Atmos. Chem. Phys. Discuss., 9, C6254–C6270, 2009
www.atmos-chem-phys-discuss.net/9/C6254/2009/

Atmospheric Chemistry and Physics Discussions

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

A. Engström and L. Magnusson

anderse@misu.su.se

Received and published: 28 October 2009

In the following section we give our responses to each of the reviewers' comments or questions in a format that lists their comments in order and each followed by our corresponding answer. Regarding the reviewers minor comments. We have chosen to answer some of them in text and if so they are shown in the minor comments section of each reviewer response. If a minor comment is not discussed below we have followed the suggestion of the reviewer and made the suggested changes. This applies to minor comments regarding language and sentence structure or typographical errors.

Both reviewers were concerned by the small data set included in the general characterization of the trajectory behavior as well as the possible differences between different seasons and altitudes. In the revised version of the manuscript we have therefore included additional trajectories for one summer month period as well as studying trajectories released at an higher altitude. Furthermore, the reviewers raised the interesting question if errors related to turbulence and convective transport could be equally useful in providing a rapid error growth. Although we have here focused on the error in the analysis due to large scale motion, this is certainly an interesting question which we have tried to address in our response.

We would like to take the opportunity to express our gratitude to the reviewers for their careful reading of the manuscript. The questions raised by the reviewers have significantly improved the discussion in the revised manuscript.

Reviewer #1

1) Some justification is needed why trajectories are only started at 850hPa. This choice strongly limits the conclusions from this study to cases where trajectories near the ground are considered. The two methods could behave very differently at different vertical levels, for instance at the mid-latitude tropopause or in the polar stratosphere.

We agree with the reviewer and have therefore extended the study to include trajectories released at the 300 hPa level for December 2005. The result obtained from this study agree with the results found at 850 hPa and the main conclusions drawn in the previous version of the manuscript can be applied to the 300 hPa level. We have chosen to include the results from this in Table 1 in the new version of the manuscript. In Table 1 the α and β term from Eq. 1 in the manuscript is presented. As shown Table 1, the initial error growth for the 300 hPa trajectories using the EA method is actually higher.

Regarding trajectory calculations in the stratosphere. The stratosphere is less chaotic than the troposphere which decreases the error growth. However, in the stratosphere the analysis error will instead be dominated by the error introduced by the lack of observations. The Ensemble Transform method is not aimed at capturing observational errors or lack of observations (as we partly argue is the case for instance in the tropics), and therefore we would expect little difference between the two methods. However, using the IS approach to estimate trajectory uncertainty in the stratosphere is, for the same reasons, not a reliable estimate of the true analysis uncertainty. We suggest other possible ensemble methods in section 4 of the revised manuscript. A discussion regarding this has also been added to section 4 of the manuscript.

2) The method description needs some clarification. It remains unclear how the individual perturbed analyses are obtained and how they differ from one another.

Each individual analysis is obtained by adding perturbations to the original analysis. The perturbations created with the Ensemble Transform method are constructed so that the perturbed analysis sample the flow dependent analysis error. This method is described in Wei et al. (2008). Each perturbed analysis will be a realization of this analysis error. The differences between the perturbations is for instance small changes in the magnitude and direction of the wind and differences in the temperature field (i.e. geopotential). We have added text to section 2.1 to clarify this. We have

also added an additional reference which further describes the Ensemble Transform method.

3) The hemispheric differences in Fig. 1 are not fully discussed. How much of the differences in the northern hemisphere (NH) are due to the limitation of the study to NH winter? Also, the implications of the tropical vs. mid-latitude characteristics are not fully taken into account in the discussion of the two case studies.

As we now have extended the study to include a summer month period we can see that the differences in the northern hemisphere is visible during the summer period as well. The general characteristics of the analysis error is however the same during both periods. We can also see that the initial perturbations (Fig. 1, black lines) are somewhat higher in the winter hemisphere of each period which is also reflected in the estimate of β in the statistics section of the paper (section 3.2). We now discuss this further in section 2.1 in the revised version of the manuscript.

4) It would be a very insightful extension of this study to compare the results with findings from calculations with ensemble members, or trajectories that include parameterizations for turbulence or convection. Furthermore, the two methods could be easily combined to increase the spread in yet another set of calculations. Some of these points are natural questions that arise on reading the manuscript and should be addressed in the discussion.

Adding chaotic processes to the trajectory calculations would most probably introduce an additional uncertainty. However, the main purpose of this study was to examine the impact from errors in the large scale dynamics. The comparison between the IS method and the EA method with an additional chaotic component is not straightforward since the chaotic component samples errors on a different scale. However, we agree with the reviewer that this could also be a viable option to test how reliable a trajectory is, by including or excluding chaotic processes. This was however outside the scope of the present study but we certainly see the interest for future studies. We have also commented on this in the conclusions (section 4) and in section 3.1.2 of the revised manuscript.

Answer to minor comments by reviewer #1

L. 16: The reference Magnusson 2009b was not available to the reviewer. Which properties where compared, and how was the expected analysis error derived?

The Magnusson 2009b reference is still in the review process and we have therefore removed it from the revised version of the manuscript. The relevant parts of the discussion related to this paper is now covered in section 2.1. However, with this question we wonder if the reviewer was referring to statement in section 1 that reads *In Magnusson et al. (2009) the properties of the singular vector method and the Ensemble Transform method were compared. It was shown that the Ensemble Transform perturbations are closer to the expected properties of the analysis error compared to singular vectors.*? This reference is available but we have made an error in the reference list. The paper appears in Tellus 61A and not 60A as previously stated. This has been corrected.

L. 6: How were the 20 perturbed analyses obtained? Did you sample the maximum perturbation randomly? Is each of the 20 sets dependent on or independent of the previous perturbed time step?

This is highlighted in section 2.1 and described in Wei et al. (2008). This question is also partly answered above in the response to reviewer question #2. The perturbations are dependent on the previous analysis and the previous ensemble of perturbed analyses since they are flow dependent. In for instance regions with baroclinic instability the perturbations will be larger due to the uncertainty related to the prediction of baroclinicity.

L. 9: 'For each analysis one trajectory...': how is this trajectory related to the 15 trajectories calculated each day?

The sentence has been rewritten. One trajectory is calculated for each analysis. This is in done for 15 different locations around the globe. For each location 20 EA trajectories are calculated and in total 15×20 EA trajectories are calculated each day. This has been clarified in the text.

L. 13: Was the displacement chosen randomly? Note that this initialization introduces a latitudinal bias (1° longitude near the pole $<>$ 1° lon at equator!).

The displacement is not chosen randomly but is evenly distributed within the 1x1° box. The initialization does introduce a bias. We have however accounted for this bias in an extra set of simulations and found no significant difference. The results presented in the statistics sections show that the size of the initial spatial perturbations in the IS method is not that important. The error growth rate start from zero and can only be described by α, while the EA method introduces the β term in Eq. 1. Essentially one can only move the IS method curves up or down in Fig. 3 with the initial displacement, that is changing $D(t = 0)$. However, one would then overestimate the error for short trajectories. We have commented on the latitudinal bias in section 2.2.

As seen in Fig. 1 the error in the extra-tropics is exaggerated - hence the EA error growth might be extreme. This would make the actual differences between the EA and IS method appear relatively small. Have you examined other case studies in the extra-tropics?

The error is slightly overestimated in the northern extra-tropics but we do not find it exaggerated or extreme. The error is in the same order of magnitude and we don't believe this overestimate impacts the findings within this study. The conclusions also apply for the tropics, where the error is actually underestimated, and for the southern extra-tropics, where the error is not overestimated. We have examined many case studies for different latitudes and they show the same behavior as is described in the statistic section of the paper.

L. 20: Where in which panel of Fig. 3 is that region? To me, the differences between the different kinds of trajectories appear relatively small. The sentence in L. 22 is not clear, what do you mean by appearance?

We have clarified the discussion in regarding this comment in section 3.1.2 as well as changing the sentence. By appearance we meant that the trajectories entered different flow regimes. This is now changed to be more understandable.

L. 27: Which other methods do you mean? How strongly is this case study impacted by the underestimation of analysis error seen in Fig. 1? Trajectory calculations in this region are probably not very trustworthy in general due to the relevance of (deep) convective transport assuming that the ECMWF trajectory model does not parameterize turbulence and convective transport in

some way.

We have clarified in the text that we refer to methods that could better capture the lack of observations in the tropics and/or the error related to observations which the Ensemble Transform method does not. Furthermore, the Ensemble Transform method is mainly aimed at capturing the dynamically growing part of the analysis error. However, for trajectory calculations also the non-growing error is of interest. This case study is most probably impacted by the analysis error underestimate seen in Fig. 1 as we also mention in the text. We could therefore expect a higher uncertainty if the analysis error in the tropics was better represented. As the reviewer speculates, chaotic components would also increase the error. Chaotic components has however not been included in this study since the aim was to study the impact from errors in the large scale dynamical motion.

L. 22: 'The growth rate is much higher...': does this finding imply that parameterized small-scale turbulence during the trajectory calculation could be equally useful in providing rapid initial dispersion growth?

Adding a chaotic component related to turbulence and convection could be useful to provide a rapid error growth. However, chaotic processes act on a different scale than the analysis error studied here. Both these components are of course important if one wishes to asses the overall uncertainty in a calculated trajectory path. We can however not conclude if a chaotic process would give the same error growth. A discussion about this has been added to the conclusions.

L. 1: 'aimed to sample special flow situations': not clear, special in which way?

We have rephrased the sentence. We mean situations with a high analysis uncertainty to do dynamically growing error structures.

L. 15: 'The conclusion from...': to what extend is this conclusion limited by only considering trajectories starting in the lower troposphere?

Every atmospheric analysis will contain errors. The errors will be of different sort depending on location and altitude. We conclude that "by perturbing the analysis consistent with the analysis uncertainties, both regarding perturbation amplitude and correlation length, the uncertainties in trajectory calculations can be more consistently estimated". This conclusions is made studying the troposphere but we think it applies for the rest of the atmosphere as well. It might however be that the effect of the analysis error is small in other parts of the atmosphere. However, if one could estimate the true analysis uncertainty one would also be able to conclude whether it has a small or large impact on the trajectory uncertainty. This is what we state in the manuscript. We discuss this more in section 4 of the revised manuscript.

Reviewer #2

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 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.

We agree with the reviewer that the analysis in the previous version of the manuscript does perhaps from suffer a too small data set. Therefore, following the suggestion of the reviewer we have chosen to include one additional summer month in the study. The results from this is presented in the newly added Table 1 in the new version of the manuscript. Additionally, the results shown in Figure 4 and 5 are now expressed as the mean of both the winter and summer period. It is found that that the conclusions drawn in the previous version of the manuscript do apply to the summer period. However, as speculated by the reviewer we can see a seasonal change in the β term in Eq. 1. The winter hemisphere is characterized by a somewhat stronger initial error growth. In the case of the case-studies their main purpose was to act as an example of how a high analysis uncertainty can impact on the calculated trajectories.

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 behaviur 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?

In the case studies the IS method perturbed in both horizontal and vertical is used. But when considering the statiscal behavior we also show the results from the method perturbed only in the horizontal plane. This is now more clearly stated in section 2.2.

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 partioning into horizontal and vertical wind per turbations becomes amenable.

The initial perturbations are only applied to the horizontal wind field. However, due to continuity, changes must also be made to the vertical wind field corresponding to the changes in the horizontal wind (following the continuity equation). Although the reviewer raises an interesting point we think that such a study is unphysical since convergence/divergence is always related to vertical wind, and vice versa. This question is probably better addressed comparing three-dimensional trajectories with trajectories using perhaps an isentropic trajectory model. Furthermore, in some ways changes are also made to the wind with the IS method since the displacement in the horizontal and vertical and the spatial interpolation of the trajectory model will give each trajectory different initial wind speeds. However, one could use analysis perturbations that are free of divergence/convergence (although this can not be achieved with the Ensemble Transform method). Divergence free perturbations would however underestimate the true analysis error and one would therefore expect less impact on the trajectory error.

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 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.

The reviewer raises a valid point. However, since vertical wind is a diagnostic variable in the ECMWF IFS, sampling uncertainties in the horizontal wind is equivalent as sampling uncertainties in the vertical wind. The vertical wind component is a product of divergence and convergence. We agree that vertical wind is very decisive and that, for instance, turbulent or convective transport through parameterization would probably increase the trajectory uncertainty. However, as we have studied trajectory uncertainties due to large scale motion, a separation of horizontal wind and vertical wind can not be made.

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 memebers". Would it be possible to meteorologically explain where this high standard deviation is coming from?

Following the reviewers suggestion we have added additional descriptions of the weather situation in the regions of the trajectories as well as some additions to the discussion in accordance to these additions. In the case of the high standard deviation it originates in the uncertainty related to the position of two low pressure systems. We have also added this to section 3.1.1.

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.

1) As the reviewer points out it is the full wind arrows that are shown. We have corrected the text in the new version of the manuscript.

2) As this quantity is not changing dramatically with height we chose to show it at the 850 hPa level since the trajectories remain close to this level for the first 1-2 days.

While trajectory height does deviate from the 850 hPa level (generally they end up between 1000 and 600 hPa after five days), the difference in standard deviation is not as significant. The general structure of the analysis error is found to be represented well by the 850 hPa level. Therefore the figure still fulfills its illustrative purpose.

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 trajectory 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).

We have made additions to the analysis as suggested by both reviewers to section 3.2. We now show results from the a summer month and also for trajectories released at a higher altitude. These additions to section 3.2 discuss both the seasonal dependence of β and the altitude differences. Additions have also been added to the conclusions of the manuscript. The discussion in section 3.2 does show the general impact from the analysis error on trajectory calculations. We certainly agree that this discussion could be further extended in another study where perhaps other perturbations methods are studied. However, section 3.2 show that no special meteorological situation is alone responsible for the differences between the EA and IS method. The results discussed in section 3.2 does also relate to section 2.1 and present a method to evaluate the effect of the analysis error on trajectories. This together with the additions made to section 3.2 we believe merits that the discussion remains in the paper.

Answer to minor comments by reviewer #2

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?

This is correct. The operational analysis has 60 model levels. The analysis was however interpolated to 40 model levels and the ensemble transform perturbations were created using 40 model levels. This should not impact the relative difference between the two methods compared. We have however clarified this in the revised version of the manuscript.

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?

The formulation was unclear in the submitted version of the manuscript. "This" refers to the previous statement in the text. Since no integration of the dynamical model takes place, the model can not develop the perturbations created with for instance the

singular vector method. The Ensemble Transform method does not however rely on the integration of the dynamical model to evolve the perturbations but instead create perturbations consistent with the analysis error. To clarify this the paragraph has been rewritten.

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?

We have clarified that we are referring to methods that are aimed at, for instance, capture effects from observational errors or lack of observations.

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

The deviation is calculated as the spherical distance at each time step between one perturbed trajectory (either using the EA method or the IS method) and the unperturbed (control) trajectory. We have added this explanation to section 3.2.

References

Magnusson, L., Nycander, J., and Källen, E.: Flow-dependent versus flow-independent initial perturbations for ensemble forecasting, Tellus, 61A, 194–209, 2009.

Wei, M., Toth, Z., Wobus, R., and Zhu, Y.: Initial Perturbations Based on the Ensemble Transform (ET) Technique in the NCEP Global Ensemble Forecast Systems, Tellus, 60A, 62–79, 2008.

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