

Interactive
Comment

Interactive comment on “Estimating trajectory uncertainties due to flow dependent errors in the atmospheric analysis” by A. Engström and L. Magnusson

Anonymous Referee #2

Received and published: 24 August 2009

Engstroem and Magnusson present an interesting study on the estimation of trajectory uncertainty. All in all, the study is well presented and there is a good balance between figures and text. I am convinced that the article will be of interest to most researches working with trajectories (and atmospheric transport in general) and therefore would appreciate publication in ACP. Nevertheless, there are some points which should be considered and further discussed.

Major points: _____

1) At the moment the whole discussion is based upon one single winter month. Indeed, there are "only" two 5-day case studies and one monthly climatological analysis. But it

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



is well known that the weather systems vary considerably from season to season. For instance: in summer we expect a substantial shift of the storm track towards the north; the intensity of the jet streams and jet streaks is weaker; the subtropical high pressure systems have a stronger influence on the midlatitudes. In summary, this limitation should be alleviated by considering at least one additional (summer) month or it should be critically addressed in the text.

2) In section 2.2. it is written "For the selected case studies we only use the method perturbed both in horizontal and vertical but when considering the statistical behaviour of the methods we show results from all methods" I am a little confused and assume that in the case studies only the horizontal displacements are considered? Or, to which "all methods" are you referring to?

3) An interesting questions arises if the two methods are compared. Let's formulate it in the context of the EA method. There the wind fields are modified according to the analysis error norm. As far as I understand the modifications affect both the horizontal and the vertical winds. In this respect the EA method might be much more "invasive" than the IS method where no changes are made to the winds. But naturally then arises the question which part of the trajectory uncertainty in the EA method can be attributed to the modified horizontal winds and which part must be attributed to the modified vertical wind. Would it make sense to perform one additional EA experiment where only the horizontal modifications are kept, but the original vertical wind is taken instead of the modified one? Possibly with this approach the partitioning into horizontal and vertical wind perturbations becomes amenable.

4) In the same line as point 3): in Figure 1 you show the standard deviation and the RMS error for the U and the V wind components. I think it is no surprise that the two wind components behave very similiary. On the other hand, you are considering 3d kinematic trajectories. Hence, the vertical wind component W might be very decisive. Would it be reasonable to show the corresponding plot (or a variant thereof) also for the vertical wind component? At least, in the present discussion (throughout the whole

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

text) the important role of the vertical wind is not discussed at all. I would appreciate very much if this discussion of W could included and refined in the text.

5) In section 3.1.1 a case study for the North Atlantic is presented. The subsection starts with some general statements about the weather systems in this sector and how they influence the trajectory uncertainty. This is certainly true and is a valuable introduction. But then it would be very nice to see some very specific statements about the synoptic-scale weather situation for the case study. Was there a low pressure systems passing over the receptor site? Was this flow situation characterised by a NAO+ or NAO- like flow pattern? Was the polar jet stream straight or curved? I would appreciate some case-specific background information about the meteorological situation. The same does also apply for the tropical case study (3.1.2). Note also, you mention in section 3.1.1 "The region shown in Fig.2 is characterised by a relatively high standard deviation in wind speed between the ensemble memembers". Would it be possible to meteorologically explain where this high standard deviation is coming from?

6) "Figure 3 shows the 850 hPa level wind direction and wind speed standard deviation" is stated in section 3.1.2. Two points in this respect: 1) Actually you are showing the full wind arrows (not only the direction); and 2) How do you justify that the arrows and deviation is shown always at 850 hPa? Do the trajectories always stay around this level, or is there simply not a very strong dependence of these fields on the level chosen? If the trajectory height significantly deviates from 850 hPa and if the wind arrows and standard deviation strongly varies with height, a different visualisation might be appropriate: For instance, you could show the fields at the corresponding height of the reference (control) trajectory? Or, a verical summation of the standard deviation could be possible? Please comment on this aspect.

7) Section 3.2 presents a statistical analysis of the trajectory spread. There it is written that the main behavior of the trajectory spread is of interest. True! On the other hand, I see a slight break between section 3.2 and the previous study. Indeed, in section 3.1 the focus was very much on distinct weather systems and how they influence the tra-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

jectory uncertainty. There the small number of trajectories was ok. But for section 3.2 the limited number of trajectories might be a problem. Here, distinct weather systems play no prominent role, but the focus is on the general characterisation of the deviation growth. I like this kind of discussion, but wonder whether the shortness of section 3.2 can really satisfy the reader. It gives a glimpse into a realm which deserves a much closer look. Possible questions arise: How does the Lyapunov exponent vary with season? With height? Is it different in the stratosphere compared to the troposphere? In summary: Section 3.2 could be a completely independent article. I see a possible replacement by a thorough discussion of vertical versus horizontal wind effects. Possibly, it will be ok to keep section 3.2, but then I would appreciate a stronger link between section 3.1 (weather systems) and section 3.1 (general characterisation).

Minor points: —————

- 1) Page 15750, line 5: "this is error correlation is...". Remove "is".
- 2) Page 15750, line 18: "method where compared...". Correct: "were compared"
- 3) Page 15750, line 26-28": Please simplify sentence structure.
- 4) Page 15751, line 12-13: "for long trajectory calculations... for shorter". Please give in brackets numbers for "short" and "long".
- 5) Page 15751, line 17: "resemble the the expected". Remove one "the".
- 6) Page 15752, line 5: "...where the displacement of the trajectory is determined". What do you mean with the displacement of a trajectory? Do you mean "the displacement (path) of an air parcel"?
- 7) Page 15752, line 8: "and 40 vertical levels". Which ECMWF data set are you using? As far as I remember, the operational archive had 60 model levels in 2005?
- 8) Page 15752, line 21-23: "Since no forward integration.. this is the rationale...in the present study". I do not understand this statement. What is "this" referring to?

Interactive
Comment

9) Page 15752, line 25ff: "and yields perturbations orthonormal to the inverse analysis error norm". This is difficult to understand if one is not familiar with the technique. Why not re-arrange from line 24 on in the following way: The Ensemble Transform method is a further....to sample fast growing error structures. The perturbations are calculated...Wei et al 2008.

10) Page 15753, line 10-13: Please simplify the sentence structure.

11) Figure 2 and 3: Make the figures larger, possibly by omitting the latitude/longitude labels on the plots. Furthermore, it would be possible to zoom in a little. Note also that you are showing the wind arrows, not only the wind direction.

12) Page 15756, line 4-6: Please simplify the sentence structure.

13) Page 15756, line 26: Here you are referring to possible "other methods". Please be more specific which methods you have in mind. Or is it just a general statement about future methods which still have to be developed?

14) Figure 5: The legend is too small.

15) Page 15758, line 7: There is one minor points which should be clarified: "The deviation is calculated as the difference between one perturbed trajectory and the unperturbed trajectory". How do you define the difference between two trajectories? Is it the sum of spherical distances along the trajectories? Or do you only consider the end-point distance between the trajectories.

16) Page 15760, line 7: "the two methods appears certain atmospheric situations". Correct: "under certain..."

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 15747, 2009.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)