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Geoscientific Model Development Discussions

Interactive comment on "3-D visualization of ensemble weather forecasts – Part 2: Forecasting warm conveyor belt situations for aircraft-based field campaigns" *by* M. Rautenhaus et al.

Anonymous Referee #3

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The main topic of the article is the identification of warm conveyor belts from ensemble weather forecasts for aircraft-based research campaigns in the framework of the Met.3D software system. The authors deal with both the scientific and technical aspects of the problem.

The overall presentation of the article is clear and the text is logically built up. Each problem emerging during the discussion is clearly explained, the possible solutions are thoroughly analysed and the reasoning on why a given method was chosen always seems logical and well supported. The usage of references is adequate and the authors give a proper credit to related works.

In general it is a well-written article revealing a significant amount of work both from the scientific and software engineering point of view. In particular, the handling of the low probabilities is a creditable achievement. Therefore I would definitively propose the paper for publication.

However, some parts of the manuscript requires further clarification and there are several other (mostly minor) comments that the authors should take into consideration.

Optimal usage of data

The authors mention several times that the data volume of the ECMWF ENS was huge and caused performance issues. The experiments with setup S4 (p. 2174) clearly showed that it is enough to use only levels up to 100 hPa to detect WCBs, since they play out in the troposphere. However, in setups S1, S2 and S3 (p. 2173-2174) all the 62 model levels are used, although the topmost 10 model levels (so 15% of all the data) are typically located above 100 hPa. It would be interesting to know why the authors did not skip these set of levels.

ENS related comments

1. The term "control forecast" is used at several places without explaining actually what it is. It might be worth adding a short description about ensemble prediction in general to clarify its concepts (at least in relation with ECMWF ENS).

2. The number of members in the ECMWF ENS forecast is not used consistently: e.g. p. 2175, line 16 mentions "50", but p. 2178, line 2 says "51".

3. FC-B and FB-D on page 2164 asks "how reliable are the weather predictions" and p. 2181 line 13 also mentions "reliability", but ensemble forecasts in general do not estimate reliability, instead they can measure uncertainty. This is an important conceptual difference.

4. P. 2167, line 8 is using the term "spherical truncation of T213" but it is more precisely a triangular truncation of a spherical harmonic spectral representation (or spectral trun-

cation in short).

5. Similarly p. 2168, line 16. mentions "spherical resolution of T639" but it is actually a spectral resolution.

6. P.2168, line: T799L91 used here without explanation.

7. The article mentions multiple times (e.g. p. 2169, line 3) that the ECMWF model has terrain-following model levels. Well, actually it is a hybrid vertical co-ordinate system: it is terrain following at the bottom-most level and isobaric at the topmost level, in between there is a transition. This is mentioned later in the text but I think this should be clarified for the users at the very beginning.

8. P. 2173, line 17 mentions that the ENS model levels depend on the surface pressure field. The authors correctly point out that this varies between the ensemble members and deal with the consequences of this fact. However, they fail to mention that it also varies between the time steps and it has implications on the data pre-processing.

9. It would be interesting to know what horizontal and vertical interpolation techniques were used to prepare the input fields from the ECMWF ENS. Also, since it is an operational environment, the computational cost of the pre-processing steps is worth mentioning, especially if it is comparable to the cost of the trajectory computations.

10. The article uses the term "initialisation time" for the model run time. I suggest that the authors should use "run time" instead.

Figure related comments

1. Mixed use of "Figure" and "Fig." throughout the article for figure references. I suggest that "Figure" should be used everywhere.

2. The caption of Figure 5b mentions ensemble member 12, but the text mentions "control forecast" (see p.2175 line: 21).

3. Figure 5b shows "binary volume rendering" but the caption does not mention it.

4. The details in Figure 5a (red isosurfaces inside transparent white isosurfaces) can only be seen at 3x magnification in the pdf. I wonder how it would work in a printed version. Also, it is somewhat hard to distinguish between the white isosurface and the greyish map background.

5. The colour code of Figure 9d should be explained in the caption.

6. The details in Figure 12 and Figure 14 are hard to see without magnification and it is somewhat hard to distinguish between the white isosurface and the greyish map background.

7. Figure 19 features the same problems as Figure 5a (see point 4 above).

Minor remarks:

1. p. 2180: abbreviation DLR is first mentioned here but not explanation is given

2. p 2167, line 21: a.s.l. stands for "above sea level" but no explanation is given

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Interactive comment on Geosci. Model Dev. Discuss., 8, 2161, 2015.