

Author reply to Anonymous Referee #3

400 The format of this reply is as follows. The referee comments are cited in grey italic font. Our replies to the individual comments are given in regular, black font. We have attached a latexdiff version of the revised manuscript, displaying the changes we have made, at the end of our reply to Anonymous Referee #1. All line numbers given in the reply below refer to this latexdiff version.

405 *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.*

410 *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.*

415 *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.

420 We would like to thank Referee #3 very much for his/her positive and constructive comments and appreciation of our work. In the following, we reply to the referee's comments.

Optimal usage of data

425 *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)*
430 *are typically located above 100 hPa. It would be interesting to know why the authors did not skip these set of levels.*

435 Thank you for pointing out this issue. In fact, we did start trajectories only on the lower 52 levels of the forecast grid (up to approximately 100 hPa). However, we did not mention this in the manuscript. We have added the corresponding information in the revised manuscript at ll. 542-546 and l. 549.

ENS related comments

440 *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).*

445 True, while we put the corresponding information into Part 1, we missed to also put them into Part 2. We have added a note about the ENS consisting of 50 perturbed members and an unperturbed control run to ll. 139-141 of the revised manuscript (note: latexdiff does not highlight changes made to citations, however, the given lines have been changed). Also, we have revised the part that references to the description of the data in Part 1 (ll. 329ff. in the revised manuscript) to better point out to the reader where more detailed information about the data can be obtained.

450 *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".*

Thank you for pointing out this typo. All numbers should have read "51", we have changed those that read "50" to "51" (e.g. l. 609).

455 *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.*

460 This is true, thank you for pointing out the inaccurate wording. We have changed the word in forecast

question B to “uncertain”. In question D, we mean how “meaningful” the computed probabilities are in the sense of how they should be interpreted. This corresponds to the proposed region contribution method and the possibility to change dp/dt interactively (as done in the case study in lines 920ff.). We have hence chosen the word “meaningful” to replace “reliable” for question D. We have fixed the wording in both Part 1 and Part 2.

465 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 truncation in short).

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

This was also pointed by Referee #2. We have changed the wording to “spectral” in both Part 1 and Part 2.

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

We have added a short explanation in brackets (l. 298).

480 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.

485 This is true. We have changed the wording from “terrain-following” to “hybrid sigma-pressure” throughout the manuscript, referencing the corresponding paper by Untch and Hortal (2004) with the first occurrence at l. 338). Also, we have noted in l. 526 that the uppermost levels do not depend on surface pressure anymore.

490 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.

495 We have rewritten ll. 529-530 in the revised manuscript to reflect the dependence on time and ensemble member. However, as our method computes the probabilities for each timestep independently, we do not see any further implications on data pre-processing.

500 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.

In our setting, the forecast data fields are retrieved in interpolated form from the ECMWF MARS archive. Interpolation is all done by the ECMWF systems during the operational data retrieval. Hence, on our servers at DLR, the data are already received in interpolated form. This is the time that we refer to as “data are available from ECMWF” in criterion “a” in the beginning of Sect. 3. Upon reception of the data, we are ready to visualize the data with Met.3D and also to start the computation of the trajectories. Hence, while there is some processing of the data at ECMWF, there is no further pre-processing step on our side. To make this point more clear in the manuscript, we have added information at ll. 332ff. (concerning the interpolation done by MARS) and at ll. 504-506 (concerning the “availability”).

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

515 “Initialisation time” is used frequently in our environment, hence we would like to stick with this term. However, we have added the terms “base time” and “run time” in a bracket at l. 796 of the revised manuscript to clarify these alternative notations for “initialisation time”.

Figure related comments

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

The mixed use is due to the guide lines of GMD, asking to use the abbreviated “Fig.” within sentences and

"Figure" at the beginning.

525

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

Thank you, we have corrected to text to "member 12" (l. 617),

530

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

We have revised the figure caption and also the corresponding description in Sect. 3.2 (l. 618) to clarify the meaning of the isosurface.

535

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.

540

This issue was also raised by Referee #1. We have changed the colour of the isosurface to a light purple, so that it can be better distinguished from the background. Also, it is true that the figures have been quite small in the "Discussions" layout of GMDD. Prior to the original submission, we had checked that all figures are readable in the final GMD layout, however, had missed that they appear smaller in GMDD. In the revised version, we have scaled the figures to the size we intend them to be in the final layout. Concerning Figs. 5 and 6, we have rearranged the layout of these two figures to make the images larger. In the revised version of the manuscript, the details are now well visible when printed.

545

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

550

We have added an explanation to 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.

555

Please see the answer to point 4 above.

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

560

Please see the answer to point 4 above.

Minor remarks:

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

565

We have added the unabbreviated name (l. 800).

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

570

We have added "above sea level" (l. 274).