Response to reviewer #1

We thank the reviewer for having appreciated the improvements made in the previous version of the manuscript and for his/her new suggestions in order to help improve the manuscript. Kindly find below our responses to the comments (quoted between []). We hope that our responses will address the last concerns and that the changes made will convince the reviewer about the potential of IASI to measure HNO3 in the dark Antarctic polar vortex.

Major comments

[In response to both reviews and the detailed comments by Gloria Manney and Michelle Santee, the authors have improved their manuscript considerably. However, I still have concerns about the correctness of their estimation on errors to be applicable within the dark Antarctic polar vortex. In this respect I do appreciate the addition of a comparison with MLS as new Figure 1. However, I don't understand why the authors have stopped half way. It would have only been a small step to show a picture comparing total column amounts integrated from MLS to those of IASI and I would strongly recommend to do this. It would just be easier to put forward and discuss these arguments when the column amounts derived from MLS are presented and quantitative differences shown. E.g. one could easily analyse how much the sensitivity of IASI applied to MLS vertical profiles would change the results. Since it is claimed that the IASI dataset is valuable for polar stratospheric studies as it is, one should do this, even when any tropospheric part would not be considered (in case of MLS). Such a comparison could also help in understanding the problems of the IASI retrievals e.g. over Eastern Antarctica. Further, the argument about "the non-negligible IASI sensitivity to HNO3 in the troposphere" is quite confusing, since all over the paper it is argued that the IASI HNO3 data well represent the stratospheric situation and that, e.g. a renitrification at lower levels cannot be captured well.]
We thank the referee for this suggestion, which was also made by referee #3. We now show in the revised version a picture comparing the total columns integrated from MLS vs IASI in three equivalent latitude bands (see Figure 1 here below). For the comparison, the vertical sensitivity of IASI has been taken into account by applying the FORLI-HNO_3 averaging kernels on the co-located MLS profile, which was first interpolated to the FORLI-HNO3 pressure grids and then converted into column profile. The MLS profile was also extended down to the surface by considering the FORLI a priori profile.

The cross-comparison with integrated columns from MLS is very favourable and gives further credit on the IASI total column observations during the polar night. The strong HNO_3 depletion is well captured by both IASI and MLS measurements with a perfect match for the onset of the depletion. Note that part of the differences between IASI and MLS are likely due to the different number of co-located data within the 2.5°x2.5° grid cells considered here for the comparison, with a much larger number of observations for IASI (through the quality filtering) than for MLS.

That new comparison figure between integrated column amounts from IASI vs MLS is now included in the revised Section 2 of the manuscript, and the text was adapted to:

"In order to expand on the comparisons against FTIR measurements which is not possible during the polar night, Fig. 2 (top panel) presents the time series of daily IASI total HNO_3 columns co-located with MLS measurements within 2.5x2.5 grid boxes, averaged in the 70°S–90°S equivalent latitude band. In order to account for the vertical sensitivity of IASI, the averaging kernels associated with each co-located IASI retrieved profiles were considered for this cross-comparison. The MLS profiles were first interpolated to the FORLI pressure grids, then converted into column profiles. They were also extended down to the surface by considering the FORLI-HNO3 a priori profile. Similar variations in the HNO_3 column are captured by the two instruments, with an excellent agreement in particular for the timing of the strong HNO_3 depletion within the inner vortex core. Note that a similar good agreement between the two satellite datasets is obtained in other latitude bands (see Fig. 2 bottom panel for the 50°S–70°S equivalent latitude band; the other bands are not shown)."

[L120-122: "…(Ronsmans et al., 2016). The total columns are associated with a total retrieval error ranging from around 3% at mid- and polar latitudes to 25% above cold Antarctic surface during winter (due to a weaker sensitivity above very cold surface with a DOFS of 0.95 and to an poor knowledge of the seasonally and wavenumber-dependent emissivity above ice surfaces which induces larger forward model errors)."
However, I think the error due to wavenumber-dependent emissivity has not been considered in the error estimation of 'Ronsmans et al., 2016'. So I wonder why only the 25% from that paper is reported here. Further, might there be also an error contribution due to cloud-clearing over very cold Antarctic surfaces?].
The referee is right, as specifically mentioned in Hurtmans et al., 2012 and in Ronsmans et al., 2016, the error due to wavenumber-dependent emissivity (a fixed parameter) is not directly taken into account in the total retrieval error estimation which only includes the smoothing error and the measurement error. The wavenumber-dependent emissivity introduces a bias that is especially used to filter out the contaminated spectra (based on the RMS and on the absolute bias of the residuals).

Nevertheless, if the surface emissivity is not taken into account in the total retrieval error calculation, it indirectly affects this error through compensation effects with the HNO_3 profile and the surface temperature that are part of the state vector (adjusted parameters). Hence, the mis-representation of the wavenumber-dependent emissivity is at least to some extent included in the HNO_3 total retrieval error calculation.

Response to reviewer #3

We thank the referee for his review and for reading the manuscript and our replies to the previous reviews with attention. We acknowledge in particular his request to go further in the comparison with MLS to "cross-validate" the IASI HNO3 product at polar latitudes. As detailed below the comparison is very favorable and this certainly strengthen the paper.

We address all other comments below on a point-by-point basis. We hope that with the changes made in the manuscript and the clarifications given below, the referee will consider that the paper can be published in ACP.

Comments

[L19: The IASI HNO3 nadir measurements can not be considered as having "good vertical sensitivity". Prospective data users need to know the vertical resolution of the measurements and that is conveyed by the standard practice of quoting the full width at half maximum (FWHM) of the averaging kernel. There is no reasonnot to do this for a nadir sounder e.g. Maddy and Barnett, Vertical Resolution Estimates in Version 5 ofAIRS Operational Retrievals, IEEE Trans. Geosci. Remote Sens., 46, 8, 2375-2384, 2008.**]**
We think that there may be a semantic misunderstanding related to this point. As explained in our previous responses to the referee and as stated in the manuscript, we here refer to "a good vertical sensitivity in the low and middle stratosphere", not to a good vertical resolution of the measurement.

The averaging kernels give the sensitivity of the retrieved value to the true profile with:
1/ the position of its peak indicating the altitude of maximum sensitivity
2/the FWHM giving an estimation of the vertical resolution (~30 km for HNO_3 from IASI; it is specifically why we consider a total column as mentioned at several places in the submitted version and in previous FORLI-HNO_3 studies). The FWHM is now indicated in Section 2 of the revised manuscript.

Looking at typical examples of averaging kernels (e.g. in Ronsmans et al., 2016 or provided in Figure 2 in our previous responses), it is clear that the maximum vertical sensitivity of IASI to the HNO_3 profile ranges in the mid-low stratosphere with values of 1 along the total columns averaging kernel in that region, indicating a good sensitivity of IASI to HNO_3 in that altitude range, even if the information coming from a specific level cannot be attributed by IASI/FORLI at that exact specific level due to the coarse resolution (FWHM of the averaging kernels of ~30 km) which forces us to consider a total column.

[L29: For reasons explained elsewhere the "formation temperature" can be better expressed as an "existence thresold"**]**
This is a good suggestion. It has been corrected, as suggested, in the introduction.

[**L26-29**: The averaged "drop temperature" disregards the considerable interannual variability in the early stage formation of different types of Antarctic PSCs and the role played by the exposure of liquid PSCs to low temperatures in the formation of NAT i.e. many studies have shown that NAT is not uniquely constrained to nucleate at TNAT and some supersaturation is generally needed leading to a lower temperature for NAT formation (as in fact you discuss in the text L55-L75). Therefore, stating that the drop temperature is "consistent with TNAT", which implies that PSCs are mainly NAT forming at TNAT, is invalid.]
Here, we just mention that, interestingly, the HNO3 drop temperature matches TNAT (for typical 50 hPa atmospheric conditions) for the purpose of the description of our results. We don't think that the

discussion on the different mechanisms of NAT formation, which are described later in the introduction, would be useful in the abstract.

However, in order to not give to the reader the feeling that "PSCs are mainly NAT forming at TNAT", "consistent with" has been replaced by "close to".

[**L28**: the" " sign should be deleted since a specific value and its uncertainty is quoted.]
Done

[L30: Some corresponding indication of the equivalent latitude range would be useful here]
'…in the region of potential vorticity lower than -10×10^{-5} K.m^{2}.kg^{-1}.s^{-1} **(similar to the 70° – 90° S Eqlat region during winter)**" has been added here.

[L30-31: The spatial distribution and inter-annual variability of the drop temperature are investigated and discussed in the context of previous PSCs studies. However, the study presented here does not include any observed data on PSCs and is therefore not a "PSC study"]
In fact it is not our intention to provide with this paper a PSC study and in the manuscript we acknowledge that: "the measured HNO_3 total column does not allow differentiating the uptake of HNO_3 by different types of PSC particles along the vertical profile". "In the context of previous PSCs studies" has been deleted to avoid misunderstanding early on the manuscript.

[L92-94: Please give the fullwidth half-max (FWHM) of the vertical response in km and not just the height of maxi-mum sensitivity.]
See our response to the first comment above. The FWHM is now mentioned in Section 2 of the revised manuscript.

[L251-255: The software bug that was fixed in the revised version has changed the drop temperatures such that the year with 202.8K (previously 190.6K) is a significant outlier since it lies 8K higher than the 10-year mean drop temperature and is almost as much above the assumed 50hPa TNAT (195K). Therefore it does not support the statement on L365-366 that the 10-year range "demonstrated the good consistency between the 50 hPa drop temperature and the PSCs formation temperatures in that altitude region".]
The year 2014, which is an outlier with the drop temperature of 202.8K, is now excluded from the analysis. The sentence has been rewritten as follows:
"Except for the year 2014, the 50 hPa drop temperatures are detected between 189.2 K and 198.6 K (194.1 K ± 2.8 K - 1σ standard deviation - on average over the 10 years, excluding 2014 that stands out with a drop temperature of 202.8 K)."

[L295-300: It is not clear why this data quality problem has not been addressed in the revised submission. Measurements that are known to be bad must be screened out.]
As specifically mentioned in Sections 2 and 4.2 of the manuscript, a series of quality flags were applied to filter out the poor retrievals. Nevertheless, there remains indeed a few poor quality retrievals above icy surface due to a misrepresentation of the seasonally and wavenumber-dependent emissivity above such surface. This parameter still remains critical and causes poorer retrievals that, in some instances, pass through the series of quality filters and could affect the drop temperature calculation. Developing a perfect filtering for these areas has not been possible at the moment. Such contaminated spectra should for now be treated with caution while a more appropriate flag could be developed.

[L332-337: Why is the discussion in L302-338 and L367-376 limited to nucleation of NAT and ice PSCs with no mention of STS? There is no nucleation barrier to STS formation and it generally forms in advance of ice nucleation except possibly under very fast cooling e.g. in mountain waves. STS is not even mentioned in the paper after the introduction in L55-72.]

In Section 4.2, we have simply considered the formation temperature of PSCs that first nucleate (typically NAT). This is why the averaged isocontour of 195 K is represented on figure 6. No distinction between the HNO_3 uptake by the different PSCs, nor specific mention of ice PSCs are made in this section.

In the paragraph L333-339, we concluded that: "… the overall **range in the drop 50 hPa temperature** for total HNO_3 inside the isocontour for the averaged temperature of 195 K, typically extends from ~187 K to ~195 K, which **falls within the range of PSCs nucleation temperature at 50 hPa**: **from** slightly below $\mathbf{T}_{NAT}$ **to** around 3-4 K below the ice frost point - $\mathbf{T}_{ice}$…", knowing that the formation temperature of STS is in between (~192 K) for typical 50 hPa atmospheric conditions.

The HNO_3 total column measured from IASI does not allow differentiating the uptake of HNO_3 by different types of PSCs along the vertical profile, as the referee points out and this is why such a discussion cannot be performed.

[The response does not address the specific case example of where IASI views HNO3 depleted higher layers that overlay lower enhanced layers. How does the IASI column HNO3 measurement change if the HNO3 is redistributed in the vertical coordinate by denitrification and renitrification? A further question would be how does downwelling of higher values of HNO3 affect the HNO3 column?]

See also above: we hope that it is clear from the replies that the coarse vertical resolution does not allow to capture such altitude-dependent processes. In the manuscript, we explicitly refer to this, e.g.

"… despite the lack of vertical resolution, which is recognized in the paper and which forces us to consider total HNO_3 columns, IASI is characterized by a good sensitivity to HNO_3 at specific levels, in particular, in the range between ~70 hPa to ~30 hPa in the southernmost latitude in winter" … "where the strong HNO_3 depletion occurs…". "Above the Antarctic, the altitude of maximum sensitivity … ~22 km in winter".

"The likely renitrification of the lowermost stratosphere (Braun et al., 2019; Lambert et al., 2012) where the HNO_3 concentrations and the IASI sensitivity to HNO_3 are lower (Ronsmans et al., 2016) cannot be inferred from the IASI measurements."

In an effort to specifically address this question and to quantify the potential impact of the likely renitrification of the lower stratosphere on the IASI total column, we should compare IASI measurements collocated with cases of renitrification at lower levels, identified from independent measurements of HNO_3 vertical profiles, to IASI observations that do not experience renitrification. It has not been investigated at this stage. Note that Braun et al. (2019) identified renitrification at the lowermost stratosphere, below ~12 km where the IASI sensitivity is the lowest.

[As a further example of the 2D potential, could IASI be used to image the HNO3 field to show depletion in the cold phases of mountains waves e.g. near the Palmer peninsula (similar to the wave structures seen in AIRS brightness temperatures) or is that defeated by the vertical integration caused by the poor vertical resolution?]

As explained in our previous responses to referee #3, Figure 7 of the revised manuscript shows the spatial distribution of the drop temperature inside a region enclosed by an isocontour PV of $-8x10^{-5}$ $K.m^2.kg^{-1}.s^{-1}$, which, hence, encircles a region larger than the inner vortex core (see Figures 5 and 6 of the revised manuscript). The drop temperatures much above the NAT formation temperature, which are mostly found outside the averaged isocontour PV of $-10x10^{-5}$ $K.m^2.kg^{-1}.s^{-1}$, do not correspond to high minima

(<-0.5 x10^{14} molec.cm^{-2}.d^{-2}) in the second derivative of HNO_3 total column with respect to time.

We cannot argue that it corresponds to the NAT belt of Höpfner et al. (2006) downstream of the Antarctic Peninsula, which was enclosed inside the region of the NAT threshold temperature; the highest drop temperatures from IASI are found on the contrary outside the isocontour of the NAT threshold temperature (see figure 7 of the revised manuscript). Comparing the distributions of drop temperatures from IASI with PSC information from CALIPSO/MIPAS remains difficult given the difference in spatial coverage and, most importantly, the highly variable distribution of PSC types and of the NAT belt, temporally (daily) and spatially (Höpfner et al., 2006; Lambert et al., 2012).

Note that Hoffmann et al. (2014; doi:10.5194/amt-7-4517-2014) has reported an intercomparison of stratospheric gravity wave observations of both AIRS and IASI instruments and "showed that AIRS and IASI provide a clear and consistent picture of the temporal development of individual gravity wave events" … "While AIRS has been used successfully in many previous gravity wave studies, IASI data are applied here for the first time for that purpose. Our study shows that gravity wave observations from different hyperspectral infrared sounders such as AIRS and IASI can be directly related to each other, if instrument-specific characteristics such as different noise levels and spatial resolution and sampling are carefully considered ».

["CALIOP measurements ... this goes beyond the goal of this paper, which is to demonstrate the capability of IASI to measure HNO3 columns that are relevant for stratospheric studies". That goal was largely achieved already by Ronsmans et al (2016) and published in Atmos. Meas. Tech. This paper is under review for Atmos. Chem. Phys. and should relate more to a science investigation rather than a technical description. The comparisons with MLS are a welcome improvement, but unfortunately fall short of the analysis I was expecting. Surely the tropospheric contribution of HNO3 to the IASI column is not all that much (you could estimate the effect to confirm). I expected the MLS profile to be integrated with the IASI response function for a more direct comparison. That would facilitate a quantitative interpretation of the differences in the variation of the column data from the two instruments.]
We thank the referee for this suggestion, which was also made by referee #1. We now show in the revised version a picture comparing the total columns integrated from MLS vs IASI in three equivalent latitude bands (see Figure 1 here below). For the comparison, the vertical sensitivity of IASI has been taken into account by applying the FORLI-HNO_3 averaging kernels on the co-located MLS profile, which was first interpolated to the FORLI-HNO3 pressure grids and then converted into column profile. The MLS profile was also extended down to the surface by considering the FORLI a priori profile.

The cross-comparison with integrated columns from MLS is very favourable and gives further credit on the IASI total column observations during the polar night. The strong HNO_3 depletion is well captured by both IASI and MLS measurements with a perfect match for the onset of the depletion. Note that part of the differences between IASI and MLS are likely due to the different number of co-located data within the 2.5°x2.5° grid cells considered here for the comparison, with a much larger number of observations for IASI (through the quality filtering) than for MLS.

That new comparison figure between integrated column amounts from IASI vs MLS is now included in the revised Section 2 of the manuscript, and the text was adapted to:

"In order to expand on the comparisons against FTIR measurements which is not possible during the polar night, Fig. 2 (top panel) presents the time series of daily IASI total HNO_3 columns co-located with MLS measurements within 2.5x2.5 grid boxes, averaged in the 70°S–90°S equivalent latitude band. In order to account for the vertical sensitivity of IASI, the averaging kernels associated with each co-located

IASI retrieved profiles were considered for this cross-comparison. The MLS profiles were first interpolated to the FORLI pressure grids, then converted into column profiles. They were also extended down to the surface by considering the FORLI-HNO3 a priori profile. Similar variations in the HNO_3 column are captured by the two instruments, with an excellent agreement in particular for the timing of the strong HNO_3 depletion within the inner vortex core. Note that a similar good agreement between the two satellite datasets is obtained in other latitude bands (see Fig. 2 bottom panel for the 50°S–70°S equivalent latitude band; the other bands are not shown)."

[I also wanted to see specific depleted vs non-depleted cases (one with a re-nitrification layer would be good also) generated along with the simulated IASI columns and the calculated columns. I suggest that the figure provided on the averaging kernels etc could be added to a supplemental material section with a description tailored to the cases studied here in addition to just referring readers to a prior publication.]
This is specifically what we have shown in the Figure 1 that we provided in our responses to the previous comments and that showed examples of vertical HNO_3 profiles retrieved within the dark Antarctic vortex (depleted case above Arrival Height) and outside the vortex (non-depleted case above Lauder). The retrieved profiles were provided along with their associated total retrieval error and averaging kernels.

As suggested, we now provide that figure in the manuscript. Note that we have found it better to include it in Section 2 than in a supplementary material.

A re-nitrification case can hardly been identified from IASI (see comment above).

[CALIOP PSC data (Pitts et al 2013, doi:10.5194/acp-13-2975-2013) have been used to show that different PSC types exist in different temperature regimes, with ice PSCs detected close to the frost point, STS follows the expected equilibrium curve and NAT exhibits two preferred mode below the NAT existence temperature. The analysis presented here is not constrained by the simultaneous presence of known PSC types and in fact there may not even be any PSCs in the atmospheric path sampled. Therefore, it is too simplistic to compare the drop temperatures to TNAT. The proximity of the 10-year mean drop temperatures to TNAT does not constitute a validation as is claimed here. Individual years could be expected to show a variation in drop temperature because of interannual atmospheric differences. For instance, the years domimated by STS should necessarily show lower drop temperature than years dominated by NAT. The highest drop temperatures are far above PSC temperatures (e.g. 202.8K at 50hPa in one particular year) and deserve more scrutiny and should be investigated thoroughly. Interannual comparisons of the drop temperature may benefit from using (T-Tice) as the temperature coordinate (rather than absolute temperature) as this removes variations due to changes in H2O partial pressure (see Fig 2 of Pitts et (2013)). There is a fundamental problem with making an assessment of the potential future scientific utility of the drop temperatures when they have only been evaluated in the absence of knowledge of the different types of PSCs present.]
We are not sure to follow the referee well:

1/ As mentioned above, we do not aim at a PSC study, given that: "the measured HNO_3 total column does not allow differentiating the uptake of HNO_3 by different types of PSC particles along the vertical profile", as mentioned in the manuscript. So indeed, we have simply considered the formation temperature of PSCs that first nucleate, i.e. NAT. This is why the threshold temperature of 195 K is considered in the study to identify likely PSCs-containing regions. We don't have the possibility to perform a more thorough investigation of the NAT existence. However it is true that years dominated by STS should induce lower drop temperatures than years dominated by NAT and this has been now specifically mentioned in the revised manuscript when discussing the variability observed in the 50 hPa drop temperatures: "The range observed in the 50 hPa drop temperature could reflect the preponderance by one type of PSCs over another." But here again, the influence of the different types of PSCs on the drop temperature that is calculated from the HNO_3 total column can hardly be investigated.

2/ As discussed in a comment above, the year 2014 with the drop temperature of 202.8K is standing out and is now excluded from the analysis.

3/ We agree that working in the T – Tice coordinate system, as in Fig.2 of Pitts et al. (2013), may be interesting for illustrating theoretical equilibrium uptake of HNO3 by STS and NAT as it removes variations due to differences in atmospheric pressure level (illustrated at 30 hPa et 50hPa in Pitts et al. (2013)). In our case, it has no influence at all on the temperature timeseries (see Figure 2 below). Tice considered here is determined as 188 K by Murphy and Koop (2005) for typical 50 hPa atmospheric conditions only. Hence we suggest to keep the absolute temperature in Fig.4 of the revised manuscript.

4/ The concept of "drop temperature" is exploited for the first time in this study; it allows relating the strong decrease in HNO_3 to the likely existence of PSCs. We believe that the concept could be used in future studies by nadir or limb sounders.

[The response does not address the specific case of whether there are differences in bias and uncertainty for depleted and non-depleted conditions.]
We think that the referee may have missed that in our responses and in the submitted manuscript. The total retrieval error values outside vs inside the polar regions during winter, i.e. during depleted-conditions, were in fact provided in Section 2 (line 129-135). We report a larger error (25%) due to a weaker sensitivity (lower DOFS) above very cold surface and to an poor knowledge of the seasonally and wavenumber-dependent emissivity above ice surfaces. When compared to ground-based FTIR measurements (whatever the latitudes and seasons; see Ronsmans et al., 2016), IASI is always positively biased. At Arrival Heights, the bias is not larger than at the other stations and it is not larger for depleted than for non-depleted conditions. For the polar stations, a bias lower than 12% is calculated over the altitude range where the IASI sensitivity is the largest.

[Thick ice PSCs have been detected by AIRS, TOVS HIRS2 and AVHRR (see Stajner et al. and refs therein,https://doi.org/10.1029/2007GL029415). Do these have an effect on the HNO3 retrieved by IASI?]
No. From that paper, thick ice PSCs are detected on the AIRS moisture channel at 6.79-μm (i.e. 1473 cm-1), while in the atmospheric window region used for the HNO_3 retrieval from IASI: "Comparisons of AIRS spectra with a radiative transfer model in the window region 10-12.5 µm show signatures of near-micron sized cirrus ice particles [Kahn et al., 2003]."

[Even after all the quality controls are applied there are apparently still cases with poor retrievals that could be removed.]
Please see our response to the same comment related to L295-300 above.

[What is the fraction of data that is affected by surface emissivity?]
This is difficult to estimate since such data pass through the filtering criteria that include bias and RMS of the residuals specifically used to flag the remaining cloudy scenes or the surfaces with sharp emissivity variations at 11.5 µm. These emissivity features are suspected to explain the few hotspots in HNO_3 observed above Antarctica (likely due to compensation effect in order to reduce as much as possible the residual during the iterative phases of the retrieval), and similarly, the high extremes in the drop temperatures found above eastern Antarctica over some years.

[The conclusion of the paper is that ability to monitor the polar atmosphere over several decades with current and planed IASI instruments “will provide an unprecedented long-term dataset of HNO3 total columns”. The drop temperature is defined as the 50hPa temperature corresponding to the greatest rate of decline of the column HNO3 with respect to time. However, even with a record now extending over a decade the scientific utility of this dataset has not been demonstrated].

See our previous responses. The concept of “drop temperature” is exploited for the first time in this study and while it does obviously not inform on the detailed formation mechanisms of PSCs from HNO3, it provides a robust indication on the occurrence of some polar processes at play in the stratosphere. There is an increasing interest and use of the IASI data products for “climate” studies (i.e. through so-called “thematic climatic data records”). We are confident that the HNO3 dataset will contribute to these data records in the near-future and benefit several modelling studies.

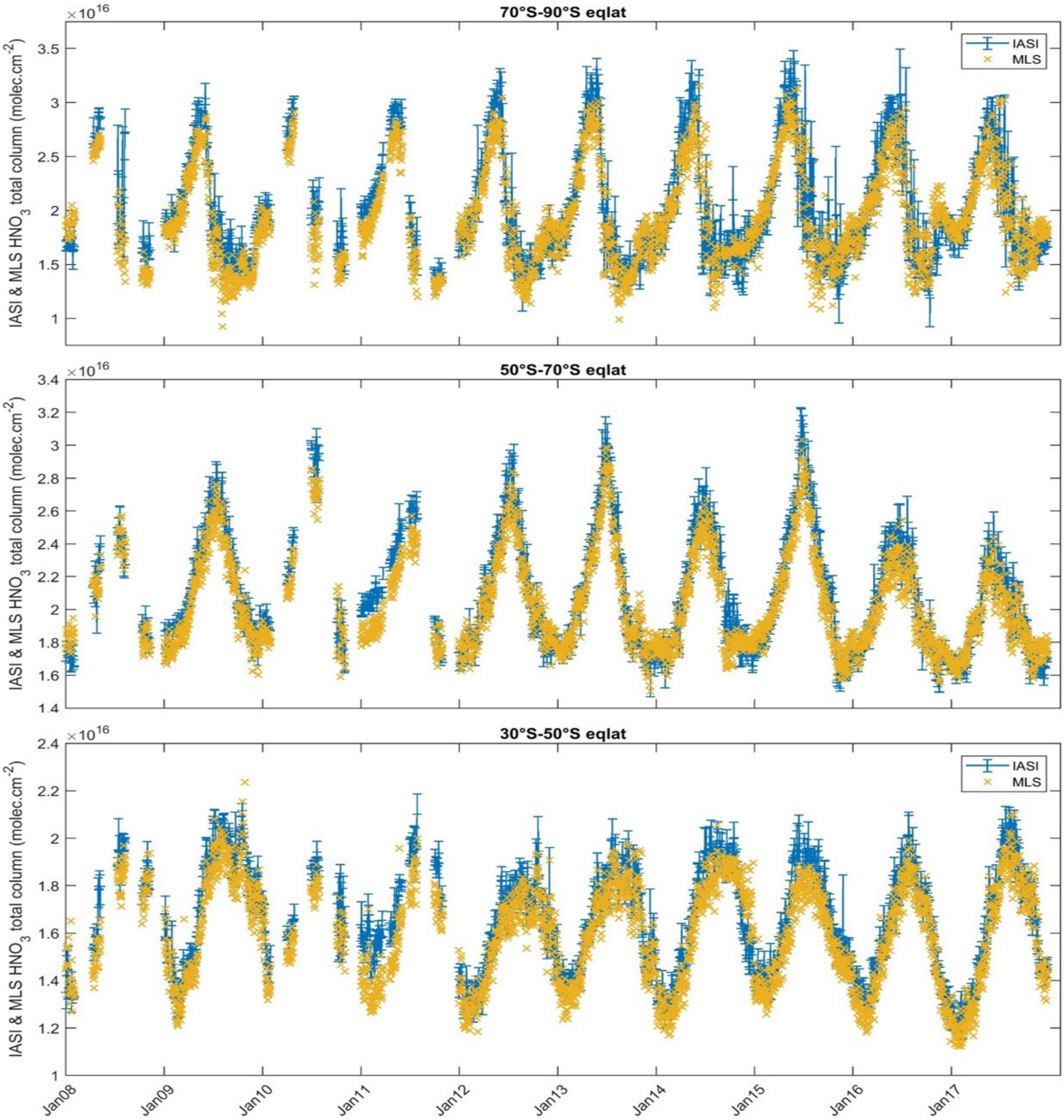

Figure 1. Time series of daily IASI total HNO_3 column (blue) co-located with MLS and of MLS total HNO_3 columns (orange) within 2.5x2.5 grid boxes, averaged in the 70°S–90°S (top panel), the 50°S–70°S (middle panel) and the 30°S–50°S (bottom panel) equivalent latitude bands. The error bars (blue) represents 3σ, where σ is the standard deviation around the IASI total HNO_3 daily average.

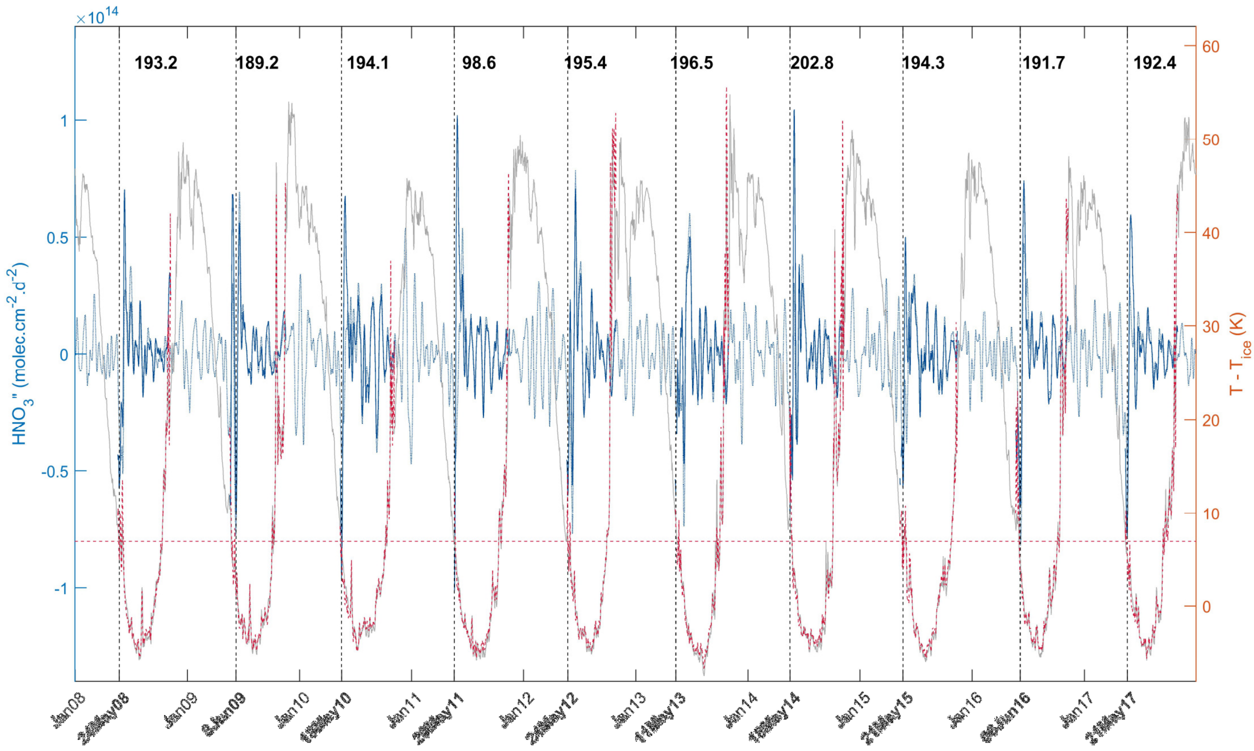

Figure 2. Time series of total HNO_3 second derivative (blue, left y-axis) and of the temperature at 50 hPa (red, right y-axis) in the $T - T_{ice}$ coordinate system (where T_{ice} is the frost point temperature determined by Murphy and Koop (2005)), in the region of potential vorticity lower than -10 x10^{-5} $K.m^2.kg^{-1}.s^{-1}$. The red horizontal line corresponds to the 195 K temperature. The vertical dashed lines indicate the second derivative minimum in HNO_3 for each year. The corresponding dates (in bold, on the x-axis) and temperatures are also indicated. The time series of total HNO_3 second derivative (dashed blue) and of temperature at 50 hPa (grey) in the70–90°S Eqlat band are also represented.