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.

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) Although primarily focusing on wind retrievals the manuscript does not address the question of local oscillator stability. However, for a wind error below 1 m/s an oscillator stability of df/f<3.3e-9 is needed. As the operation geometry does not allow observations in opposite viewing directions as done by other wind measurement techniques this stability needs to be long-term (i.e. for the entire mission lifetime) in order not to introduce trend artifacts. What are the expected frequency stability during heating/cooling of the satellite? What is the aging of the planned frequency source? Are there plans for a method to monitor the LO frequency? Please comment on this issue in the manuscript. Even if very high stability could be achieved and and the induced bias was marginal, an upper limit should be indicated. I assume it is one of the questions a reader has when reading about Doppler shift measurements.

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 month) 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)).

Given the proposed design of the instrument (Murtagh, 2016), 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):
~~*The need for deveoloping ad-hoc methods ...*~~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.

Methods for mitigating wind retrieval bias have to be defined. Looking at opposite directions such as it is done with a ground based instrument, is difficult in space (e.g., solar illumination issue) and not be efficient (the two opposite measurements will be 2000-3000 km away from each other). Other methods more likely based on daily zonal statistics have to defined. For instance we may use the fact that systematic errors lead to zonal-wind retrieval errors with opposite sign on the ascending and descending orbit branches.
This issue has been added in Appendix A:
...
A systematic error *e_los* on the LOS wind retrievals propagates to the *U* and *V* components as follows:
1. The systematic error on the zonal-wind estimate is *e_u=e_los (cos(alpha_n) + sin(alpha_n))*
2. The systematic error on the meridional-wind estimate is *e_v=e_los (cos(alpha_n)-sin(alpha_n))*

We assume that *e_los* does not depends on the LOS orientation which is a valid assumption for the errors investigated in this paper (LO frequency, calibration, spectroscopy). We should note that *e_v* = 0 for *phi_n* = 45 deg or 225 deg which occur at latitudes between 30N–50N on the ascending branch of the orbit and between 10N–30N on the descending branch. The cases *e_u = 0* occur for the retrievals at the lowest and highest latitudes.
At the equator, the bias on the meridional wind is partly canceled out and the bias correction method used for JEM/SMILES analysis may not be satisfactory. For instance, an error *e_los* = 1 m/s induces an error *e_v=0.2* m/s. On the other hand, the error on the zonal component is 1.4 m/s with an opposite sign on the ascending and descending orbit branches. The sign difference may provide us with a way to characterize LOS wind retrieval systematic errors.

2) You suggest a sun-synchronous orbit crossing the equatorial ascending node at 18:00. I agree that this allows perfect conditions for wind observations by allowing to constantly observe the night side with the higher ozone concentrations. My concern about this choice is that the representativeness of the measurements of trace gases near the day-night terminator may be delicate as during this period the concentrations of photochemically active species undergo rapid changes. This may among others introduce an articial annual cycle to your measurements simply by modifying the time before (after) the sunrise and after (before) the sunset the observations are made. Can you quantify this effect and what are your ideas to mitigate it?

Unfortunately the choice of the LT ascending node is fixed by the launcher and, in order to keep low Innosat mission budget, the choice is limited. This being said, as explained by the reviewer, wind and temperature retrieval performances are better in nighttime conditions but reactive species should be measured both in day and night times as for Aura MLS or MIPAS.
There is a compromise to be done. For this mission, we have chosen the most favorable conditions for wind measurements which are very challenging. Also, flying near the terminator provides favorable conditions in term thermal stability and solar-panel illumination.

The analysis of the species with diurnal variations (meso-O3, ClO, HO2, NO) will be performed with photo-chemical models. We will benefit from all the studies and methods implemented for previous missions like Odin or ACE/FTS. Most of the observations will be performed at SZA (+/- 10-20 deg from the terminator) when abundance changes will be slower than at sunrise or sunset.

Can you quantify this effect and what are your ideas to mitigate it? This is a very broad topic that cannot be addressed here. Each molecule, altitude, latitude and season should be treated as a special case.

Specific comments

Page 1, L1: Why is your instrument called \Stratospheric Inferred Winds" when effectively assessing the upper stratosphere and lower mesosphere?

Siw is a chacracter of the Swedish mythology. The acronym put the focus on the stratosphere and wind. Though winds are only measured over about half of the stratosphere (30–50 km), the mission is primary a stratospheric mission. Middle and upper stratospheric winds are a key product of the mission as explained in the paper. The mission is also able to provide a rather comprehensive description of the full stratosphere including high precision temperature and O3 measurements as well as good measurements of important species for studying its chemistry, dynamics and radiative budget (H2O, HCl, N2O, HNO3, ClO or HCN).

Good measurements of the mesosphere will also be performed but the instrument design is not optimized for such altitudes. A spectral resolution of 0.5 MHz would have been better as well as having an additional spectral channel with a strong line for improving the temperature retrieval performances, temperature being a key parameter for inverting molecular lines.

p.2, l.5: “risk of an observation gap in the near future" Is there no citation for this statement?

We are not aware of a refereed paper discussing this issue. However the following presentation at a recent SPARC meeting is available on the Web. We have added its reference in the manuscript.

Livesey, N. J. and Santee, M. L.: Prospects for future spaceborne measurements of interest to the SPARC DA Community and how to improve those prospects, in: S-RIP 2017 and 13th SPARC-DA Workshop, https://events.oma.be/indico/event/18/material/slides/16.pdf, 2017.

p.2, l.20: You could maybe add a citation of a modeler stating that wind simulations in these regions are hard to obtain.

The references given in Line 20 (Baron et 2013, Pichon et al., 2015….), though they focus on the measurements, clearly shows difficulties of analysis and re-analyses to reproduce wind in the mesosphere. Because of the lack of measurements, most of the GCM wind evaluations have been performed for altitudes below 10~hPa. For instance, the difficulties for a GCM to reproduce Equatorial wind at 10 hPa has recently been discussed in Kawatani et al., (2016). A key dynamical feature of the upper part of the middle-atmosphere is the vertically propagating tides. Using temperature data, Sakazaki et al. (2018) found significant tide signature differences between the latest re-analyses and measurements of in the upper stratosphere and lower mesosphere. We have added these references as:

Modeling middle-atmospheric major dynamical phenomena such as vertically propagating tidal waves, *high-latitude sudden stratospheric warming or equatorial quasi-biennial oscillation are still challenging (Limpasuvan et al., 2012; Newman et al., 2016; Orsolini et al., 2017;* Sakazaki et al., 2018*). Wind … cannot be described by the geostrophic approximation such as in the equatorial region where the Coriolis force is weak and, in the upper stratosphere and mesosphere where waves and tides ~~phenomena~~ tend to dominate the wind fields (Baron, 2013, LePichon, 2015,* Kawatani et al., 2016*….*

Kawatani, Y., Hamilton, K., Miyazaki, K., Fujiwara, M., and Anstey, J. A.: Representation of the tropical stratospheric zonal wind in global atmospheric reanalyses, Atmospheric Chemistry and Physics, 16, 6681–6699, doi:10.5194/acp-16-6681-2016, https://www.atmos-chem-phys.net/16/6681/2016/, 2016.

Sakazaki, T., Fujiwara, M., and Shiotani, M.: Representation of solar tides in the stratosphere and lower mesosphere in state-of-the-art reanalyses and in satellite observations, Atmospheric Chemistry and Physics, 18, 1437–1456, doi:10.5194/acp-18-1437-2018, https://www.atmos-chem-phys.net/18/1437/2018/, 2018.

p.2, l.24: does -> do
Done
p.2, l.25/27: The measurement approach presented in Baumgarten 2010 is also providing wind in the gap region. Please add citation at l.27.
Done

p.2, l.30: Rather 20 or 30 km?
Using the Rayleigh channel (signal backscattered by molecules), LOS wind can be retrieved up to about ~30 km with a precision better than 5 m/s and a resolution of 2 km.

Do you see Aeolus as a good complement to your mission if you get synchronous mission activity? Please comment on this.
In case of overlap between both missions, there is an obvious complementary since Aeolus targets wind below 30 km and SIW targets higher altitudes. However, in the altitude range between 20-35 km, the performances of both missions are weak. Also, Aeolus measures only the wind component along a single line-of-sight. These information are given in the paper and further discussions about the complementary of both missions are out of the scope of this paper.

Using Aeolus data together with SIW for scientific studies will be investigated though Aeolus lifetime is officially between 2018-2021. More generally, combining lidar and micro-wave or infra-red passive sensors should be the solution in the future for measuring winds from the surface to the lower thermosphere.

p.3, l.1: Please clarify that the wind profiles published in Wu et al. 2008 do not cover the gap region you defined as 30-70 km.

The sentence has been rephrased as follows:

The potential of MM/SMM limb sounders for measuring winds has been demonstrated with LOS wind retrievals between 70–90 km from MLS O2 line (Wu et al., 2008) and between 30–80 km from O3 and HCl lines measured with Superconducting Submillimeter-Wave Limb-Emission Sounder (SMILES) (Baron et al., 2013b).

p.3, l.9: Please indicate the expected lifetime of SIW. Is there a chance to have it observing at the same time as SMILES-2? Would there be an added-value if both missions would be observing synchronously or would the expected higher performance of SMILES-2 make SIW obsolete?

Given the uncertainties on SMILES-2, having SIW is a very good thing. SIW lifetime is planned to be 2 years in order to allow the launch of a new Innosat mission every two years. If SMILES-2 is selected this year, the launch should be near 2025. In this case, SMILES-2 will follow SIW without overlaping time.

However, beside the budget consideration, SIW hardware lifetime could be longer than 10 years (e.g., as for MLS and SMR instruments) and a time expansion of the mission may then be possible. Having an overlap with SMILES-2 will benefit to both missions, and the quality of the scientific outcomes will be improved. For instance, the spatial and local time samplings are different and complementary. The fixed local times of SIW measurements will help to characterize non-diurnal changes in the SMILES-2 dataset such as non-migrating tide effects on temperature and wind. A long-term database can be produced with SIW that can not be done with SMILES2 whose the lifetime will be shorter than 5 years due to the limited lifetime of the cryo-cooler.
In term of data processing, we will share problems and information related to the 655 GHz band that has never been measured before (spectroscopic data, retrieval strategy, ...). Collaborations between both teams already exist and they will be strengthened if SMILES-2 is launched. This will lead to more efficiencies for defining, implementing and validating the processing chains.

The sentence p.3, l.9 has been rephrased as follows:
… a*nd SIW has been selected for the 2nd launch near 2022.* It will observe the middle-atmosphere (15–90 km) for a period expected to be at least 2 years, and will provide *horizontal-wind vector within 30–90 km.*

Sect. 2.1: Please extend the instrument description. It is clear that this is not an instrument paper, but some core characteristics of the receiver should be introduced here. This will moreover avoid that questions arise during the further read of the manuscript.

In this is manuscript, we want to focus only on the main instrument characteristics that are relevant for the simulations. We do not want to go too deep in the description of hardware details. Moreover some of them may still be modified for optimizing the performances. We do not wish to add more details but, if the reviewer believes that an important information is missing, we will be happy to add it.

P.4, l.6: With your scanning scheme LOS winds at 45 and 135 deg will be recorded from a similar location only for 1 altitude of your scan. How large will the distance between this two components be at maximum? Is this sampling mismatch not critical for your calculations of one zonal and meridional wind profile with Eq. (A2)?

The maximum distance between 2 scans is less than 400 km. This is equivalent to the LOS horizontal resolution. No significant errors should be induced by the position mismatch.
The manuscript has been changed has follows (the modifications also includes reviewer 2 comment answer):

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 allows us to derive the meridional and zonal wind components (Appendix A). The separation between the LOS wind profiles is less than 400 km.

p.4, l.7: "continuously rotate" is misleading as it is in fact not an unaccelerated rotation but rather a succession of upward and downward scans.
I think this was an issue corrected after the quick review process. In the current version, the text is:
"*… the whole satellite will nod up and down in order to scan the limb alternatively upward and downward from about 15 to 90 km*"

p.4, l.28: "at least a factor of 2... compared to other spectral regions." Please be more concrete. What are "other spectral regions".
The sentence has been rephrased as follows:
… a factor 2 the wind measurement sensitivity between 40–70 km compared ~~to other spectral regions~~ to retrievals performed from a band with similar characteristics but located at any other frequency under 800 GHz.

Fig. 3: You display 9 GHz but the spectrometer bandwidth is 8 GHz. Please mark the (un)used frequency range in this figure.
The ranges outside the spectral bandwidth are now indicated with grey-shaded areas. The caption has been updated accordingly.

Fig. 3: What is the reason for the 2 GHz frequency shift compared to SMILES-2 (Ochiai et al 2017)?
There is no frequency difference with the band shown in Ochiai et al. (2017). In Ochiai et al., the band is displayed differently: each sideband is divided in two ranges of 4 GHz.

Fig. 3: Displaying the centre frequecy of LSB and USB directly in the different panels would help to further clarify the figure.
The central frequency is now indicated in the plot x-labels.

p.5, l.7: I suggest to modify "so-called brightness temperature" to "so-called Rayleigh-Jeans brightness temperature" to make sure the user is not confused by the 1 mK of cosmic background (as I was at first).

Done

p.6, l.6: i → nu_i
Done

Fig. 4: It is not completely clear from the text what you intend to communicate to the reader with this figure. Please extend the description and reasoning in the text.
The text P6 l.7 is changed as follows:
and I is the specific intensity. The specific intensity is integrated along a LOS as that shown in Fig. 4. The LOS is characterized by the altitude of the tangent point (i=0), the angle with the meridional direction *phi_n* and narrow ranges *i* over which the atmosphere is considered homogeneous.

The index "*i*" (frequency) in Eq (3) is replaced with "k" to avoid confusion with the LOS range index used in the figure.

Eq. (2): Why do you use quadratic addition of the static antenna pattern and the broadening due to the scanning velocity? I would argue that, if you combine the static pattern with the scanning, your beam pattern becomes non-Gaussian. In any case, I think that by quadratic addition you drastically underestimate the width of your main lobe unless the scanning broadening is much smaller than the static beam width. If not the case, I would think that a linear addition would already be closer to reality while still underestimating the width of your beam (due to the non-Gaussicity introduced by the scanning). Please review the information about the beam width in the manuscript.

We agree with the reviewer that considering the effective antenna pattern as a Gaussian is an approximation. But it is a satisfactory approximation for a sensitivity study. The static antenna vertical width is about 5 km which is significantly larger than the altitude range scanned during the spectrum integration (1.1 km). Moreover the retrieval vertical resolution is 5 km, so such an approximation has no impacts on the results. Antenna side-lobes have also small impacts on the retrieval errors estimation and can be neglected.
The text is changed in order to clarify these points:

Given that the altitude range scanned during the spectrum integration is small compared to the static antenna vertical resolution (1.1 km and 5 km, respectively), the effective antenna pattern including the vertical scan, *is approximated by a Gaussian function* …
The antenna sidelobes are also neglected. These approximations have negligible impacts on this study.

p.8, l.14: Why are you using JPL for some lines and Hitran for others?

The JPL lines used in this work are not in HITRAN. The text is changed as follows:
… that are not available in HITRAN and are, then, taken from the Jet Propulsion Laboratory catalog (Pickett et al., 2018).

p.8, l.16/17: You could indicate worst-case bias induced by these effects to show how marginal they are.
There is a lack of information for the pressure shift parameters. In HITRAN 2016, only values for HCl and H2O lines are available. At 10 hPa (~30 km), the H2O line shift is 230 kHz and an error of 2% corresponds to a wind error of ~2 m/s. This value should be smaller for O3 lines.

The text is changed as follows:
The line frequency is also shifted by pressure but this effect is small above 25 km where winds are measured. For the H_2O line at 620 GHz, 2% error on the shift parameter corresponds to an error of 2 m/s at 10 hPa. The shift on O_3 lines should be smaller but the information is not available in HITRAN and further studies are needed to infer it.

p.10, l.1: You refer to Tso as antenna spillover. Looking at Eg (8) Tso rather refers to the average brightness temperature of the regions where the radiation which you receive because of the spillover in your optics actually comes from. Please adapt the wording.
The text has been rephrased as follows:
T_{so} is the mean brightness-temperature introduced by the optics spillover, ...

p.10, l.2/l.15: "spill-over" or "spillover". Please use consistent spelling
The term "spillover" is now used instead of "spill-over"

Eq. (10): You may state that you assume $T_c = 0$ here.
We changed the text as follows:
Assuming a linear response of the radiometer and using $T_c \ll T_h$, the radiometer gain is derived

Eq. (15): Does this linear approach suffice for all situation you expect to encounter? What happens if the truth is further away from the first guess than in Fig. 6? Is this linear retrieval also sensible for photochemically active species close to the day/night terminator?

The retrieval approach presented here is good enough for estimating the retrieval errors with respect to the atmospheric state. This is a very common approach used for other mission studies such as MLS, Odin and SMILES. The definition of a robust retrieval algorithm is not needed and it is not discussed in this paper. Let's note that methods to handle non-linear effects that can arise for cases with large differences between the first guess and the true atmosphere, exist. For instance, the linear scheme presented in the paper can be integrated into a standard Levenberg-Marquardt iterative scheme (e.g., see Urban et al. 2004 and Baron et al. 2011 given in the paper). For species such as ClO, N_2O, HOCl the problem is nearly linear even for large differences and a linear approach should provide good performances even near the terminator. The main issue in this case is possible large horizontal inhomogeneities near the tangent point. But such cases will be rare (see 2nd main comment answer).

Eq. (15): You use Sdy instead of Sy. What about error correlations? Can they be neglected and why? Please state it in text.

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 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.

We changed the text as follow to make it clearer:
P13L10: "*… the standard deviation of x*, Sy is the full measurement error covariance matrix (Eq. 14) *and ...*"

p.13, l.15: Please add the reason for the increasing error at lower altitudes here.

lower altitude for wind retrieval. The error increase is due to the pressure broadening of the lines that is about 20–40 MHz at 10 hPa.

p.14, l.19: I suggest to refer to Fi as “centre frequency" instead of just “frequency".
Done (as well as in Table 2 and A1 captions)

p.13, l.20: How do you retrieve the elevation offset?

The elevation offset is one of the retrieval parameter in x. This is explained at the beginning of the section (P12,l10):
“*The retrieved state x_hat is a vector including all the unknown parameters of the forward model, namely the atmospheric vertical profiles, a radiance offset on each spectrum and a mean pointing angle offset of the whole scan.*”

Since it is a standard approach, we do not think that we need to provide more details.

p.16, l.10: Please indicate the reason why the best performance is found over the northern polar regions.

The mesospheric wind measurement performances are the best over the night-time poles because of the O3 enhancement. The text is changed as follows:
“*The best performances are found over the northern polar region where* the nighttime \chem{O_3} enhancement is the largest. There, the *LOS-wind can be retrieved with a precision better than...*”

Sect. 5.2: Why using a 10 times to large error for the sideband ratio? Indeed a 1% sideband uncertainty seems rather large. Please consider to modify it to the value of 0.1% that you found in your preliminary study. The choice of 1% uncertainty which you then qualify several times as too large unnecessarily complicates the reading of this section.

We changed the manuscript to consider an error of 0.1% on the DSB parameter. The x-scales of Fig. 11 and 12 have been changed accordingly as well as the DSB error discussions in sections 5.2.1 5.2.2 and conclusion (P22,L1).

p.18, l.15: have -> Has
Done

p.18, l.16: overlap over each other → overlap each other
Done

p.21, l.7: unusual -> Unusually
Done

p.22, l.28: Grammatically incorrect sentence
The sentense has been rephrased as follows:
“The retrieval of two line-of-sight winds over the same region allows us *to compute …* ”

p.23, l.1: alpha_n → phi_n
Done

Eq. (B1): What is the significance of “max" here? eps_(x,M) seems to be a scalar (see Eq. (18)) so I don't see what you want to do by taking the maximum.

This term eps_(x,M) means the set of the errors induced by all the parameters of all the lines of a given species M. In the revised version, it is replaced by *{eps_(x,M,pi)}_pi* and the text is rephrased as:

“where M denotes the chemical species, *eps_(x,M,pi)* is the error induced by the parameter *p* of the line *i* (Eq. 18), and *{eps_(x,M, pi)}_pi* is the set of errors induced by all the parameters of all the lines of the species M.”