

## ***Interactive comment on “Measurements of hydrogen cyanide (HCN) and acetylene (C<sub>2</sub>H<sub>2</sub>) from the Infrared Atmospheric Sounding Interferometer (IASI)” by V. Dufлот et al.***

### **Anonymous Referee #2**

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This paper represents a useful contribution to remote sounding of important trace gases in the troposphere using IASI data and so is suitable for AMT. It seems clear that both HCN and C<sub>2</sub>H<sub>2</sub>, the targets of this paper, are retrievable from IASI and the paper provides evidence of the authors' innovative development of the IASI suite of trace gases. The paper, however, needs some considerable strengthening to support the more detailed results obtained and to diagnose the reasons for the comparisons observed. Also the conclusions need to be much more carefully drawn (see point 6 in minor comments, for example). I have identified four major points to help the authors improve the paper but overall there is a need to provide more evidence and character-

isation of the data.

Major comments:

1. It is clear that line mixing is important as argued by the authors. However, there is no discussion of the ability of the line mixing model to represent the real atmosphere spectrum and what uncertainty remains. It would be useful to see statistical/averaged plots of the spectrally-resolved residuals from the retrievals which demonstrate that there is no residual bias e.g. a time-averaged mean difference spectrum plus standard deviations of the residuals. Please provide some analysis of the residuals for more than one spectrum to show how well the data fit in general.

2. Important details are not given, even in summary for the ground-based measurement system. We are not shown the a priori profiles for each system, nor are we told which spectral intervals and retrieval methods are used for the ground-based FTIR retrievals. Was line mixing taken into account or not for the ground-based FTIR or is it not necessary because of different spectral intervals? Please provide more detail on the FTIR retrievals and comment on the averaging kernels (see next point).

3. There needs to be more detail on theory of comparison of the IASI and FTIR columns since sensitivity or lack of it is a key to this paper. If an aspect of Rodgers' theory was used then the relevant facts should be given and also their appropriateness demonstrated in some way. The ground-based FTIR averaging kernels seem very different for C<sub>2</sub>H<sub>2</sub>, and the HCN averaging kernel for Reunion looks strange in the stratosphere. These would affect the results of comparisons. Please provide more details on the intercomparison methodology, citing other papers, and possibly using simulated profiles to demonstrate that the method used is successful.

4. In Figure 3, the only result that seems to match well is the HCN at Reunion when elevated; is there any significance to the low values or are they in fact only a priori. The IASI C<sub>2</sub>H<sub>2</sub> elevation at Reunion occurs at the same time as the IASI HCN elevation but this is poorly matched in the FTIR data for C<sub>2</sub>H<sub>2</sub>. Is this a problem with the C<sub>2</sub>H<sub>2</sub> from

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the FTIR or the IASI? For HCN at Jungfraujoch the IASI retrieval seems to jump from low values to high values. These effects need some diagnosis. I would suggest some correlation plots of IASI vs FTIR and also a clear explanation of the high values in IASI HCN, C<sub>2</sub>H<sub>2</sub> which are not matched by the FTIR. Finally it should be shown that for low values of C<sub>2</sub>H<sub>2</sub> and HCN columns, the IASI (and FTIR) values differ significantly from the a priori total columns. What are the a priori total columns for each?

Minor comments

The English generally needs some tightening.

1. “Specie” should be “species” everywhere.
2. P.7572. How many IASI column retrievals are lost because of a high RMS fit error (fraction of total clear sky)?
3. P.7572 “quiet” should be “quite”.
4. “indiscernible from” should be “overplotted by”?
5. P.7573. Bottom. These statements on poor performance when not using line mixing should be justified. I would expect them to be true but we need some figures. What are the corresponding correlations, RMS errors etc.?
6. P.7575 “2-yr” should be “two year”.
7. P.7576. The results do not seem to demonstrate that these retrievals can be used for long-term time series without understanding of where they work and where they don't. We only know where the in situ agrees which is HCN at Reunion; for the other comparisons there are significant caveats. Please qualify this statement (having done the major corrections above). Given the evidence if we were to use these retrieved data, how do we know where a good result can be obtained? African biomass plumes only? Mostly for HCN in the tropics?

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