Reviewer report: P. Baron et al. 2018

General comments

The paper describes a simulation study for a future InnoSat instrument. It focusses on wind, temperature and trace gas concentration observations in the middle-atmosphere a region where such space-borne observations are missing (wind) or expected to be lacking in near future due to the end of lifetime of other satellites (trace gases). The study is therefore of high scientific relevance and perfectly suited to the scope of AMT. The research lies on solid grounds and is generally well presented in the manuscript. A few modifications pending I suggest publication in AMT. My most important questions are the following:

- Although primarily focusing on wind retrievals the manuscript does not adress the question of local oscillator stability. However, for a wind error below 1 m/s an oscillator stability of $\Delta\nu/\nu < 3.3 \cdot 10^{-9}$ is needed. As the operation geometry does not allow observations in opposite viewing directions as done by other wind measurement techtiques this stability needs to be long-term (i.e. for the entire mission lifetime) in order not to introduce trend artefacts. 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 first questions a reader has when reading about Doppler shift measurements.

- 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 artificial 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?

Specific comments

p.1, l.1: Why is your instrument called "Stratospheric Inferred Winds" when effectively assessing the upper stratosphere and lower mesosphere?

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

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

p.2, l.24: does → do

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.

p.2, l.30: Rather 20 or 30 km? Do you see Aeolus as a good complement to your mission if you get synchronous mission activity? Please comment on this.

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.

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?

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.

p.4, l.6: With your scanning scheme LOS winds at 45 and 134° 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)?

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.

p.4, l.28: "at least a factor of 2... compared to other spectral regions." Please be more concrete. What are "other spectral regions".

Fig. 3: You display 9 GHz but the spectrometer bandwidth is 8 GHz. Please mark the (un)used frequency range in this figure.

Fig. 3: What is the reason for the 2 GHz frequency shift compared to SMILES-2 (Ochiai et al 2017)?

Fig. 3: Displaying the centre frequecy of LSB and USB directly in the different panels would help to further clarify the figure.

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).

p.6, l.6: $i \rightarrow \nu_i$

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.

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.

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

p.6, l.16/17: You could indicate worst-case bias induced by these effects to show how marginal they are.

p.10, l.1: You refer to T_{so} as antenna spillover. Looking at Eg (8) T_{so} 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.

p.10, l.2/l.15: "spill-over" or "spillover". Please use consistent spelling

Eq. (10): You may state that you assume $T_c = 0$ here.

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?

Eq. (15): You use $S_{d,y}$ instead of S_y. What about error correlations? Can they be neglected and why? Pleas state in text.

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

p.14, l.19: I suggest to refer to F_i as "centre frequency" instead of just "frequency".

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

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

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.

p.18, l.15: have → has

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

p.21, l.7: unusual → unusually

p.22, l.28: Gramatically incorrect sentence

p.23, l.1: $\alpha_n \rightarrow \phi_n$

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