We would like to thank the anonymous reviewer for his comments. We believe they help us to improve the manuscript and give us some matters to improve the mission. Here below are our answers. A corrected version of the paper has been uploaded as a supplement.

Please note that we found an error in Table 1. The IF range is 10.975-18.975 GHz and not 10.075-18.075 GHz.

The reviewer comments are in blue, the citations from the original manuscript are in italic and the manuscript modifications are in red. The answer "Done" simply means that the manuscript has been modified following exactly the reviewer comment.

1) I think the authors have somewhat glossed over the trade inherent in such measurements between precision and resolution (vertical resolution mainly in this case). They can afford not to dwell on it in this study because they've chosen a rather coarse 5km spacing for their state vector resolution, but I do think it deserves some mention (but probably not any new calculations). At no point do they discuss averaging kernels, but I imagine they've computed them, or could do so very easily. I expect that such kernels indicate that the information content is a good match for their 5 km grid (i.e., that A is a good approximation to the identity matrix, except perhaps at the top and the bottom of the profiles). I would however like them to add a discussion to that effect somewhere in the text. Their mention of the potential of finer 3km resolution for temperature (page 17, line 7) implies that they have looked into this issue somewhat. I encourage them to talk about it just a bit more.

A weak regularization is used for inverting the forward model Jacobian matrix which gives unity averaging kernels. The vertical resolution is then given by the resolution of the retrieval altitude grid. We have chosen to use the same resolution of 5 km for every product over the whole altitude range in order to simplify the discussion. The resolution corresponds to the antenna field of view resolution (i.e., the vertical resolution of the measured radiance). However proper retrieval algorithms will consider the best resolution for each product considering the trade-off between vertical resolution and precision. The manuscript has been modified to clarify these points as follows:

Page 12, Line 12:
"The retrieval altitudes range … *The ~~retrieval~~ grid vertical resolution is 5~\unit{km} that corresponds to the effective* vertical *field-of-view of the instrument*"

Page 13, Line 4:
"… *Ux is a diagonal matrix for stabilizing the matrix inversion. Its element square-roots correspond to very large standard deviations* (STD) *of x, typically > 10000%, 1000 K, 1000 m/s for VMR, temperature and LOS wind, respectively. The regularization effects are negligible where ~~the measurement is relevant~~* the retrieval errors (null space and measurement errors) are much smaller than the *Ux* related STD. In other words, the averaging kernels are unity at altitudes where the measurement is relevant and the retrieval vertical resolution is that of the retrieval altitude grid. "

Page 15, Line 4:
The precision (1-σ) is given for ~~a retrieval vertical resolution of 5~\unit{km} and~~ for a single-scan and a vertical resolution of 5 km ~~and for a single scan~~. It is possible to use the altitude information inscribes in the pressure broadened lineshape, for retrieving atmospheric profile with a better resolution but at the cost of the precision. Precision degradation can be afford for products retrieved from strong signals (e.g., \chem{O_3} or temperature) or for those whose the vertical resolution is more scientifically relevant than the temporal or horizontal one (precision can be

improved by averaging data). On the other hand, degrading the vertical resolution may be necessary for providing useful information on products derived from weak signals (e.g, \chem{HOCl}). Later, using the results of this study and based on scientific requirements, the retrieval algorithm will be optimized for providing the best compromise between precision and resolution for each of the main products.
…~~*Also the retrieval vertical resolution can be increased for improving the precision of species with weak lines.*~~

2) The authors make no mention of frequency stability requirements for the instrument Local Oscillator (presumably tied to some lower frequency clock source). I presume the instrument (or spacecraft) design includes some suitable source, possibly tied to GPS signals. If is better than 1 part in 10ˆ9, then I think it's OK to ignore it, otherwise it should probably be investigated for its impact on wind accuracy. Either way, it should probably be discussed. Measuring lines on either side of the LO can significantly reduce sensitivity to that term (at the expense of wind precision), but if the measurement approach relies on that it should certainly be discussed.

The LO stability is indeed a key parameter when measuring the winds. We should have discussed this issue in the paper and we have included a discussion in Pages 13-14 based on the instrument design described in the proposal.
Here are some additional information not included in the corrected manuscript:
In the instrument design report, it is stated that "a highly stable 10 MHz TCXO (Rakon RPT7050) is used as frequency reference to the PLL circuit. This TCXO has a long term stability (aging) of only ±1 ppm/year (>0.5 MHz). The VCO used is HMC529LP5 from Analog Devices. The output frequency of the LO unit is 13.293 GHz."
A short term (24 hours) LO stability of 2 kHz (df/f = 3*10^-9) has been required, it corresponds to a wind error of 1 m/s. The instrument team considers that such a performance is challenging and they guaranty a stability of 10 kHz. However discussions with the instrument team are still going on and we expect to improve the performances for both long and short timescales using different hardware or connecting SIW to the Innosat Spacecraft bus clock.

Table 2 (P13) has been modified to include short-term (24 hours) and long-term (1 year) local oscillator frequency variability: 2-10 kHz and >0.5 MHz, respectively.

Page 14, Line 4. The paragraph has been rewritten in order to include a discussion on the LO frequency uncertainty:
Systematic retrieval errors emerge from uncertainties on the instrument, calibration and forward model parameters, and LOS angles (Tab. 2). ~~*The most critical parameters are investigated using a perturbation method:*~~
It is difficult at this stage of the mission definition to provide proper values for these uncertainties. *The* given *values are relatively close to those expected but rounded in the way that it will be straight-forward to linearly scale the retrieval errors according to any future better knowledge of the parameter uncertainties. One may notice that the uncertainty on the line broadening parameter (Gi) is likely underestimated and the actual values should be between 1–4 % depending on the line. On the other hand, the calibration parameters* ~~*and sideband ratio*~~ *are likely overestimated. Anyway these errors induce a relatively constant retrieval bias that could be mitigated with ad-hoc corrections if their properties are well understood, e.g., time scale and latitudinal variabilities* (see for example the JEM/SMILES data analysis in Baron et al. (2013b)).

The 24-hours variability of the local oscillator frequency is between 2 and 10 kHz which straightly results to a LOS wind retrieval uncertainty of 1–5 m/s. The lower limit corresponds to the scientific

requirement and the upper one is the worse acceptable case. Though it is a systematic error, it changes from one scan to another with a time correlation that has to be determined before launch. The impacts on other retrieved parameters are negligible. The 1-year frequency variability may be relatively large (>0.5 MHz or 250 m/s) and we should consider that an absolute frequency knowledge, good enough for retrieving winds, may not be available. The frequency calibration will be performed using short-term wind retrieval bias estimates within 40–60 km where other systematic errors are small.
Retrieval errors from other parameters are investigated in Sect. 5.2 using a perturbation method:
EQ17 ...

And in the conclusion (P22,L5):
Hence ad-hoc methods for reducing retrieval biases must be studied. These methods can be used to calibrate the LO frequency long-term trend that may arise with the proposed hardware. However, improvements of the instrument design for following the frequency trend with a precision better than 2 kHz, are still being investigated.

Specific comments

Page 1

Line 2: "... platform, with a launch planned for near 2022. It is …"
Done

Line 6: "... perpendicular directions in order to reconstruct …"
Done

Line 7: Consider putting commas before and after "near 655 GHz"? Also add "amount" of" between "small" and "wind"
Done

Line 10: First word "the" -> "a"
Done

Line 17: ".... parameters and for study of methods …"
Done

Page 2

Line 7: First word "of" -> "in"
Done

Sentence spanning lines 7, 8, and 9: I'd turn this sentence around: "Some important species, such as HO2 and ClO, have their clearest signals in this region of the spectrum (refs.)" or something similar.
Done

Line 11: "... and measurements are not perturbed by ..."
Done

Line 16: "... have difficulties in reproducing it where ..."
Done

Sentence spanning lines 20-22: Again, I'd turn this around: "As climate and weather models increase their vertical range to encompass more of the stratosphere and mesosphere, the need for measurements to improve the accuracy of models in this region, and hence at lower altitudes, can be expected to rise", or something like that.
Done

Page 3

Discussion in first paragraph: Would be good to mention the WINDII and HRDI instruments and UARS. Was the information they provided not useful for your purpose, or at least some aspects of your purpose? Even though it was a while ago, were there not some questions that those instruments answered?

Both HDRI and WINDII are described in Shepherd et al. (2015). However it is true that HRDI should be explicitly cited since it is the only spaceborne instrument designed measuring wind in the stratosphere and mesosphere (WINDII measured winds above 90 km). The manuscript has been changed has follow:

Page 2, line 21-22:
~~*Though there is a strong need for middle-atmospheric wind measurements to validate and constrain the models, only high altitude (>90 km) wind measurements with optical sensors currently exist on a global scale (Shepherd, 2015).*~~ Only High Resolution Doppler Imager (HRDI) on Upper Atmospheric Research Satellite (1993-2005) has been able to measure horizontal winds over the stratosphere and mesosphere (Ortland et al., 1996), and current spaceborne sensors are not able to measure wind accurately below 90 km (Shepherd, 2015).

Page 4

Lines 5-10: If you're nodding the spacecraft, presumably the rotation axis of that nod is along the flight direction. Does that not give the two tangent points a non-vertical locus?

Is the choice to alternate the two views between the up and down scans intended to make them more vertical? If so it would be good to mention that explicitly.
The tangent height foot-print is not vertical. It moves along the orbit track with the satellite (7 km/s) and toward the satellite along the LOS in the ascending scan and away from the satellite during the descending scan. The nodding movement is performed over a small angle range, the deviation of the tangent points wrt to the vertical is small.

The text (P4,L6) has been modified as follow:
… *upward and downward from about 15 to 90 km.* The forward antenna is used during the upward scans and the aftward one during the downward scans. With this choice, the horizontal displacement of the tangent point during a vertical scan is less than 300 km, the vertical motion of the line-of-sight partly counterbalancing the satellite motion. *Using the line-of-sight* (LOS) *winds retrieved with the two antennas over close regions ~~will~~ allows us to derive the meridional and zonal wind components (Appendix A)*. The separation between the LOS wind profiles is less than 400 km.

P4,L10:
The following sentence is removed:
"The forward antenna is used during the upward scans and the aftward one during the downward scans."

Page 5

Table 1: 1 MHz for the spectrometer resolution seems a bit on the coarse side to me, given the upper stratosphere / mesosphere target. Have studies been performed to see if finer resolution (e.g., some "zoomed in" lower bandwidth spectrometers on selected lines) might not improve the wind measurements?

The paper shows simulations based on the proposal status but the instrument design optimizations are still investigated such as the possibility to have different frequency resolution. A spectral resolution of 0.5 MHz for key mesospheric lines would improve the wind retrieval precision by more than 20% above 70 km (e.g., Fig. 6 in Baron (2013a)). Temperature retrieval should also be improved but further simulations have to be conducted to quantitatively assess the improvements.

The 200 MHz frequency range between IF = 17.2 and 17.4 GHz (LO=638.075 GHz) is the range that should be selected in priority. It contains the strong H_2O line (620.701 GHz) and the two strongest O_3 lines (620.825 and 655.289 GHz).
The second priority would be to increase of the resolution for the NO lines (651.1 GHz).
In order to compensate the telemetry data increase, the resolution could be decreased in other frequency range such as in the spectral window near the N_2O stratospheric line (652.8 GHz).

and we will not discuss this point in the main text. However this potential improvement is added in the conclusion:
P22L18: "*...could improve the measurement sensitivity by more than 20 %*. Retrievals could also be improved in the mesosphere by increasing the frequency resolution to 0.5 MHz between the intermediate frequencies 17.2 and 17.4 GHz, a range that contains the strong H_2O line (620.701 GHz) and the two strongest O_3 lines (620.825 and 655.289 GHz). Implementing such a setting is under investigation."

Page 9

Line 24: If it's not too difficult, it would be nice to quantify "small" (e.g., of order 10 cm/s?)

The vertical width of the intensity weighting function is about 1 km. (WF are defined for a single ray before antenna convolution). At the equator, a horizontal wind of 100 m/s induces an error is 7 cm/s for a LOS point at 1km above the tangent point.
The error is given as (Ve + Vlos)*(1-sin(phi_i)) with Ve the Earth rotation speed along the LOS (<370 m/s) and Vlos is the horizontal LOS wind and phi_i the angle between the nadir direction and the LOS.

The manuscript is changed has follows:
P9, L24: *These errors are ~~small~~* smaller than 10 cm/s *and have negligible impacts on the retrievals*.

Page 11

Line 21: "AURA/MLS" -> "Aura MLS"
Done as well as replacing AURA with Aura in other places.

Line 27: You cite Figure 2, but that figure shows the coverage for the SIW orbit, not the Aura orbit. I don't see the need for a second figure, so perhaps it's simpler just to remove the citation of Figure 2 here?

The black dashed lines show the Aura SZA vs Latitude for January.

Page 12

Line 13: "that corresponds to" -> ", corresponding to"
Done

Page 13

Line 4: I completely understand your dropping the non-diagonal terms in Sy, but it seems a shame after you went to such lengths to compute them long hand. Given the power of computers these days, is it still too much work to compute the full matrix inverse, at least once, and see what difference it makes? I guess it is rather large, so probably not. In which case, why to go such lengths to take up space in the earlier sections defining it? It might simply be easier to tell us up front that you plan to ignore those terms and explain why that's OK, rather than exposing the reader to the full algebra only to discard it.

The correlations are not neglected. They are taken into account in Eq. 16 since the full matrix Sy is used. The diagonal matrix *Sdy* is only used in the inversion of K as a measurement weight. The manuscript is changed to make this point clearer:
P13L10: "… *the standard deviation of x,* Sy is the full measurement error covariance matrix (Eq. 14) *and ...*"

The reason to not invert the full matrix is because it is too large (~8000 frequencies and 150 tangent heights) to be done with the computer used for this analysis. In the future, we will optimize the computations in order to reduce the matrix size and use sparse matrix algorithms for the inversion. In theory (i.e, if the frequency correlations are properly characterized), the retrieval errors will slightly be decreased.

Page 17

Line 18: "First, we note that, except for O3 and H2O, all ..."
Done

Line 30: Actually, ClO can be non-zero at night in some cases.
We agree and the statement has been softened: "… *but vanish* in general *during nighttime.*"

Page 18
Line 24: Comma needed after "On the other hand"

Done