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Comment

Interactive comment on “Acetylene (C₂H₂) and hydrogen cyanide (HCN) from IASI satellite observations: global distributions, validation, and comparison with model” by V. Duflot et al.

V. Duflot et al.

valentin.duflot@univ-reunion.fr

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Interactive comment on “Acetylene (C₂H₂) and hydrogen cyanide (HCN) from IASI satellite observations: global distributions, validation, and comparison with model” by V. Duflot et al.

Response to Anonymous Referee #2 General Comment: This is a well-written, clearly structured article, documenting the fast retrieval and evaluation of HCN and C₂H₂. I recommend publication after the following (minor) comments have been addressed.

The authors would like to thank the reviewer for reading the manuscript and suggesting

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changes which have helped us to improve it. The responses to individual comments are developed here below:

- Abstract, lines 8 and 14: Please include the range of correlations coefficients found for the agreement.

The suggested changes have been made in the Abstract (lines 8 and 14).

- Intro, line 19-20: Please include some information on atmospheric levels (background, urban, biomass burning) from literature.

The suggested changes have been made in the Intro (lines 21-23) for background and biomass burning levels (no reference has been found for urban levels).

- Intro: Think including an extra table for C₂H₂ and HCN summarizing their budgets (sources/sinks etc) would be useful to the reader & improve paper.

The suggested changes have been made in the Intro (lines 21-23) and an extra table has been added (Table 1).

- Page 12363, line 11: Why were the million spectra only chosen from 2009? Surely a sample selected from 2008-2010 would have been better? Were all these observations cloud free? If not, how does the HRI vary with different amounts of cloud cover?

The goal here was to have a set of spectra wide enough to generate a globally representative total measurement error covariance matrix. The choice of the year has no impact on the results. The randomly chosen spectra are cloud free spectra; this has been specified in the text (page 14363 line 11).

- Page 12363, line 24: Here I assume you mean minimal interference from CO₂, H₂O and O₃? Sentence read ok, but could be phrased slightly better.

No, the goal here is not to minimize the interferences from the interfering species: we need to have clear signatures from them to calculate the total measurement error covariance matrix and maximize the contrast with the spectral background. This is why

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the selected ranges include signatures from the interfering species.

- Page 14364, line 15: Please add the units of B_HCN and B_C2H2.

The units have been added in the text (Page 14364, line 15).

- Page 14364, line 20: Where the C2H2 and HCN profiles taken from (i.e. before the 1 km perturbations were applied)? Were they from a model, climatology, or aircraft observations? Page 14365, line 2: what are 'standard absorption profiles'? Please clarify.

To take into account all possible cases of concentration value and altitude of the polluted layer, we artificially built up the C2H2 and HCN profiles, without using any model, observations or climatology. Then, to compute the corresponding spectrum for each of these artificial profiles, we used the forward model of the line by line radiative transfer model Atmosphit together with absorption profiles coming from standard modeled atmospheres (as stated page 14365, lines 6-8) for the other species. This has been clarified in the text (page 14365, line 2).

- Page 14365, line 13: Please add the units of b_HCN and b_C2H2.

The units have been added in the text (Page 14365, line 13).

- Page 14366, line 6-11: Think some discussion of the Jacobians with respect the HCN and C2H2 vertical distribution is warranted, and/or also include or add to the figure some HCN and C2H2 profiles. I would like to see where the HCN and C2H2 is.

HCN and C2H2 vertical distributions in a standard temperate atmosphere are now shown in Figure 3, and discussed with respect to the Jacobians (Page 14366, line 6-20).

- Page 14366, line 14-18: There is no mention of cloud cover/contamination. Is it important? Please add some brief discussion.

We consider only cloud free spectra here. It has been stated in the text (Page 14366,

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line 14-18).

Page 14366, line 23: Why was a 1.5 km altitude chosen? What is the justification of this level? If a level of 1 km, 2km or 3 km were chosen how would this impact the sensitivity analysis?

In general thermal contrast can be defined as the temperature difference between the surface and the air temperature at some altitude of interest. We chose here to consider the same definition as in Van Damme et al. 2014. This has been added in the text (Page 14366, line 23-25).

- Page 14367, line 18: Sorry, why was a 30% confidence level chosen? Please explain in more detail.

This 30% confidence level for subtropical atmospheres was first chosen as a prudent assumption (considering that subtropical atmospheres share properties with both tropical and temperate atmospheres, and that the confidence in the independence of the retrieval method to the atmospheric parameters for a $0.26 \cdot 10^{16}$ molecules/cm² stability threshold is 10% for a tropical atmosphere and 40% for a temperate atmosphere). We decided to perform simulations in subtropical atmospheres to check our first guess. It results in a 25% confidence level for a $0.28 \cdot 10^{16}$ molecules/cm² stability threshold in subtropical atmospheres. It has been added/corrected in text (but not shown in Figure 5).

- Section 3.1: How do the IASI/FTIR comparisons change if the 0% cloud contamination is used, or if the co-location limits are shrunk or expanded?

IASI spectra are usually considered to be exploitable with a cloud contamination fraction up to 25% (Clerbaux et al., 2009 ; Van Damme et al., 2014). As we deal in this study with species with quiet weak spectral signatures, we chose here a prudent 10% cloud contamination fraction threshold to avoid interferences in the considered spectral ranges. Using a 0% cloud contamination threshold would not change significantly the

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comparison, but would only decrease the number of spectra useful for it. These (usual) co-location limits ($1^\circ \times 1^\circ$) were chosen as they allow to have a significant number of collocated spectra together with a reasonable representativeness of the ground-based measurement point.

- Also why were these four sites selected, it was not clear in the text (but I assume because they only measured these target gases)?

Indeed, these four sites were selected because they were the only ones to measure the target species during the studied period (to our knowledge). This has been added in the text.

- Page 14369, line 23 and onward: How does the peak-to-peak accuracy of the measurements compare (i.e. how well does IASI actually capture the timing and magnitude of peak events)? Please add this statistic, as it is important given HCN and C₂H₂ biomass burning sources.

The way IASI capture the timing of the peak events can be evaluated considering Figure 8 and the given correlation coefficients. The magnitudes of the peak events are now given in the text for both space and ground-based observations (Section 3.1).

- Page 14370, line 3-6: Could the FTIR averaging kernels be applied to the IASI data? Or could the IASI Jacobians be applied to the FTIR data (as done later for the model comparison)?

Unfortunately not. One should always apply either the averaging kernels of the less sensitive instrument on the data measured by the more sensitive one, or a combination of the two averaging kernels on both data sets. In the case of the comparison FTIR vs IASI, one should then apply IASI averaging kernels on FTIR measurements (IASI being obviously less sensitive than the ground-based instruments). Regarding the Jacobians, they only can be applied on profiles to take into account IASI sensitivity. It was then possible to apply them on model outputs (profiles), but not on FTIR measurements

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(total columns).

- Section 3.2: Nicely summarised!

Thanks !

- Section 3.3: The key point for me here (which is not discussed) is how well does the model agree with the FTIR data? I think a sub-section addressing this point needs to be added to the paper (& probably an extra figure).

This is right. A subsection (3.3.2) as well as a new figure (Figure 14) have been added to the article to address this point.

- Figure 9: There are some C₂H₂ enhancements over the China coast in DJF and MAM, simulated by the model that are not observed by IASI. I don't think this was raised in the text. Can you elaborate?

This is right. Discussion has been added in the text on this topic (Page 14377, lines 5-15).

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/15/C6158/2015/acpd-15-C6158-2015-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14357, 2015.

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