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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 #2

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The paper describes a method for retrieving ozone vertical profile from a simultaneous use of collocated IR (IASI) and UV (GOME2) radiances. It analyses and discusses the results in terms of vertical sensitivity in the lowermost troposphere (and lower troposphere) and goes a step further by examining how the combination of instruments improves the retrieval of lower tropospheric ozone columns by comparing the retrievals during a summer pollution episode over Europe to model (CHIMERE) simulations. A comparison to ozone sondes at mid-latitudes is also provided.

The paper is well written. It tackles an important but challenging methodological problem (until recently the advantage of the synergy was only demonstrated theoretically).

Overall it provides good evidence of the improved performances of the combined ("multispectral" UV+TIR as called in the paper) versus individual (TIR or UV) ozone profile retrievals. However, I feel at the same time that there is insufficient critical analysis, here and there some overstatements and that the discussion is too much oriented towards the "positive" side. The paper is suited for ACP but before publication the following general comments should be addressed.

GENERAL COMMENTS:

1. Section 3.2, especially page 2968. The discussion around the DOFS is done here on a relative (percent improvement) basis. In my opinion this is very misleading. For example, the authors explain that the DOF for the combined retrieval is 77 %, 40%, 21% higher than IASI for the column up to 1 km etc. . . But if I take it from Figure 3, the first number (77%) refers to a change of DOF from about 0.05 (IASI alone) to about 0.1 (IASI+GOME2). This is thus an increase of DOF of only 0.05. I wonder how significant that can be and more generally how representative a DOF of 0.1 is for a partial column. The same applies for all DOF numbers given in the paper (not for the altitude of maximum sensitivity, which is therefore probably also more convincing). I urge the authors to revise this part of the discussion of vertical sensitivity by providing absolute values for the DOF comparison and by building the discussion more critically and carefully around these. I would also suggest to add variabilities (plotted as shaded areas around the mean curve?) in Figure 3. Another example of overstatement (or lack of critical analysis) is provided with Table 1 and the related text page 2967: The text refers here to a significant gain of sensitivity of the combined retrieval as compared to IASI or GOME2 taken individually by comparing the total column DOF. The gain is indeed impressive, with a DOF increasing from 3.02 (IASI) or 3.68 (GOME2) to 5.2 for the combination. But the improvement in the troposphere (the focus of the paper) seems, from the other values listed in the table, far from being that impressive. So where is the improvement on the profile; in the UTLS or the stratosphere? The paper is too oriented towards what it wants to show and misses some other important aspects. According

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to this, the second sentence in the conclusion "The information content enhancement for IASI+GOME-2 enables an increase of sensitivity to the whole atmospheric column and especially below 3 km" is (unless I missed something) overstated and probably incorrect.

2. In section 3.3. the authors explain that (page 2970, lines 19-20) "both comparisons show good agreement between IASI+GOME2 retrievals and ozonesondes, with very similar results as for the IASI only retrieval ...". This is important but surprisingly not discussed further in the paper. To fully support their findings and add critical information, the authors would need to include the IASI alone and GOME-2 alone retrievals vs ozonesondes comparison in the paper, especially in Figure 6 with similar diagnostics (R coefficient, bias, RMS). They should also add one Figure showing the comparison of profiles between the mean sondes, the mean IASI, the mean GOME2 and the mean IASI+GOME2 (with standard deviations), in order to discuss in what altitude range the improvement is and if it is significant.

3. Few details are given here to the "failed" or "aberrant" retrievals. Considering the challenging aspects of the retrievals, if would be instructive to know more on, for instance, the fraction of failed retrievals but also (at least to some extent) the reasons for this. Beyond spectroscopy, which is discussed thoroughly, what is the impact of the fractional cloud cover, of aerosols, of TIR emissivity, of different air masses probed (viewing geometry of GOME2 vs IASI).... All seem critical, as they could lead to incoherencies between TIR and UV but this is not discussed. Furthermore, are the 200 "typical" residuals in Figure 1b examples of good fits? Are there other cases? I don't say here that bad results need to be shown but at least that the good ones be discussed in a balanced way. Related to this, on page 2970, the authors mention "quality checks". What are these? And on page 2971, what are "aberrant retrievals"; can the reasons for the failure be identified?

4. Table 1 Looking at the altitude of maximum sensitivity we see a rather large variability, especially for IASI (3.02 +- 0.67 for the LMT). In the case of IASI, is this due to

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thermal contrast? And what is the reason for GOME2; surface reflectivity? The paper should make clear if in the favorable cases for the TIR or the UV the combination remains as advantageous (or reversely highlight the cases in which the combination is the most advantageous). For instance, it seems that in the best cases the IASI alone retrievals provide a sensitivity down to 2.3 km, pretty close to the combined retrievals. This is briefly discussed page 2969, line 7 but again, a more detailed discussion as function of the principal parameters influencing the sensitivity to the surface concentrations would be helpful.

MINOR COMMENTS

Page 2961, line 17: The retrieval is not sequential (as TIR and UV are combined in a single vector) but this would be an alternative option for combining the information. This just to say that the word "sequential" is here ambiguous and, I think, could be avoided ("independently" says it all).

Page 2968; third paragraph: the discussion of the altitude sensitivity is given here in comparison of the LMT, which is not defined on a "geophysical" basis but arbitrary as the ground-3 km layer. The authors should accordingly be careful in the way the results are presented as it sounds that if the altitude of maximum sensitivity is higher than this arbitrary 3km limit, the retrievals misses something important.

Page 2969, line 12: 400 below H_LTmax. Not sure that a value of 2.6 km for HLTmax is mentioned before.

Page 2969: Last paragraph of section 3.2. This discussion is unclear to me. Please clarify.

Page 2973, throughout section 4.1. : What is the altitude of the pollution plumes? Is it boundary layer pollution? Or higher up? CHIMERE should be able to tell and it would be instructive to know if the instruments (alone or with the combination) are able to probe this surface layer.

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TECHNICAL CORRECTIONS:

Page 2962, line 6: Cai et al. (2012) Page 2970: Uccle is in Belgium. Page 2974, line 13 HungAry.

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