**Atmospheric
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Interactive comment on "High resolution assimilation of IASI ozone data with a global CTM" *by* B. Pajot et al.

Anonymous Referee #2

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This paper examines the assimilation of ozone data in the form of IASI total columns and MLS profiles. Analyses are made at two different resolutions (T42 and T170) with the hope that the higher resolution would agree better with independent observations. In fact, the higher resolution experiments do no better and in some aspects are worse. The main result of scientific interest is the impact of background error correlation length scales. Because these have been set smaller in one experiment, information from MLS observations is not well-spread horizontally. As a result, the vertical profile of ozone is less well constrained. Unfortunately, though the study presents a lot of detail it does not go into enough scientific depth in any area. It would benefit from a much greater focus on the interesting science, and a greater depth of investigation in those areas. As it stands there is not enough scientific content to recommend publication.

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Separately, there is a need for much better presentation: the paper is confusing and vague, particularly in many important aspects like the different spectral and spatial representations, grid resolutions, and the superobbing. The results would be easier to follow and more interesting to read if condensed and restructured. The division of the results into three sections (first model (4.1), then analysis (4.2) and then high resolution analysis (5)) and then each of those into different subsections (e.g. ERA-interim, sondes, OMI) makes for much repetition. As mentioned before, some of these results are presented in too much detail given what they contribute to the conclusions of the paper.

Main scientific points

A) The effect of the background error correlation length scale is scientifically interesting and rightly given prominence in the paper's abstract. However, there is no justification for the choices made and no attempt at further scientific investigation in this area. For such a crucial part of the paper, this is completely insufficient. All that is said can be found on p. 29369: "horizontal length scales are set to 275km for the assimilation <of data superobbed at T42>. As shown in Bouttier (1994), the horizontal length-scales decrease in areas with dense observations, so we choose to set the horizontal length scales to 77km for <assimilation of data superobbed at T170>". And in the conclusion "the horizontal correlation length scale of the background error has to be adapted to represent the small-scale structures present in the observations". The Bouttier reference is not justification: background errors are supposed to describe the characteristics of the first guess, not the resolution of the observations, so this is scientifically incoherent. Given that the authors already apply some of the Desroziers et al. (2005) diagnostics, they could perhaps have determined the appropriate background error length scales in a similar manner to Bormann et al. (2010). Ultimately, the paper demonstrates that 77km is an inappropriate choice. But what would be a better choice? Would better quality ozone analyses be possible?

B) The combination of limb profiles and total columns in ozone data assimilation is an

interesting subject that has seen much previous research (e.g. Struthers et al. 2002 and many following papers). The authors need to acknowledge this and to present their research in the context of all that previous work, particularly in section 3.2.

C) Given that so much of the paper concerns model resolutions and grids, the presentation is far too vague in the way these different concepts are described. There is an ongoing confusion between spectral and spatial representations, with for example "T170", which describes a spectral representation, being used to refer to a Gaussian spatial grid. This means that both the "T170" version of MOCAGE (on p. 29361) and the "T255" ERA interim analyses (on p. 29364) are described as having a 0.7 degree spatial resolution. This is certainly confusing and quite possibly wrong. The paper needs to clearly introduce these different kinds of grids and representations at the beginning of the paper, including references. There are dozens of confusing statements in the text, and indeed dozens of different grids and resolutions are mentioned: e.g. in the work of the other groups mentioned in the introduction; previous MOCAGE work on 2 by 2 degrees; ECMWF operations and ERA interim (which use both spectral and spatial grids); the two new MOCAGE grids described in the paper (whatever these are, spectral or spatial - I am still confused); the grids on which the Valentina analyses are made (spectral?); the grids used for the superobs; the original observation sampling patterns. This makes for a confusing read, and may reflect a real scientific confusion too.

D) ERA-interim ozone analyses are presented as a reference for validation. If that is so, is there any point in the current work?

Detailed scientific points

1) 29358.8 (see point C) a "gaussian grid" cannot have a triangular truncation and cannot be described by the term T42 or T170 - those are spectral representations.

2) 29358.16-17 and 29384.9-10 "this modification results from a better representation of the vertical velocity". There is no scientific backing for this statement in the paper.

Unless some can be added, these statements should be deleted.

3) 29358.18-19 "in a general way comparisons with independent data show large reductions in the ozone standard deviations when the resolution is increased" and 29384.11-17 "increase of the CTM ... resolution ... showed a mainly positive but small influence on the ozone analysis": there does not appear to be any basis for these statements in the paper, e.g. in Figs. 9 and 10. These statements should be removed.

4) 29360.13-14 "... superobservations close to the datasets resolution...". This is confusing. What's the point of a superobservation that's almost the same resolution as the dataset itself? Here there are just one or two raw observations per superobservation. Why not just use the raw observation?

5) 29361.2 (see point C again) "so-called T42 low resolution" - we need references and a precise definition of what this means.

6) 29364.1 "we had to construct super-observations". Is that really true? In the present study, MLS data could have been assimilated at its natural resolution. Wouldn't that be easier? In the case of only one observation per superob (which would be quite common), the only difference between a superob and an ordinary observation would be that a superob would represent a model grid point whereas an ordinary observation would require an interpolation operator. The second approach is the normal one, as it can deal better with sharp gradients. As mentioned in technical point 2, aspects relating to superobservations need to be presented better.

7) 29364.19-21 "N128 reduced Gaussian grid" (see point C) - be more consistent in describing the different grids.

8) 29365.6-8 (point C again) ERA interim data are interpolated from the N128 grid to the T170 grid. But it's already indicated that both these grids have the same 0.7 degree resolution with 512 by 256 points (on p. 29361 and 29364). Why interpolate if they are the same grid?

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9) 29366.14 Only 11 ozonesonde stations are used, with the result that it is very hard to assign statistical significance to the differences shown in Fig. 9 later. There are plenty more ozonesondes out there - why not use them too? The study referred to in this section, Geer et al. (2006), used data from 42 different ozonesonde stations.

10) 29368.2-9 "ECMWF operational analyses suffer from a lack of dissipation...too much energy remains in the small scales": What is meant by "dissipation"? Against which reference can these statements be justified? The production of "unrealistically noisy ozone fields" does not justify this. Perhaps there is a problem in the time-resolution, or in the way the interpolation or spectral truncation is done? Using anal-ysed winds to drive CTMs is widely acknowledged to be tricky; many papers have been published on the subject.

11) 29369.10-13 "we apply diagnostics like those of Desroziers et al. (2005). A bit more detail is needed here. Also, scientifically, isn't it strange that Massart et al. (2011) determine $s\_o = 10$ (s_b is not specified) but you get $s\_o = 2$ and $s\_b = 10$? Why are the correction factors so different?

12) 29371.2 and 12. The standard deviation of departures is assigned as the IASI measurement error on line 2. In data assimilation, the departure covariance matrix is $R + HBH^T$, so this gives a maximum bound to the observation error R. So why then is an additional inflation factor, $s\_o = 2$ applied on line 12?

13) 29373.5 "all values diverge". But why does one go up and the other two go down?

14) 29373.7-14 s_o and s_b are described here. But how do these relate to the values mentioned in section 3.2? Are these the values applied in the assimilation, or are these new values? And why not put descriptions of the Desroziers (2005) method together in one place, rather than in both 3.2 and 3.4?

15) Fig. 2. What is the reason for the spike in s_b in experiment All on 7th September?

16) 29374.8 "T170 horizontal grid" (see point C again). Again a confusion between

horizontal representations.

17) 29375.4-10 Why, physically, is there less ozone between 45S and 45N?

18) 29375.24-29 This discussion on vertical velocity is vague and unsupported. Either remove, or provide supporting evidence.

19) 29376.7-9 Low resolution results are being "projected" on the "T170" grid. "Thus the error of representativeness is the same for all the experiments". This can't be true. It would work if you averaged the high resolution results to the lower resolution, but not the other way round.

20) 29377.23-25 "the Ahl experiment has globally a better agreement with the ERA-interim reanalysis than the All experiment". Is this any surprise given that MOCAGE is being driven by the ERA interim wind fields, which come on the N128 Gaussian grid?

21) 29380.20 With 275km for the correlation length scale "we are not able to represent the structures that are smaller than that size". That is untrue. The model should be able to generate structures at its own length scales. It is only the analysis increments, not the model itself, that will be smoothed by the background errors.

22) 29383.22 "the solution is likely to involve using the IASI AKs". But also it needs a better choice of background error correlation length.

Technical

1) 29362.24-26 "a lower error and better coverage Massart et al. (2009)": this is meaningless without a detailed knowledge of Massart et al. (2009). Be precise about what is meant and give some justification, or remove this statement.

2) 29363.11 "we build superobservations" .Information on the superobservations is too spread out through the text. Please gather together tables 1 and 3 and the discussions associated with them (e.g. the first part of section 5.1). Ideally this could go in the introduction, e.g. in section 2.1 next to where the assimilated observations are described.

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Please also say how the observation error of the superobs is derived, given that the raw observations come with a specified error. This may be described in section 3.2 but I found that very hard to understand. Also, what longitude and latitude is assigned to a superobservation? Or does a superob represent a model grid point?

3) 29364.12 Berrisford et al. (2009). There is now also a peer-reviewed reference for ERA-interim: Dee et al. (2011).

4) 29364.15 "The ERA interim analysis was made to improve the quality of the stratosphere... " - surely this was not the only reason?

5) 29365.15 "OMI combines the advantages of .... with the characteristics of ...". This is meaningless unless you also say what those advantages or characteristics are.

6) 29365.24 "agreement better than 2%" in what sense, i.e. mean or standard deviation, accuracy or precision?

7) 29366.4 "using its nearest neighbours for the spatial interpolation". What's the point of a horizontal interpolation if you have already superobbed onto the spatial grid of the model?

8) 29366.7 "a rejection threshold of .. 40%". Why is this rejection done?

9) 29366/8-10 "results remain consistent for the whole month... ". What results, and in what way do they remain consistent? This statement is meaningless.

10) 29366.16 "random variability". What does this mean? Accuracy, precision, or just natural variability of the ozone field?

11) 29366.22 "the assimilation is output at 12:00 UTC". Why not interpolate in time like for OMI? Indeed, what time are the ozonesonde observations valid?

12) 29368.1 It is claimed that ECMWF analyses are computed on a "N800 reduced Gaussian grid". This appears incorrect. The analysis and forecast spectral resolution has been T1279 since 2009

and that appears to correspond to N640 on the Gaussian grid: http://www.ecmwf.int/products/data/technical/gaussian/spatial_representations.html

13) 29368.10 "As a CTM, MOCAGE works with a linear time interpolation between two forcing steps." This is imprecise. What is being interpolated in time? And why?

14) 29369.4 "in the physical space" (see point C). This is confusing and would benefit from a more consistent terminology with respect to grids and representations.

15) 29369.9-11 "a global bias may appear" but "a multiplicative correction factor" is applied. "bias" tends to imply an offset, not a multiplicative correction, so this a bit confusing.

16) 29370.7-10 Very long winded. Why not just say "the observation errors are assumed uncorrelated?"

17) 29370.24-26 "for each IASI measurement we compute differences and grid these on the T42 and T170 grid". Aren't the differences already on the grid? Are IASI super-obs or raw observations being used here?

18) 29371.23 to 29372.6. This is summarised in table 2: a lot of text could be pruned here.

19) 29372.11-12. Line colours are specified in the figure legends. No need to repeat that here.

20) 29372.25 "divided by half the number of assimilated observations". This makes the simple formula J/2n sound quite confusing. Why not just give the formula?

21) 29373.18-19 "observations constructed with MLS data only during that period". This is confusing. You're not merging the MLS and IASI observations together, except in the data assimilation, but that's what's implied here.

22) 29373.25-27 This paragraph could be deleted.

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23) 29374.9 "energy spectra". Please give a reference or equation for this.

24) 29374.25 First this sentence says "differences are below 2% over the globe", which implies everywhere. But then it says they "reach 4% over the SP".

25) 29375.3 The biases in Fig.4 do not appear to be large or significant at 70S. 90S maybe?

26) 29375.14 "Maximum values are located" - note that a figure in ppmv, not mPa, would likely give a very different location for the maximum.

27) 29377.6-15 This text seems to be just describing the figure without adding any new information. Is that necessary?

28) 29378.6,8 "above 68hPa" - this is ambiguous.

29) 29378.12-27. Being based on at maximum 47 observations, it's hard to believe that differences between Ahl and All in Fig.9 are significant. Given that, this discussion could be much shorter. Indeed the whole of section 4.2.3 could be reduced to just a sentence or two and joined with the slightly more interesting results given in section 5.2.

30) 29379.15-28. This is quite a long winded paragraph. Could you pull out the key points and forget the rest?

31) 29380.24 This text duplicates what's on the caption. Could it be deleted or compressed?

32) 29381.16-21 This text has an appropriate level of detail, and one that would be recommended for the results in section 4 as well.

33) 29384.16 "better agreement .. in terms of variability". Variability is a vague word. Be more precise.

34) Table 2 column headings: "hor. res."; "assim" and "hor. len." should be expanded.

35) Figure 9: x scale in RH panel should be expanded to include the spike in the Ahh (green) curve.

36) Figure 11 panel (a). This illustrates one confusing thing about the superobs. They do not seem to be on a regular 0.7 degree Gaussian grid as might be expected. Also, "cumulated" -> accumulated.

Typos / grammar

1) 29358.11 "the whole atmospheric columns" -> "the whole atmosphere"

2) 29358.20 "when the ozone" - delete "the"

3) 29359.13-16 sentence is very hard to read

4) 29360.2 remove colon

5) 29360.9 generates -> causes

6) 29360.23 that -> which

7) 29361.15-16 main clause has no verb

8) 29361.17 "." -> ":"

9) 29363.5 remove "alternatively"

10) 29364.26-27 data, data - awkward repetition

11) 29365.22 (and elsewhere) "total ozone columns data": either "total ozone column data" or just "total cozone columns"

12) 29366.1 "per band of latitude"

13) 29366.6 "observation measurement". tautology

14) 29366.19 "ozonesonde measurements"

15) 29367.5 "super-observation resolution"

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16) 29367.18 "As we are only interested in the ozone distribution" -> "We are interested just in ozone, not other chemical species".

17) 29372.26 "validation diagnostic"

18) 29374.20-21 This is a confusing sentence. Do you mean the difference between concentrations and columns, or Ml and Mh?

19) 29375.18 "September 30 and 55hPa" confusing.

20) 29381.18 differ from -> differ by

21) 29381.24-26 this is a confusing sentence

22) 29382.6 2nd instance of shows -> has

23) 29382.11 are -> is

24) 29382.12 smoother -> less distinct? shallower?

25) 29382.28 compared to -> over

26) 29383.1-2 "different in Ahh from Ahl" -> "different between Ahh and Ahl"

References

Struthers, H. et al. (2002) "Assimilation of ozone profiles and total column measurements into a global general circulation model", Journal of Geophysical Research (Atmospheres), Volume 107, Issue D20, pp. ACH 10-1, DOI 10.1029/2001JD000957

Bormann, N. et al. (2010) "Estimates of spatial and interchannel observation-error characteristics for current sounder radiances for numerical weather prediction. I: Methods and application to ATOVS data", Quarterly Journal of the Royal Meteorological Society, Volume 136, Issue 649, pages 1036–1050

Dee, D.P. et al. (2011) "The ERA-Interim reanalysis: Configuration and performance of the data assimilation system", Quarterly Journal of the Royal Meteorological Society,

137, 553-597

See the paper under review for the other references.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 29357, 2011.

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