Response to Reviewer #2

First, we would like to thank the referee for his positive general comment about the paper. The revisions have helped improving the paper and each of the comment was addressed. The comments are reproduced below in italic, each with a number, and our responses are provided on a point-by-point basis.

Minor issues

(1) *Line 67 – Does Wang et al. 2007 actually state that ACE-FTS is measured with a 3% accuracy, or does ACE-FTS HNO3 agree with correlative data from other instruments to within 3%? Because ACE-FTS does not have official accuracy/precision estimates for their data sets.*

Wang et al. 2007 does actually state that ACE-FTS retrievals are obtained with a precision of 2-3%. The information can be found at page 4927, section 7.3: "Version 2.2 FTS retrievals taken during 9 February to 25 March 2004 are used for the comparisons in this paper. The retrievals are carried out in the 10 to 37km range with a typical precision of 2 to 3%." (Wang et al., 2007).

(2) *Lines 69-70 – Please explain what is meant by "less used".*

The purpose of the sentence was to notify that the data set obtained from SMR/ODIN has been somewhat less used to analyse geophysical processes in the atmosphere. We agree that it could have been misinterpreted and the sentence has been modified for more clarity to: "Measurements from the SMR instrument on the ODIN satellite made at high vertical resolution (1.5-2 km) but with a precision of only 1.0 ppbv over the altitude range 18-45 km (Urban et al. 2009; Wang et al., 2007) have not been used much so far for geophysical analyses." (page 3, lines 59-61).

(3) *Lines 81-82 – "suffers" might be a bit harsh. I would suggest rewording to something like, at the time of Wespes et al. 2009, the FLORI algorithm only allowed for total column retrievals, and it wasn't possible to do a rigorous validation study.*

The sentence was reworded to: "Although important conclusions and perspectives for the capability of IASI to sound HNO_3 were drawn from this study, the FORLI algorithm at that time only allowed for total column retrievals, and it wasn't possible to do a rigorous validation study due to the absence of archived data from ground-based FTIR measurements." (page 3, lines 70-73).

(4) *Line 89 – Why are you limiting to just one year of data? Please explain why 2011 was chosen. As well, perhaps in the Conclusions section (or wherever you feel it fits best), it would useful to mention if 2011 are representative of other years, or how results from other years might differ.*

We have chosen to characterize and validate the data set based on one year, first to make it manageable to carry in-depth analyses. The fact that several latitudes have been selected allow capturing representative seasonal cycles, which would be similar in other years. The year 2011 was chosen based on the homogeneity of the temperature, humidity and cloud data from the EUMETSAT operational Product Processing Facility (August et al., 2012). A sentence explaining this was added to the Conclusions section: "One year (2011) of data has been investigated here but we expect the results and conclusions to be valid as well for other years of IASI data. The only particularity of the year 2011 is the exceptional denitrification that happened in the Arctic and that IASI was shown to capture well" (page 14, lines 427-429).

(5) *Lines 134-135 – Hurtmans et al. 2012 does not actually go over how sensitivity is calculated, nor the total column averaging kernel. There should be a quick one or two sentences (either here or in the discussion of Fig. 2) on how these are calculated, just so there's no confusion.*

The method for calculating the sensitivity and the total averaging kernel was moved to make the interpretation of Figure 2 clearer. The explanation about these two quantities is now given where Figure 2 is introduced: "Also represented on Figure 2 is the IASI (and FTIR, in bottom panels) so-called "sensitivity" (red curves). The sensitivity at altitude i is calculated as the sum of the elements of the corresponding averaging kernel, $\sum_j \boldsymbol{A}_{ij}$ (with $\boldsymbol{A}$ the averaging kernels matrix) and represents the fraction of the retrieval that comes from the measurement rather than from the a priori profile (Vigouroux et al., 2007). As opposed to this sensitivity, the total averaging kernel (dashed black lines) is calculated as $\sum_i \boldsymbol{A}_{ij}$ and represents the contribution of each level to the sensitivity at a given altitude i." (page 6, lines 158-164).

(6) *Line 138 – Please briefly explain how water vapour is accounted for in the retrieval, and what is the effect on HNO3 uncertainty?*

The initial water vapor profile introduced in the radiative transfer is specific to each IASI observation and is that retrieved using the operational EUMETSAT L2 PPF. While it is assumed to be accurate, we nevertheless further readjust the water vapor column in the range of HNO_3 to improve the spectral fitting (Hurtmans et al., 2012). The resulting error on the water column (part of the state vector), although small, propagates in the error on the HNO_3 profile. It was shown in Wespes et al. (2007) for a similar nadir-looking instrument (see Figure 5 in Wespes et al. 2007) to be a negligible part of the total error. A short sentence was added in the text to clarify this: "Water vapor is adjusted as a column to improve the spectral fits in the range selected for the HNO_3 retrievals. The uncertainty on the water vapor column induces only small errors on the retrieved HNO_3 concentrations." (page 5, lines 121-123).

(7) Line 151 – Please explain what is meant by "suspect" averaging kernels.

Averaging kernels are considered suspect if they show unexplained and unrealistic oscillations in their profile.

(8) Figure 1 caption – I'd suggest changing "variability" to "1σ variation" (i.e. square root of the diagonal of the covariance), unless this is not what is shown.

Indeed, the error bars represent the 1σ variation around the a priori profile. The caption has been modified: "Figure 1. Example of IASI HNO3 vertical profiles, for July, at three locations in the northern hemisphere: Izaña (28.3°N, 16.5°W), Jungfraujoch (46.6°N, 8.2°E) and Thule (76.5°N, 69.0°W). The a priori profile and a priori 1σ variation (horizontal bars) are represented in red, and the retrieved profile and its error are in black. The concentrations are expressed in molecular density, i.e. molec.cm-3 (top panels), and in volume mixing ratio (ppbv, bottom panels). The black dashed line is the altitude of the tropopause, calculated as the lapse-rate tropopause." (page 25).

(9) Line 179 – "close to one" is vague, please give a typical range.

We agree that the statement was too vague. The text was modified to give the range of values for the DOFS: "On average, all DOFS values range between 0.9 and 1.2, further indicating that only one level of information can be extracted from the IASI data for HNO_3." (page 6, lines 167-169).

(10) Lines 180-187 –How do increased surface temperatures increase DOFS when there is no or little sensitivity at near-surface altitudes (it is not immediately clear to me)? Does this mean that where you have higher surface temperatures you also have increased sensitivity to HNO3 at near-surface altitudes? If this is the case, can you please show this in a plot?

Surface temperatures can be related to the signal to noise ratio of the measurements. When the surface temperature increases, the signal to noise ratio also increases and this leads to a slightly larger DOFS. The gain of sensitivity is on the entire profile and not specific to the near surface. In fact the sensitivity to the lowest part of the atmosphere is more driven by the temperature contrast between the surface and the air than by the surface temperature alone. (the surface temperature may be high with a small –unfavourable- thermal contrast; as discussed in e.g. Clerbaux et al. (2009)). The word "surface" was added to the text to ensure that no confusion is possible: "However, there are some latitudinal differences, with the DOFS being generally larger in the intertropical belt, with values around 1.1 or slightly more (e.g. in the deserts during the summer) due to larger surface temperatures inducing a better signal to noise ratio, in comparison to the mid and polar latitudes, where the DOFS is mostly around 0.9." (page 6, lines 169-171).

(11) Lines 242-243 – In what way are the data sets updated?

The updates on the Lauder and Arrival Heights data sets are actually described from line 224-230 (page 8). The sentence was completed to make it clear that the updates are following in the text: "An updated Lauder and Arrival Heights HNO_3 data set was used in this study and the updates are detailed hereafter." (page 8, line 224-225).

(12) *Lines 283-287 – as discussed above, this discussion may be more useful when Fig 2 is first mentioned*

We agree with the referee that this discussion is more relevant when Fig 2 is first mentioned. We have followed his suggestion and this part of the text was thus moved to where Figure 2 is introduced on page 6 (lines 158-164). Cf. comment *(5)*.

(13) *Line 364 – here and table 4 state that the overall mean difference is 11.5%, but the legend in Figure 7 states that it is 10.8%. Which value is correct?*

We thank the referee for spotting this. The correct value was 10.8%. However, we would like to notify the referee that there was, after the manuscript was submitted, a further correction made to the averaging kernels for Lauder and Arrival Heights. All figures and values have thus been updated with the new averaging kernels. The correction only slightly changed the values for the bias and standard deviation for these two stations (cf. Table 4) but had no further impact on the results and discussions. Hence, to address this comment in particular, the actual value for the overall mean difference is 10.5% and this is the value now reported in the manuscript. The complete updated table (page 24) is as follows:

Stations	**Bias (%) Smoothed FTIR**	**Standard deviation (%)**	**Mean IASI (molec.cm^{-2})**	**Mean FTIR (molec.cm^{-2})**	**# pairs**	**R**
Thule	4.0	9.7	$2.3.10^{16}\pm0.5$	$2.2.10^{16}\pm0.4$	151	0.84
Kiruna	8.6	11.9	$2.4.10^{16}\pm0.4$	$2.2.10^{16}\pm0.4$	206	0.81
Jungfraujoch	13.9	9.6	$1.9.10^{16}\pm0.3$	$1.7.10^{16}\pm0.3$	583	0.91
Izaña	9.2	9.8	$1.1.10^{16}\pm0.2$	$1.1.10^{16}\pm0.1$	256	0.74
Lauder	~~21.7~~ 16.3	~~13.0~~ 11.9	$1.7.10^{16}\pm0.3$	$1.4.10^{16}\pm0.3$	~~74~~ 80	0.81
Arrival Heights	~~1.1~~ -0.8	~~16.3~~ 15.9	$1.4.10^{16}\pm0.4$	$1.4.10^{16}\pm0.3$	75	~~0.77~~ 0.78
All stations	~~11.5~~ 10.5	~~12.1~~ 11.5			~~1345~~ 1351	0.93

(14) *Lines 373-374 – It should be stated that the biases at both stations are still within the uncertainties of both IASI and the FTIRs. Hence, the "different behaviour" isn't of great concern.*

This is a good comment and the text has been modified to bring attention to this: "The reason of the different behaviour for the Jungfraujoch and Lauder mid-latitude stations is unclear at this point but, seeing that the biases at both stations are within the uncertainty range of both IASI and the FTIRs, this behaviour is not of great concern." (page 12, lines 341-343).

(15) *How is one year of data enough to be able to reliably comment on the trend in the bias?*

Thank you for this comment. One year of data is indeed not enough to establish any trend in the data or the biases. The point that we wanted to make was more that of a lack of seasonality in the bias throughout the year. The text was thus modified to: "The relative differences clearly illustrate the positive bias of IASI described above, but also that the bias does not show any particular seasonality." (page 12, lines 350-351).

We found interesting to address the question of a bias in the trends in the Conclusions section: "While no seasonality was observed in the biases, it could be interesting to conduct a similar study on a larger time scale to account for any potential trend in the bias.” (page 15, lines 449-450).

(16) Line 488 – What factors in the retrieval codes lead to biases?

Numerous factors in the retrieval codes could have an impact on the biases of the comparison between the two measurement methods. To cite a few, there could be the number of inversion levels, the pressure and temperature profiles, the surface characteristics (temperature, emissivity, …), the model used for the water vapor continuum, … It is hard to know exactly what factors are responsible for the observed biases and to what extent, but it seems fair to assume they are at least partly responsible for the mismatches between the two types of retrieved values.

Technical issues

(1) Figure 4 – legend label “covariance” should read “variance”.

The legend label was modified to read “variance” (page 28).

(2) Line 255 – by “regardless” do you mean “independent of”?

Yes we meant “independent of”. The text has been modified to avoid any confusion: “The a priori profiles are described individually at each station, independent of seasonality.” (page 8, lines 234-235).

(3) Figure 7 – the coloured dashed lines are not easily visible (and perhaps could be omitted).

The coloured dashed lines have been removed of the figure for more clarity (page 31).

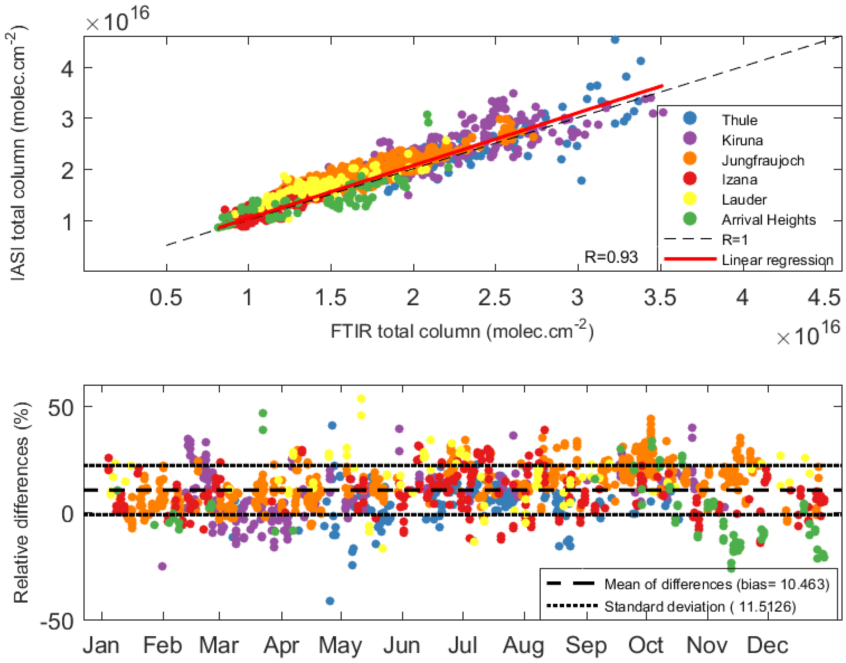

Figure 7. (top) Comparison of the smoothed FTIR and IASI partial (5-35 km) columns for all stations considered, for the year 2011. Also shown is the correlation coefficient value. (bottom) Time series of the relative differences between IASI and FTIR total columns (calculated as ((IASI-FTIR)/FTIR)*100) (%). Also shown are the bias and standard deviation when considering all stations together (black dashed and dotted lines, respectively).

(4) Figure 9 – Many of the shaded regions are covered, diminishing their usefulness. I would suggest perhaps not shading, instead using coloured dashed lines at the extremes.

We agree with the referee that the shaded areas are not easily visible. We have carried out several tests to try and find the best way possible to represent the standard deviation (cf. Figure A for two of these tests) but it seemed to us that the shaded areas are still the best option. It was thus chosen to leave the figure as it is in the original version of the paper.

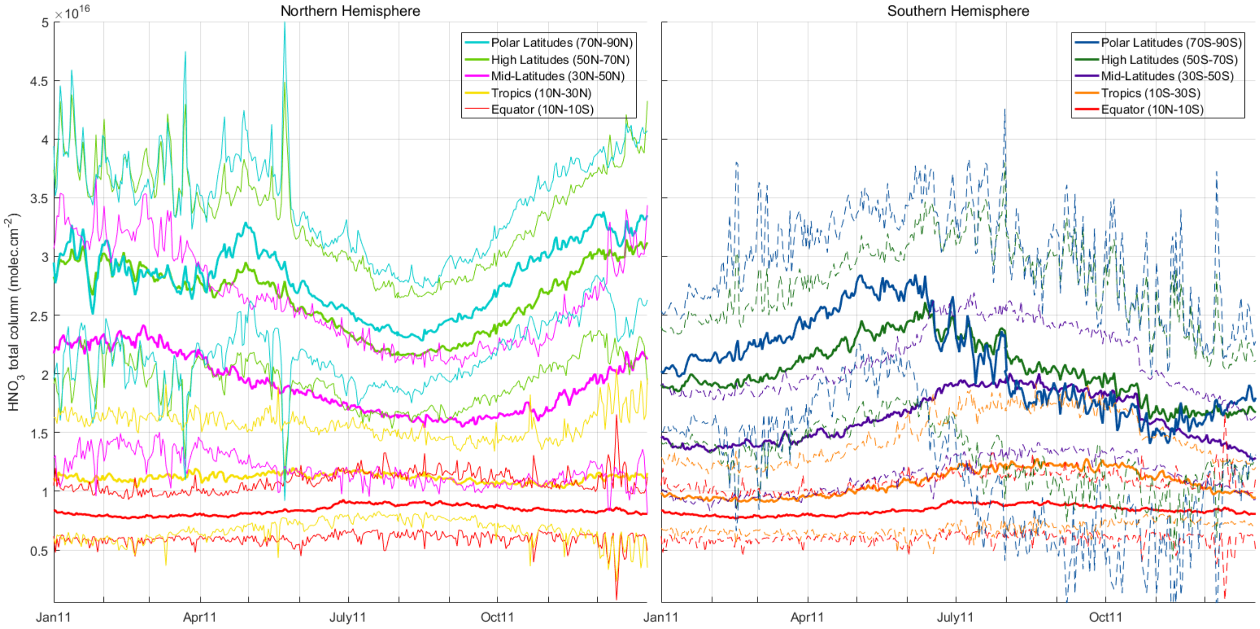

Figure A. Test of different layout for Figure 9 of the paper. (left) bold line for column and normal lines for the extremes of the daily variability. (right) bold line for column and dashed lines for the extremes of the daily variability.

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