

Interactive comment on “Assimilation of IASI partial tropospheric columns with an Ensemble Kalman Filter over Europe” by A. Coman et al.

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Authors response to Anonymous referee #2

The authors thank the referee for all his remarks which allowed increasing the paper's clarity and interest.

Overview This paper explores the potential of IASI column ozone observations to constrain a regional chemistry transport model using an Ensemble Kalman Filter, focusing on a special case in July 2007. The paper demonstrates clearly that the assimilation is able to improve the agreement between the model and two types of independent observations, but also that the improved modeled ozone fields do not necessarily yield improved forecasts. This paper is interesting and presents a useful analysis of the

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impact of IASI data. However, the language describing the assimilation approach is at times ambiguous and not in line with the jargon of the data assimilation community. Considerable time is spent on the rather obvious result that assimilation improves the fit of a model to the assimilated observations; the paper could be made far more concise by focusing instead on how much information can be gleaned from the IASI observations, rather than showing that the assimilation “works”. In the following review, all page numbers will be referred to by the last two digits only (e.g. P45 for page 26945).

We agree that significant time is spent showing the fact that assimilation “works”. But with our (medium-scale) system this was not obvious from the start. It is our belief that the results shown are significant and interesting. Nevertheless, we tried to limit the extent of this section.

General Comments 1. An Ensemble Square Root Filter (EnSRF) is used to perform the assimilation, but the method is referred to as an Ensemble Kalman Filter (EnKF) throughout the study. Thus it comes as a major surprise to the reader when the EnKF is described up to halfway through Section 4, and suddenly the switch is made to EnSRF. Even though the En-SRF is considered a variation of the standard EnKF, the present-day data assimilation literature considers these to be two different algorithms, with the moniker “EnKF” implying the classical, “perturbed observation” Ensemble filter described Evensen (1997) and Burgers et al. (1998). Therefore I strongly recommend that the method be referred to as EnSRF throughout the paper, including the abstract.

The criticism is justified. Throughout this study, the method used is EnSRF and this fact was clearly stated in P56: L16-17 (ACPD published version): “This is the reason for selecting square root formulation in our study (we use the same formulas and notations as in Evensen, 2004).” Throughout the paper, the text has been changed as suggested by the reviewer to clarify this point.

2. Four figures (Figs. 4-7) are dedicated to showing that the assimilation of ozone data into the model results in a better fit of the model to the assimilated observations.

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However, an improved fit to the assimilated observations is actually an expected result of any working assimilation scheme and doesn't really tell us anything about either the quality of the assimilation scheme or the knowledge gained by assimilating observations (in other words, if we were only interested in fitting the model to observations, we might as well throw away the model and only look at observations). I recommend greatly reducing the discussion of observational fit, and instead devoting more space to discussing the impacts of IASI ozone assimilation on the modelled ozone, e.g. by examining the reduction in ensemble spread as the assimilation progresses.

We understand the referee's comment, but still we think that most of this discussion is needed and justified. Nevertheless, to follow referee's recommendation and shorten this section, Figure 7 was removed. Other figures (i.e Fig 4, Fig 5, Fig 6) are kept: Figure 4 was presented in order to exemplify the period selected for our sensitivity tests (on the 17 July 2007, high ozone concentrations were recorded on the European domain), moreover it shows how cloud cover can affect observation availability. As for the two other figures 5 and 6, they show how assimilation works but the first one as monthly (temporal) average on the spatial dimension (2-dimensional maps) and the other as a temporal evolution of a spatial mean bias and rmse (over the available pixels). This was not considered redundant.

Specific Comments

1. P45, L28: The distinction that sequential algorithms are used for realtime analyses and retrospective algorithms for reanalyses is not entirely valid, since, for example, 3D-Var is considered sequential but is used in the ERA-40 reanalysis (Uppala et al., 2005).

We agree that this point should have been treated more carefully. In fact the examples were taken directly from Fig. 2, chapter 1 of Bouttier and Courtier, 1999 and they are roughly classified according to their applicability to real-time problems. But compromises between these approaches are possible. As presented in the reference given

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by the anonymous referee #2, the 3D-Var was used in the ERA-40 reanalysis. This comment was taken into account. A sentence was added to correct the example cited before as following: "Examples of the two types of algorithms identified can be found in the above cited work (in Fig.2, chapter1): Optimal Interpolation, 3D-Var, Extended Kalman Filter for the first category and 4D-Var, fixed-lag Kalman Smoother and Kalman Smoother for the second one, roughly classified according to their applicability to real-time problems. Compromises between these approaches are possible (example the use of the 3D-Var in the ERA-40 reanalysis following Uppala et al., 2005)."

2. P47, L11: The introduction seems to makes a distinction between (a) assimilation of total column ozone, (b) assimilation of ozone on the free-troposphere, and (c, line 11) assimilation of ozone for purposes of air quality. However, at least to this reader, it's not obvious that there even is a distinction between these areas. Can you explain, for example, why the free-troposphere assimilation studies cited in the first paragraph on P47 would not fall in the category of air quality?

The term air quality generally refers to ground level pollutant concentrations. Data assimilation of surface ozone most obviously falls in the category of air quality related studies. Assimilation of free tropospheric ozone could also have an impact on ground level ozone fields, but only indirectly through downward transport of free tropospheric air. This is why we distinguished it from the first case of surface level ozone assimilation. Total column ozone assimilation mainly improves stratospheric ozone, because most of ozone in the vertical column is located in the stratosphere.

3. P47, line 26: The use of "poorly documented" here tends to imply that the multitude of studies that have just been cited about tropospheric ozone concentration and its evolution, are poorly written. Surely the authors are simply making the point that, despite the studies above, key information is still missing. This paragraph could be written more clearly to show exactly what exactly is missing in our understanding after all the cited studies are considered (presumably it's the fact that observations are still very sparse?).

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The aim of this sentence was to say that data covering was sparse. This became clear from the next sentence in the manuscript “To complement the existing, but sparse, observations from sondes and commercial aircraft, ...”, so the ambiguous sentence was skipped.

4. P50, L25: “Degrees of freedom” is not synonymous with “an independent piece of information”, so this sentence should be rephrased, e.g. by writing in the parentheses something like, “i.e. increasing the degrees of freedom, or DOF, of the signal”. Moreover the connection between DOF of a retrieved column and the amount of independent information is unclear, mainly because no information is given about how the DOF shown in Fig. 2 are computed. P52, L4 states that the 0-6km columns “do not contain an independent piece of information”. – What does this mean?

We agree that “degrees of freedom” is not synonymous with “independent pieces of information”, nevertheless the two notions are related. In Rodgers (2000), “the degrees of freedom for signal describe the number of useful independent pieces of information in the retrieved quantity” or the number of useful independent quantities that can be determined from a set of measurements. It can be evaluated by taking the trace of the averaging kernel matrix (sum of the diagonal values). As the maximum DOF of our data reaches 0.7 (as shown in Fig.2), we stated that, in our case, we do not dispose of “one” independent piece of information.

5. P54, L9: I object to the description of the EnKF as a “3-D” assimilation method. Even though it is sequential (not retrospective), an important trait of the EnKF is that it evolves background error information forward in time, which is exactly what is meant when referring to 4-D methods.

We have changed in the sentence “3-D” by “4-D”.

6. P56, L1: Equation (1) describes the update at every timestep, not just the initial one. Thus,

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refers to the ensemble at any, not just the initial, time. Thus the word initial should be deleted here.

Yes, indeed, this has been corrected.

7. P56, L3: This sentence just describes what happens in eq. (1), which is not unique to the EnKF (the same thing is done in OI, variations on the EnKF, and variations on the linear KF) and thus not really “the key” to this method. Rather, the distinguishing trait of the EnKF is the use of an ensemble to get the covariance matrices, i.e. eq. (4).

Indeed, the distinguishing trait of the EnKF is the use of an ensemble to get the covariance matrices (forecast and analysis). A sentence was added to highlight this fact as suggested by the referee.

8. P56, L19: “...ten times larger” – than what? The comparison here could be to any number of things (numerical weather prediction, climate reanalyses), so this should be made more clear. Moreover, it is not obvious that more observations require larger ensembles than are typically used - please explain this, and why 100 ensemble member makes the problem ill-conditioned. Also, this paragraph currently reads as though covariance localisation is necessary in a wide range of applications because the AQ problem has more observations – which of course doesn’t make any sense. In fact, covariance localisation is used because, in almost all problems, the state to be estimated is much larger than the ensemble, which causes spurious correlations due to sampling error. This problem is separate from the issue of more observations, and this should be made clear.

Our comparison (“ten times larger”) was with the ensemble size mentioned just before in the same sentence. However, to avoid any misunderstanding, the sentence was rephrased. More generally the text was revised and modified in order to better explain and clarify the localisation issue (see the end of the set-up section pages 13-14). It was not our intent to state that “more observations require larger ensembles than are typically used”; nevertheless in Evensen (2004) it was shown that this fact (number

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of observations superior to the ensemble size) has some impact on the rank issue (problem tackled also in Kepert, 2004). However, there are two reasons for using localisation: first the spurious covariances and second the insufficient ensemble rank. As the analysis in the EnKF/ EnSRF is computed in a space spanned by the ensemble members, this space is rather small compared to the total dimension of the model state. These issues are explained in a hopefully clearer way in the added text at the end of the set-up section, pages 13-14.

9. P57, L6, and elsewhere: It has already been explained that the En-SRF is being used in this study, so it is inappropriate (and potentially misleading) to introduce it as “Ensemble Kalman Filter” here (see general comment above).

The replacement was made everywhere in the text.

10. P57, L15: Actually, more basic ways to initialize an ensemble also exist (e.g. one could select initial conditions from a long model simulation, or just perturb a set of initial conditions) - that is worth mentioning.

We have stated that other methods/ways to initialize an ensemble exist (ourselves, we use one of them: perturb a set of initial conditions)

11. P58, L20: I don't see a reason to split the discussion on localisation between this paragraph and the last paragraph of Section 4.1 It would be more straightforward to describe localisation entirely in one place.

This is agreed. Instead of splitting the discussion, we attempted to improve the readability and flow and the whole discussion is placed in the setup section. (see comment 8 also)

12. P59, eq. 6: It seems as though eq. (6) would be more appropriately placed in the earlier discussion of the averaging kernel (Section 2). This would make that section clearer, and then A would already be defined by the time at eqn. (5) is introduced.

We moved the equation 6 to section 2 as suggested.

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13. P60, L18: Modest reductions in the RMSE as ensemble size / patch size are increased does not reflect robustness of a DA system, but rather the saturation of errors, i.e. the fact that all other errors (model error and observation error) are so large that increasing the ensemble size doesn't improve the accuracy.

We stated that the modest improvements in the RMSE reflect the saturation and not the robustness of the system.

14. P60, L22: I don't see a reduction of RMSE by increasing path size from 20 to 30, in fact in three cases (ens. sizes 10, 20, and 80), larger patch size slightly increases RMSE.

Indeed, we found slight variations (second digit) in the RMSE when increasing from 20 to 30 the number of the pixels in the local patch, but our final choice was 30. Here the modified text in the manuscript: “As for the local patch size, its increase from 10 to 20 pixels slightly improved the quality of our analysis and then the increase from 20 to 30 pixels leads to a small loss of precision (values are very close, differences at second digit). Finally, the largest value tested (30) was used in the simulations presented here.”

15. P61, L1-2: How is the slope of the IASI-Model scatterplot different from the correlation coefficient between them? If the difference is only numerical, there is no benefit to showing both numbers.

The slope was removed.

16. Fig. 4: It is difficult from comparing these three panels, to see where the modeled ozone fields are corrected the most strongly. A more informative plot would be to show the ensemble mean assimilation increment (analysis - forecast); this would show where the observations had the most impact, especially when compared to the difference fields between the forecast and observations, and analysis and observations.

By presenting fig. 4, our intention was only to exemplify with an interesting case (high ozone concentrations) how the assimilation system corrects the ozone field. We plotted

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the three panels to compare the forecast with the observations and analysis, which is a classical comparison usually made in the assimilation field. Here after in fig. 1, we plotted the difference fields (IASI-mean of the ensemble forecast, left panel) versus (IASI-mean of analysis ensemble, right panel) in DU for the 17th July 2007, but again it was not our intention to quantitatively analyse more in detail the correction made by the filter. Nevertheless, we can see that the amplitude of the increments obviously decrease on a large part of the domain, mainly over the continental part.

17. P61, L3: It is pointed out that the situation depicted in Fig. 4 could have a larger than usual model bias. How should this affect our interpretation of Fig. 4?

The question here is perfectly justified. In fact, the figure 4 presented in the paper is part of the sensitivity tests performed only for the 3 days mentioned in the manuscript (17, 18 and 19 July 2007), whereas our comment intended to highlight the fact that in the one month assimilation experiment, on the 17th July 2007 we could have a larger model bias because no assimilation was performed for the previous day (16th July 2007) due to missing data. The incriminating sentence was removed.

18. Fig. 5: It is not really surprising that the analysis O3 fields are similar to the observations, since this is exactly what the assimilation is designed to do. Again, it would be more interesting to see an analysis forecast increment field. It would also be informative to show how the ensemble spread changes after assimilation, since this would show us where the IASI observations had the most impact.

We think that it would be a little more difficult to interpret the mean (over the month) of the increment field (as the differences between the simulated ozone and the measured ozone can be positive or negative during the simulation month). Showing Fig.5, we wanted to highlight also the general tendencies for the corrections performed in the assimilation experiment (the CHIMERE model overestimates the ozone field – consistent with the results obtained by Zyryanov et al. (2011) using an ensemble of models, and this fact is corrected by the assimilation system). The absolute values of the ozone

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fields like presented in Fig. 5 are also interesting to show, generally for a quantitative evaluation of the pollution level in Europe for a summer month. We plot also the increment fields (as before for 17th July 2007) as suggested by the referee (below Fig. 2) and the impact of assimilation is clear for a large part of the domain (ocean and continental) mostly over the northern half but also over Spain and Mediterranean basin. Therefore, even we didn't show how the ensemble spread changes after assimilation, we can see where the IASI columns had the most impact.

19. P61, L1-2: I am not convinced that the correlation between the model and observations before and after assimilation is an interesting result here. Showing that correlation increases merely shows that assimilation is able to make some sort of fit to observations (which it has to if the assimilation is working), but it tells us nothing about whether this fit was achieved for the right reasons, or whether the model was actually improved by assimilation.

We removed the statistical indices as correlation and slope.

20. Fig. 7: I am not convinced that Fig. 7 shows anything that hasn't already been shown at this point, i.e. that the analysis is closer to the observations than the prior, which in itself is not even very surprising.

The figure 7 was removed.

21. P63, L25-26: It is unclear what is being compared here – does “innovations” refer to the difference between the observations and the forecast following assimilation?

The term innovation is used to design the difference between observation and forecast (following assimilation) and it was defined in the manuscript (P62, L7-8)

22. P67, first paragraph: Here it would be good to clarify that the bias is actually the bias with respect to Mozaic, not some sort of general bias. This is especially important since the bias with respect to soundings (Fig. 12) is not really reduced with the assimilation.

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This has been done.

23. P67, L8-25: Assimilation improves the agreement between the model and MOZAIC as well as ground-based stations, but not for the ozonesondes. However, I don't think this is very troubling since both the reference run and the EnKF analysis are mostly within the uncertainty level of the ozonesonde composite (Fig. 12). However, some discussion of why the EnKF might fail to improve the fit to the sondes would be helpful. Here it would also be helpful if Figs. 11 and 12 had the [same] horizontal axes; this would make it easier to compare the error bars for each of the independent measurement types.

The horizontal axes between Figures 11 and 12 were homogenized. In the discussion already included in the manuscript, we state that the ozone sonde data-set is a composite of measurements at five different sites. For some of the individual sites, only a few sondes are launched (see table 2). This makes the interpretation of the results difficult. The horizontal error bars correspond to the standard deviation (variability) so this value cannot give an indication of the bias significance.

Technical Corrections

1. P45, L2: "AQ" is introduced here as an abbreviation for air quality, but not really used until P56. It would be better to be more consistent.

We removed the abbreviation introduced at page 45 and we added it at page 56.

2. P45, L9: If "Regional Chemical Transport Model" refers to a specific model, it should be cited, and if it refers generally to regional CTMs, it should be written in lower case. The abbreviation RTCM is used on P53, L2, but has not been formally defined anywhere.

Change has been made to use lower case at page 45 as suggested by the referee. As for the abbreviation it was introduced in the title of the Section 3.

3. P46, L20: "began to be set-up" sounds awkward. Perhaps, "was begun" will suffice.

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Done.

4. P46, L27: It's unclear whether the reference to Dethof and Holm (2002) refers to the ECMWF ozone assimilation, or just the SCIAMACHY instrument. Both this reference and the one to Eskes et al.(2003) should be more strategically placed to make clear what these references refer to.

The references were replaced as suggested to make the text clearer.

5. P48, L20: The wording in the first sentence here is awkward, since no assimilation framework has been introduced at this point in the paper. Maybe a more simple way to say this is: "The assimilation of IASI data has already been studied."

Done.

6. P54, L25: More simply: In the EnKF, the ensemble mean and covariance are presumed to fully describe the PDF of both the prior and assimilated fields, which are assumed to be Gaussian.

Done.

7. P55, L3: up-date ! update.

Done.

8. P57, L11-12: This sentence would be better placed after the one stating that the filter is used for state (not parameter) estimation.

The sentence stating that the filter is used for state estimation was moved at the end of the paragraph.

9. P58, L9: regrouped ! grouped.

Done

10. P60, L203: Delete "which" and "were" for a more concise sentence.

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Done.

11. P61, L15: Since the simulation only covers one month, “monthly averaged mean” can just be “mean”.

Done.

12. Fig. 8: It would be far more straightforward to the reader to refer to the zones as “NorthEast”, “NorthWest”, etc., rather than the letters.

Done.

13. P65, L22-23: There is no need to put these phrases in quotes.

The quotes were removed.

14. P65, L25: Figure 10 should be introduced before individual curves in it are referenced.

Done.

15. P65, L25: “Simulated ozone levels in the reference simulation are larger in the...”

Done.

16. P68, L7: The sentence beginning with “Especially...” is not complete.

The sentence was modified.

17. P69, L10: spatial differences.

Done.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 26943, 2011.

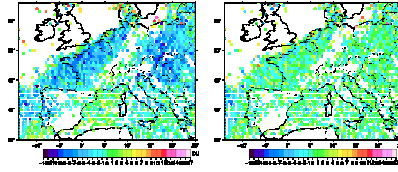


Fig. 1. Differences between IASI columns (Dobson Units) and the mean of the forecast ensemble (left panel), and IASI columns and the mean of analysis ensemble (right panel) for the July 17th 2007 (at 9 a.m.).

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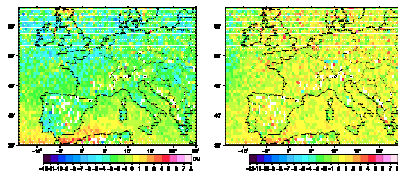


Fig. 2. Differences between IASI columns (DU) and the mean of the forecast ensemble (left panel), and IASI columns and the mean of analysis ensemble (right panel) averaged over the whole month of July 2007

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