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Atmospheric Chemistry and Physics Discussions

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Interactive comment on "From ERA-Interim to ERA5: considerable impact of ECMWF's next-generation reanalysis on Lagrangian transport simulations" *by* Lars Hoffmann et al.

Anonymous Referee #1

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Review of *ACPD* manuscript 10.5194/acp-2018-1199 by Hoffman et al.: *From ERA-Interim to ERA5: considerable impact of ECMWF's next-generation reanalysis on Lagrangian transport simulations*

This manuscript compares atmospheric Lagrangian calculations that use two different reanalysis datasets: ERA-Interim and the new ERA5.

Major comments

1. §2.2.1: Please demonstrate (at least for MPTRAC) that truncation error of the Lagrangian numerical scheme is not significant. This can be done relatively easily be examining the convergence of the solver as a function of time step size. A small figure or table would suffice.

2. §2.2.2 Most of this sub-section is not of general interest and can be deleted. Some exceptions: the URLs/DOIs for the data and the size of the ERA5 data per year (indicate how many variables that represents). How much larger is a single ERA5 variable compared to ERA-Interim? Is the disk space increase for your simulations (factor of 80) due to increases in the input (winds) or output (trajectories) or both?

3. §2.3.2: I don't understand why the horizontal difference statistics are calculated approximately(?) using Cartesian coordinates when it is straightforward to calculate the actual distance for a spherical Earth using the great circle distance formula.

   The authors do not justify their use of mean absolute differences (m.a.d.) rather than root-mean-square (r.m.s.) differences, which are standard for most statistical applications, other than that Kuo et al. used m.a.d. in their 1985 paper. Later authors have simply followed Kuo et al. There are good reasons for using the standard deviation instead of the absolute deviation, as discussed in most introductory statistics books (e.g., Bulmer, 1979), and there are situations where absolute differences have advantages. It is not clear that this one of those situations.

   The choice of statistics also limits the information about differences between the two reanalyses. The authors could present valuable information about the character of those differences by showing examples of the distributions of differences (i.e., histograms), for both horizontal and vertical dimensions. For example, are the vertical differences approximately Gaussian? Are they symmetric or skewed? Is there a bias (mean difference) as well as dispersion?

Because the horizontal differences are vectors rather than scalars, the distributions are slightly more complex, but it is easy to calculate the distance and azimuth between two particles and then create two-dimensional scatter-plots/histograms of the differences (e.g., 2-D polar plots). Again, it would be very useful to know whether there are systematic differences (biases) between the two data sets.

Similar comments apply to the statistics of the meteorological tracers.

Please define all of the symbols used in your equations.

4. §3.1: I recommend that this sub-section be discussed last in §3. The impact of 'diffusion' depends strongly on the selection of the diffusion coefficients, which is not directly related to the differences between ERA5 and ERA-I. This is discussed further in my summary recommendation.

5. §3.2: During the 10-day trajectories the particles are more likely to encounter chaotic regions in the flow where the evolution is sensitive to the initial conditions. Is that what you mean by 'separated by different airflows'?

   Figure 7a (for example): If I understand this figure correctly, the horizontal differences between ERA5 and ERA-I are several hundred kilometers, while the differences due to 'diffusion' are only $\sim$10 km. That is the opposite of the results shown in Figure 4, where there is wide dispersion of particles by diffusion but the 'non-diffusive' trajectories are very similar. Does that mean that good agreement between the base trajectories in Figure 4 is rare? If so, why is this used as a representative sample?

   How is it that the global range (min to max) is smaller than the ranges of the individual latitude zones? Shouldn't the global range be the min and max of the all of the ranges in the latitude zones?

This figure basically provides semi-quantitative comparisons between results in different altitude layers and latitude zones. It is currently arranged to allow easiest comparisons between different altitude layers. Is that the highest priority? The figure might be easier to understand if the results were grouped by layer altitude rather than by latitude zone.

As discussed above, it is important to know whether these differences are systematic or random.

6. §3.3 and §3.4: How much of the difference between the 'tracer' variables is due to differences in the *trajectories* and how much is due to differences in the *analyses*? If you use an ERA5 trajectory to evaluate a variable in the ERA5 analysis *and* in the ERA-I analyses, how large are the differences?

7. §3.5: Users may choose to simply downsample the ERA5 data in space and time rather than filtering the data first. How does downsampling the data compare to low-pass filtering and then downsampling? Are the errors comparable, or does the filtering significantly improve the results?

Minor comments

1. The authors should cite the seminal papers on Lagrangian atmospheric methods, not just their own recent papers. For example, Djuric, *J Meteorology*, 1961 and the papers cited therein lay out many of the same issues discussed here. More recent examples include Hsu (*JAS*, 1980) and Kida (*J Met Soc Japan*, 1983).

2. Table 1: Please provide the dimensions of the horizontal transform grids (e.g., 512 $\times$256 for ERA-I) in addition to the spatial resolution. If there are standard

grids on which the analyses are made available (e.g., regularly-spaced grids that differ from the models' spectral transform grids), please include that information also.

3. Paragraph beginning on page 2 line 2: Lagrangian models may or may not include parameterizations for 'diffusion'. Whether they do or not, I think it is important to maintain the distinction between *molecular diffusion* (e.g., random walks of molecules due to collisions) and the *stirring* of fluid by unresolved scales of motion, which is often represented as a diffusive process by using an eddy diffusivity coefficient. Because molecular diffusion acts on very small spatial scales, in the atmosphere it is almost always insignificant compared to unresolved scales of motion, whether turbulent or not.

   Lagrangian calculations do not necessarily involve a grid (regular or irregular). Particles can be initialized randomly within the domain, for example.

   Lagrangian calculations are not *explicitly* affected by numerical diffusion, but the wind fields used for Lagrangian calculations are almost always produced by a model or data assimilation system. The model winds *are* influenced by both the explicit and numerical diffusion in the model.

   The real power of Lagrangian methods is that, given a *continuous* wind field, which is typically provided by a combination of velocities at discrete grid points and a space-time interpolation scheme, a Lagrangian solver can compute the trajectory of a particle with essentially arbitrary precision. The interpolated wind field is an approximation of the real wind field, of course, and is typically designed to be smooth at the grid scale (i.e., at least piece-wise continuous). Lagrangian methods can avoid the diffusion of passive tracers that is always present to some degree in an Eulerian model and provide an effectively higher spatial resolution.

4. Equation 9: Strictly speaking these are not errors because the quantities in question are not perfectly conserved. Some degree of non-conservation is to be ex-

pected. You are really evaluating the non-conservation, which may be real, due to diabatic heating or dissipation, or a result of various sources of error.

Instead of 'dynamical tracer' you might say 'quasi-conserved variable'.

5. Figure 4: Is the horizontal dispersion in Figure 4 largely due to a combination of vertical displacement by the sub-gridscale wind parameterization and vertical shear of the resolved horizontal wind?

6. Page 10 line 22: If the horizontal displacement differences are several thousand kilometers, differences in the planetary vorticity f may also contribute to the differences in PV. I think it should be straightforward to sort out the contributions of ζ, f, and stability.

7. Page 16 line 1: The stratosphere is not 'dynamically less disturbed than the troposphere', but the spectrum of motion is redder in both space and time, so the actual flow is likely to be better represented on the model grid than in the troposphere.

Recommendation

This will be a very useful paper for both research and applications of Lagrangian methods in the atmosphere. I recommend publication after some revisions. My greatest concern is discussed in major comment #3. I think it would be of great benefit to see some actual distributions of differences between the two reanalyses, rather than only descriptive statistics. Also, the authors should explain why the absolute deviations are more useful for this application than standard deviations.

My second major concern is about the comparisons between calculations with and without 'diffusion'. A serious problem throughout the paper is that you don't really know the magnitude of the dispersion by sub-gridscale components of the flow. You only know the impact of *parameterized* sub-gridscale winds, which may be very different from reality. You *can* argue that, at least with current estimates of the magnitude of the sub-gridscale dispersion, its impact is smaller than the differences resulting from differences at resolved scales.

In general I feel the paper attempts to do to much and presents too many results with too much detail (especially figures 7, 9, 10, 12, and 13). The results are comprehensive rather than illustrative, which makes it quite difficult for the reader to synthesize the results and extract the essential points. Perhaps the complete results could go into appendices or supplementary materials (as tables?), with only the most significant results discussed in the main text.