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Discussions

Interactive comment on "An online trajectory module (version 1.0) for the non-hydrostatic numerical weather prediction model COSMO" *by* A. K. Miltenberger et al.

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1 General remarks

The discussion paper introduces a module for the calculation of trajectories on-line during the integration of a non-hydrostatic operational numerical weather prediction model. This model, the Swiss version of COSMO, is presently used at grid-spacing down to 2.2 km (and preparations for reducing it further, which I think is not mentioned). The authors correctly point out that the fact that spatial resolution has been increased much more than the temporal resolution at which output is available operationally as

input for off-line trajectory models calls for an on-line implementation.

Therefore, the creation of this module for COSMO and the documentation, along with the tests made and evaluated, in the present discussion paper is certainly welcome. However, the description of the module should be made more detailed and better structured. It would also be useful to better explain (and maybe explore) certain decisions and their consequences.

Another reviewer has questioned the value of mean-wind trajectories at the spatial scale covered by the model. Also the authors allude to this issue in their discussion of the behaviour near ground. My opinion is that a trajectory model (this is the term that I am using for a mean-wind-based model, as opposed to a Lagrangian particle model which would simulate also the effects of subgrid-scale motions) does have its place also at high resolution (even at LES scale) as it allows to investigate atmospheric motion patterns represented explicitly in the model in a way that cannot be achieved e.g. by a Eulerian tracer carried (unless a number of tracer species is used which is the same as the number of trajectories, but even then diffusive properties of the numerical integration would deteriorate the result). However, I think the authors could invest some additional work to include a survey of possible application types, the set-ups related to them, and their merits and shortcomings. This would be a significant benefit for users beyond their own group and enhance the value of the paper.

GMD guidelines call for supplementary material such as codes and user manuals. The authors should give some consideration to this issue and explain at least why they don't think that they can or want to attach such material. It would also be important to state the conditions for using their module, whether it will be included in COSMO in general, etc.

2 Specific remarks – major issues

1. The list of possible applications of Lagrangian, trajectory-based analyses is obviously only meant to give some illustrations, but in the light of my remarks above on applicability of this specific high-resolution trajectory model, they may want to give more consideration specifically to high-resolution applications.

2. Repeatedly, we find the wording *"from a reanalysis data set or a numerical weather prediction model"*. This is a bit strange as reanalyses are carried out with NWP models. I see three categories: (operational) forecasts, operational analyses, and reanalyses.

3. Page 1226, error source 3: Wind field errors are not only due to prediction errors. Also the initial condition contains errors. One may also wish to either mention here or in a separate item the representativity error between wind field represented in the model and present in Nature.

4. Page 1226, error source 4: I don't understand this point, and also not how this would depend on the forecasting system.

5. Module description. I have the following suggestions for improving:

   - First, summarise the algorithms used in LAGRANTO, and clearly point out where additional details (if not included) can be found (is the Wernli and Davies paper fully comprehensive? If not, add other source, or add corresponding document as supplementary material).
   - Then, give an overview where the COSMO module deviates from the off-line version.
   - Finally, give the details on the COSMO-specific features.

6. Section 2.2: It would be very helpful to have a better idea of the numerical costs of the trajectories before this discussion, but it comes only in Section 3. It is necessary to give at least some more information here. It is not clear immediately why the simple trajectory integration should contribute significantly to the computational resources compared to the integration of a comprehensive NWP model.

7. Missing technical information: I presume that all is implemented in Fortran90/95/2003/. . . , it would be useful to explicitly say this. Which implementation of the MPI library is used? Also say at the beginning and in the abstract that it is an MPI (distributed-memory machine) implementation.

8. Section 2.3: I would call this "selection of trajectory starting points" instead of "initialisation". The fact that back trajectories are not (easily) possible for on-line calculation needs to explained already in the introduction, and then be discussed in the Section on applications that I propose. I disagree with the statement that this limitation "*slightly* complicates" studies (page 1241 bottom) – it is a limitation!

9. Time step: In section 3.2 time step durations are stated. Are they specific to the case study? If not, move them up! How is the time step determined in COSMO? Are there different time steps for different processes? How do they relate to the trajectory time step? All this should be in Section 2.

10. Section 3.3, discussion of performance. I don't know whether it is justified to discuss the performance with a single case study. Please explain if and why. If not, more case studies need to be done (no need to discuss them in detail, just for quantifying performance). The relative runtime (or runtime increase) should be evaluated also as a function of the number of trajectories. This will be important for future users.

11. Figure 4: The result that transport differences are very similar for 2.2 vs 7 km and

2.2 vs. 14 km is very interesting. It deserves deeper investigation.

12. Discussion of error quantification at the end of Section 3.4: I think one needs to define first which errors one wishes to quantify. Investigation of differences between configurations has its justification!

13. The ground intersection problem should not be introduced as the second-last paragraph of the whole paper! It belongs into the module description.

14. I am surprised that consideration of turbulence is offered as possible way to overcome the ground intersection problem. I don't see why this should be effective. On the other hand, this would totally change the character of the model, and transform it from a trajectory model to a dispersion model.This would be a big step, in concept but also in terms of algorithm and code. Introducing turbulent diffusion in only a part of the boundary layer (if this is meant) would be quite unphysical.

3 Specific remarks – minor issues

1. Page 1227: I would not say that the Petterson algorithm requires specifically a short time step.

2. The word föhn can be written in lowercase, like bora or mistral. Most English-language papers would spell foehn. Note that mistral is spelt once with upper- and once with lowercase. (And bora features interestingly are not discussed in the case study although present.)

3. Page 1229: I am wondering whether there are no more recent applications of LAGRANTO than 2005.

4. Same page, "shut on/off", write "switch on/off".

5. Starting a new paragraph with line 19 on page 1234 would improve the readability (number of trajectories is important).

6. Section 3.2, last line: The "jump flag" needs to be explained (by the way, the wording is misleading, it is not so much a flag=indicator but rather an algorithmic feature.)

7. Section 3.3, list of variables which is written out probably is not specific to case study, so it does not belong to Section 3.

8. Page 1236, bottom: The symbol for the number should be N instead of n to be consistent with the equation on p. 1237. You are using three levels of round brackets – you could use curly, square, round, or a root symbol instead of the outermost brackets.

9. Figure 5: One would expect $\Delta z = z(t_2) - z(t_1)$ but it was defined the other way round.

10. Some Figures are quite small, I hope they will appear larger in the final version. Especially the legend inset of Fig. 4 is much too small at print-scale (printer-friendly version).

Interactive comment on Geosci. Model Dev. Discuss., 6, 1223, 2013.