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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Satellite observation of lowermost tropospheric ozone by multispectral synergism of IASI thermal infrared and GOME-2 ultraviolet measurements" *by* J. Cuesta et al.

Anonymous Referee #3

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General Comments

The paper by Cuesta et al. presents a demonstration of the improvement of sensitivity to lower tropospheric ozone upon combining information from IASI and GOME-2 observations. The authors use an altitude dependent regularization approach to constrain the inversion and can show in the form of the averaging kernels enhanced sensitivity to ozone in parts of the troposphere above the single instrument cases. A lower tropospheric ozone pollution event in north Western Europe in August 2009 is further used to demonstrate how well the joint and single instrument approaches perform. The profiles are evaluated against 3 months of ozonesonde profiles at this time of year and in

the same geographical region and compared with two days of output from a chemistry transport model. In general this paper is well presented and researched, although relies heavily on the reader to seek specific explanations or descriptions of methodology elsewhere in the literature, including for some aspects or derivations that are essential to the methodology presented. Some aspects of the method should be presented more clearly as at times it is confusing. The authors are able to clearly demonstrate the advantage of combining the measurements from both sensors, which is a significant step. However, I do not think it is sufficiently made clear in the title or the paper that the information presented (including numbers quoted and conclusions drawn) relates to just a small region or a short period of time. This is a short case study paper rather than a full validation of the technique. This is important to emphasise because a case study in Northern Hemisphere summer over Europe provides arguably optimal conditions for retrieving tropospheric ozone with both instruments, and the technique might not yield such significant improvements under different conditions globally, seasonally or over the life of the instruments. The paper is very suitable for publication in ACP, but subject to addressing some of the following points.

Specific Comments

1) Introduction: In addition to the often referenced EAA (2011) there are a number of other direct references that at relevant which the authors may wish to consider for variety (eg http://www.biogeosciences.net/9/271/2012/bg-9-271-2012.pdf)

2) Introduction page 2960 line 3: It would be contentious to say that this is a completely validated method due to the very limited temporal and spatial application of the method presented in this paper, without such a caveat or ability for the reader to compare it to prior knowledge.

3) Section 2, page 2960 line 18: What is the reason for using the smaller GOME-2 pixels when the method of Cai et al., 2012 did not? Is it to reduce the inhomogeneity of the scene for the combination with the smaller IASI pixels? The combining of GOME-2

pixels is specifically to increase the signal to noise because not doing so results in very noisy tropospheric ozone retrievals, so it would be very relevant to comment on this.

4) Section 2.1 page 2061 line 13: The presence of cloud would be expected to impact spectra from both instruments differently in different pixel domains. This may introduce some homogeneity issues, how are they addressed? I would suggest that the description of the treatment of cloud could be generally improved. It affects both IASI and GOME-2 radiances differently and there are a number of ways of handling this. The ozonesonde comparison later in the paper eliminates IASI and GOME-2 pixels with a high cloud fraction but not necessarily those with cloud at a high altitude - which even with a low cloud fraction can impact the retrievals depending upon how clouds are dealt with in the forward model, for which there is only a passing reference to other papers.

5) Section 2.1 page 2961 line 23: Have the authors considered that one way of increasing the degrees of freedom for signal for the GOME-2 only retrieval would be to use more of the spectrum below 290nm?

6) The approach presented uses a vertical grid of 1km interval which given the number of independent pieces of information for each altitude makes them very highly correlated. This is not made sufficiently clear to the reader. The 0-3 km and 3-6 km sub-columns are not independent. This last point is touched on at the end of section 4 but the vertical correlation should be indicated, especially in reference to the averaging kernels.

7) Section 2.1 page 2963 line 9: It would be good to state why a “soft” recalibration is applied to the GOME-2 data if it is important, as it is not clear from the sentence.

8) Section 2.3: I am confused by the terminology and explanation of the altitude dependent regularisation matrix. Eremenko et al., (2008) states that the method yields an information content not dependent upon the prior knowledge. In that paper, the prior covariance matrix is replaced by the regularization matrix in defining the averaging kernel A (c.f. Rogers 2000, page 56). In this paper however, the a priori total

error is introduced but not fully explained. The description of s_colˆtot seems inconsistent with its use in Eremenko et al., (2008) if they are termed a priori errors. Are they rather a posteriori errors? In equation 2 it isn’t clear to me how s_LMTˆtot and the other sub-column values are calculated. It is not clear from this to what extent the altitude-dependent regularization also acts as a smoothing function to the profile, which would be pertinent to the interpretation of the results. I don’t feel that the altitude regularization matrix is sufficiently explained to make the work reproducible, or for the reader to understand the relative impacts of the covariance matrices on the results.

9) Section 2.3, page 2965, line 4 and section 3.3 page 2971 line 13: By matching the coefficients in this way, isn’t the contribution from GOME-2 measurements then always going to be limited? In terms of spectral consistency between the UV and TIR, does the fit of YUV degrade when YTIR is also fit? It would be very relevant to comment on these points because they pertain to the relative improvement of the joint approach.

10) Section 3: It would add context to the size of the fit residuals to indicate how their specified values compare with the measurement noise. I would disagree that systematic features in the fit residuals for GOME-2 in channel 2 are only “slightly apparent”. Whilst the axis scale of Figure 1b is not optimised to show this, close inspection does indeed show that there is a robust systematic shape - particularly compared to the other wavelength regions – and in the region where tropospheric ozone information is derived from GOME-2.

11) Section 3.1 page 2967 line 8: This is quite a difficult sentence to follow, I would suggest rewording it. Do the authors mean that using multiple IASI pixels for each GOME-2 pixel yields similar fit residuals in clear-sky pixels? Is this the case globally or in the case study presented in this paper?

12) Section 3.2, first paragraph: It is not sufficiently clear in this section over what domain the statements made about the approach apply. There is an indication in the table caption but it should be mentioned in this paragraph so that the statements can

be taken in context.

13) Section 3.3: The end of the first paragraph states that "The comparison is made for each ozonesonde with the average of collocated satellite retrievals." Are all pixels within the collocation criteria for a given sonde averaged, resulting in one mean profile representing that region/timeframe? Smoothing over this large area effectively removes noise from the retrieved profile. Is an average AK applied to the sonde or are individual AKs applied to the sonde and a mean value taken? Perhaps it would be positive to give a more detailed description of the method used to validate the case study. It would be useful to state how well the sonde performs against the prior in addition to the convolved and direct comparisons.

14) Unphysical or aberrant retrievals are mentioned. How do the authors know if it is a good retrieval or not? It is not sufficient to just discuss those that worked well without at least an indication of the frequency of aberrant retrievals or their origin, and the figures do not indicate where a pixel was removed because of cloud or because it was aberrant. Are all (non-cloudy) IASI pixels used or are there any other quality control criteria applied? Is there any scan position dependence on the product or its uncertainty?

15) Section 4 page 2972 line 13: Conditions precipitous to these events are discussed in the literature and a personal communication seems inadequate as a reference. For example, see references within Richards et al., (2013) [ACP 13] which though focussed on high ozone events in Southern Europe also discusses mechanisms.

16) In the comparison with the chemistry transport model in section 4, it would be useful to see how the prior sub-column amounts compare. The sub-column ozone (as measured in Dobson Units) is strongly influenced by orography and it is not immediately obvious from figures 7-9 where enhancements are due to a deeper physical column (which occur over sea) or an actual ozone plume. If the prior does indeed consist of a constant (latitude-dependent) volume mixing ratio a plot of the prior in DU would make

the skill of the retrieval immediately clear in the places where it departs from the prior.

Technical Corrections

Introduction page 2958 line 5: The sentence starting "Air pollution's most important. . ." is a cumbersome sentence and "damages on" would need to be "damages to" to make sense. I would suggest rewording as "The most important damage to ecosystems by air pollution is caused by ozone through. . ."

Introduction page 2958 line 17: I would suggest "capable" or "suitable" in place of "adapted" in this context, since it is not a transitive case.

Introduction page 2958 line 18: Since the paper concerns IASI, I would suggest replacing the text in brackets "(e.g. . . .)" with "such as the Infrared. . . (IASI) onboard the MetOp satellites (Clerbaux et al 2009)"

Introduction page 2958 line 24: I would suggest somehow breaking apart the sentence beginning "They are based on. . ." which is 7 lines long. The part "spaceborne instruments as the Global. . ." should probably be "spaceborne instruments such as the Global. . ."

Introduction page 2957 line 5: It should be specified for future clarity that the IASI and GOME-2 data considered here are from the instruments aboard MetOp-A, particularly as the sentence goes on to mention the future platforms.

Introduction page 2959 line 27: The use of the word "provided" here isn't quite correct in this context. "resulting from" or "afforded by" would be suitable alternatives. Introduction page 2960 line 1: The footprint size of a GOME-2 pixels at the edge of the swath is the same as at nadir, unlike IASI.

Section 2.1 page 2961 line 5 & 7: Was the data used in the study specifically from these sites? If so using 'from example given' is redundant.

Section 2.3: "a priori" should be italicised.

Section 2.3 page 2966 line 12: Should "...and it provides with a greater number..." read "...and provides a greater number..."

Section 3.2 page 2967 line 16: Should "in average" actually read "on average"?

Section 3.3 page 2971 line 15: "...keep as low as the ones for IASI..." would be better as "keep as low as those for IASI..."

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