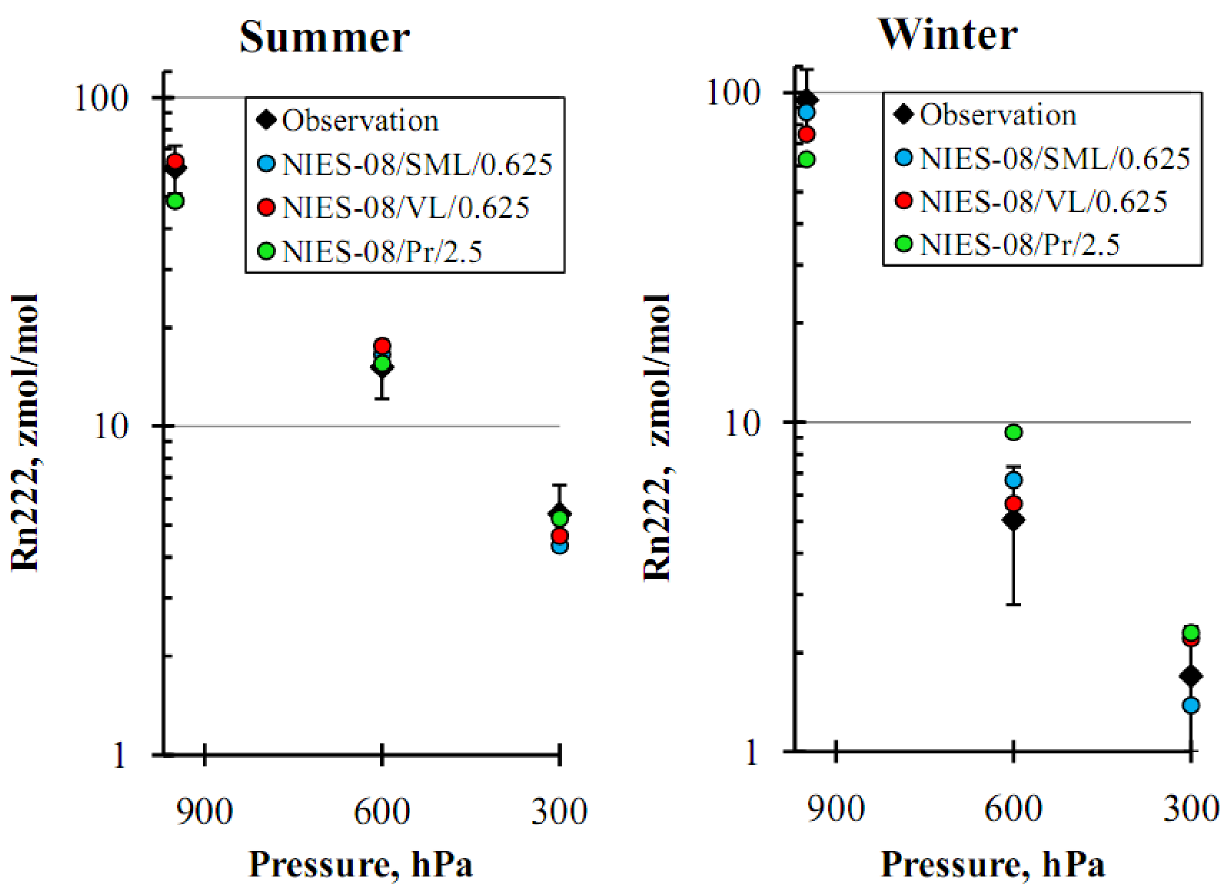

Fig. 2. Comparisons between observations and model simulations of ^{222}Rn averaged concentration over three northern midlatitude continental sites during summer and winter seasons. Results at three vertical levels (mixing layer, 600 hPa and 300 hPa) are shown. The observation data was obtained by aircraft measurements made at Cincinnati (40ºN, 84ºW), Socorro (34ºN, 107ºW), and Kirov (58ºN, 49ºE) with the corresponding standard errors.

Comment 21.

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.

Answer 21.

We agree with the referee. Lines 7-10 should be replaced with: Better agreement is found with observations implemented in the TransCom2 intercomparison (Denning et al., 1999).

Comment 22.

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 ?

Answer 22.

This question is similar to Problem 4 asked by the Referee 1.

The flux-form version tends to merge plumes from multiple sources, as seen in the area of Shanghai (Fig. 12a), because the dispersion associated with time step truncation due to CFL criteria has caused a noticeable distortion in the numerical solution (Ritchie, H.: Application of the Semi-Lagrangian Method to Global Spectral Forecast Models, Numerical Methods in Atmospheric and Oceanic Modelling, NRC Research Press, 445-467, 1997.). Moreover in case of high-resolution simulation, the side effects of the horizontal flux correction method may have a significant importance. The erroneous mass flux corrections may smooth out sharp fluctuations and distort the direction of movement of tracers and as result lead to additional smearing of the concentration. This effect is similar to action of numerical diffusion. The semi-Lagrangian numerical scheme with less dispersion due to less restricted time step (2-3 time bigger than in flux-form version) shows better performance in terms of resolving the sources clearly, as it resolves the plumes from Tokyo, Beijing, Seoul, Shanghai, Hong Kong, Taipei, and other cities (Fig. 12b). For such short-time tracer transport near emission sources, mass-conservation problem of semi-Lagrangian scheme is less important.

Comment 23.

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

Answer 23.

We agree. Figure 12 should be replaced with following one.

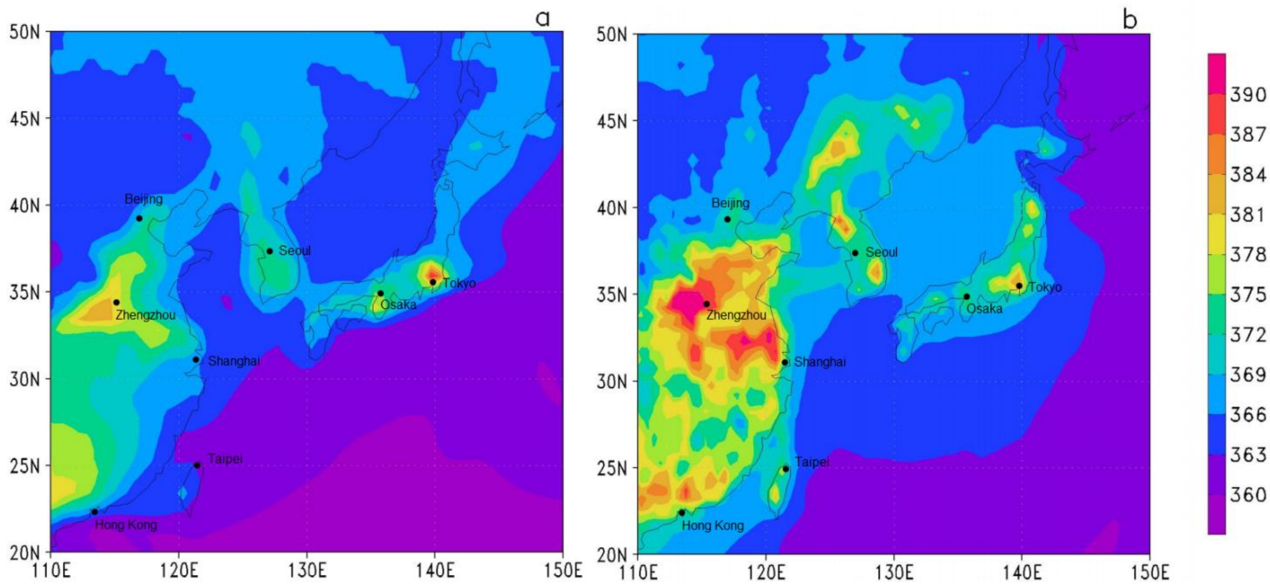

Fig. 12. Simulated surface CO_2 concentrations around Japan at 21:00 UTC on 26 March 2008: (a) NIES-05/VL/0.625, (b) NIES-05/SLM/0.625.

Comment 24.

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.

Answer 24.

Here we compare flux-form (VL scheme) and semi-Lagrangian model versions with high-resolution (0.625deg). The CPU cost of the second-moments scheme is too high for performing simulation with such high resolution and comparing with VL scheme.

Since detailed description of the problem was given in Comment 22, lines 15-17 should be replaced with: "While the flux version (NIES-08) is less able to describe sharp fluctuations in concentrations."

Comment 25.

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.

Answer 25.

We propose to use a less categorical phrase "... no significant adverse effects associated with implementation of the reduced grid". This statement has qualified with implemented solid body rotation test, when tracer field pass through the poles regions. Comparison of model versions with different resolutions (2.5 and 0.625 deg) and as results different level of reduction also revealed no significant error in tracer transport (Figures 5-9).

Comment 26.

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

Answer 26.

Should be replaced with: "… this version of the model doesn't employ convective parametrization and which results in insufficient mixing in the free troposphere".