Geosci. Instrum. Method. Data Syst. Discuss.,
https://doi.org/10.5194/gi-2017-50-AC1, 2018

Geoscientific Instrumentation Methods and Data Systems Discussions
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Interactive comment on "Mars sub-millimeter sensor on micro-satellite: sensor feasibility study" *by* Richard Larsson et al.

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Received and published: 23 October 2018

1 General changes

The changes to the paper between this and previous version are substantial. The major changes are as listed

- The error analysis has been redone completely based on overarching comments from both reviewers. This has been done mainly to be able to discuss what both reviewers mentioned as important, namely the trade off between precision and resolution in an added subsection at the end of section 3. This discussion is not exhaustive but we think it addresses the reviewers' concerns about the the

aforementioned trade off.

- The forward simulations have been changed by using available GCM model atmospheres and updating the Zeeman effect spectroscopy. The former was done to better represent cases with a changed correlation distance (or scale length) in the a priori covariance matrix required to discuss mentioned trade off. The latter is a more recent development where the main author found a small (a few percent) error in the Zeeman splitting for the used molecular oxygen line.

Other smaller changes have also been incorporated in the current version.

Below in red are the reviewers' comments one-by-one as copy-pasted from the original response files provided by Copernicus. The one alteration that the main author has done to these comments is to add appropriate LaTeX-coding for references to variables in equations.

Before going over the detailed response, we wish to thank the reviewers because their suggestions have improved this work substantially.

Attached is the updated paper.

2 Response to reviewer 1

2.1 General comments

This manuscript describes an effort to evaluate the potential scientific contributions of a planned submillimeter sounder making limb and nadir observations of Mars' atmosphere from a Martian orbit. While the instrument may indeed make useful and needed contributions to the state of knowledge, I'm afraid this paper in its current form does *not convey those potential contributions at all well, and it needs further work before it is in a useful state to truly convey the information needed.*

We have updated the manuscript as a response to this comment as detailed below.

The authors have overlooked a fundamental issue inherrent in the formulation of atmospheric remote sounding instruments, in the algorithms used to retrieve vertical profiles of atmospheric properties from the raw radiance measurements, and in the scientific interpretation of those profiles. The issue is that it there is an underlying tradeoff between precision and resolution. That is to say, one can always improve the precision of a measurement at the expense of coarser vertical resolution. The factors that affect that trade are varied and have different impacts. One factor that the paper touches on is the choice of the 10-km length scale in the a priori covariance matrix (page 5, line 20). Increasing this to, say, 20-km will reduce the vertical resolution of the result while improving the precision. For a limb-scanning instrument, implementing a slower vertical limb scan can improve both precision and vertical resolution but, because the instrument is moving, this 3 results in a worsening in effective horizontal resolution. Developing a lower-noise submillimeter wave receiver can lead to improvements in all properties.

We agree that there is a trade off between resolution and precision that was ignored in the last iteration. The current iteration should sufficiently explain this by updating both method and results to give room for such a discussion. We also agree that it important to keep developing newer and better receiver technology, and if these were available within our mass/power/cost/political budgets, we would be using them. It is beyond this work to develop new receivers.

We have updated the paper to detail what a change in length scale / correlation distance does to the retrieval errors at the end of the results section. We have also updated the paper to use somewhat less arbitrarily chosen variances. Both of these combined means the results are changed somewhat. While updating the discussion

about correlation distances, we also changed the atmospheric scenario to be more inline with GCMs. This means our magnetic field errors are greatly increased in this version.

We still keep the same integration times as well as system noise temperature in the updated calculations. We add at the end of the error analysis subsection, a discussion parroting the reviewer's point that there is a trade-off between spatial/temporal resolution and precision.

The authors seem to be unaware of this trade, or at least do not discuss it in the paper. By its very nature, this trade means that it is meaningless to discuss the precision of the measurements (Figures 5, 6 and 7) without describing the resolution at the same time. On a related note, the authors fail to describe their instrument in enough detail to properly convey other aspects of this trade. The integration time is quoted as 1s, but nowhere is it stated how many limb tangent altitudes are observed and the vertical range they cover. For this paper to be suitable for publication, these key absences from the discussion must be addressed.

It was an oversight to not clearly state that we are using the same scanning altitudes as in the figure showing the signal at said altitudes. Relevant parts of the text has been updated to reflect this.

The trade off between resolution and precision is now discussed as part of the correlation distance, and mentioned elsewhere in the text where deemed appropriate.

Related to this is the authors dwelling on "detection limits". What do they mean by that? Is it meant to be the precision in a single profile retrieval? If so, it is a poor choice of terminology for it, as that "limit" can be reduced by, for example, averaging together observations over multiple days to improve the precision (at the expense of poorer temporal resolution - another example of a precision/resolution trade). I would avoid the use of detection limit, as it is not an accurate description of the way such measurements work. They are characterized (in the traditional but perhaps simplistic *view) by precision, resolution, and accuracy. Using the term detection limit implies there is some minimum abundance of a molecule in the atmosphere that is needed to somehow trigger a useful measurement. The reality however is that, for example, if the single-profile precision of the instrument is 0.1 ppmv for a species, it doesn't matter whether the true atmospheric abundance is 0 ppmv or 10 ppmv, the "noise" in the result is the same.*

We have removed discussions of detection limits. While we still think single profile detection are important (since this is interesting for short timescales), as the reviewer notes, this kind of limit is anyways implied by the retrieval errors so introducing unclear terms is not desired.

We still keep the reduced atmospheric gases case in the paper because the VMR "noise" is nonlinear as the opacity gets too high for the variable species.

A secondary weakness of this paper is that, while claiming to follow the formalism of Rodgers 2000 (and others who take that approach), the authors have applied aspects of that formalism in a rather bizarre way, while ignoring other important aspects of it. Specifically, the authors make no explicit reference to the Averaging Kernels that are key to the interpretation of such measurements (and provide the resolution information so sorely needed as described above). While their approach is not a debilitating weakness as such, it does speak of a lack of understanding by the authors that it would be better to remedy if the paper is to be viewed credibly by the community.

We have made the implicit references to the averaging kernel more explicit.

In updating their manuscript, I would suggest that the authors view the earlier paper on this topic by Urban and colleagues [Urban, J., K. Dassas, F. Forget, and P. Ricaud (2005), Retrieval of vertical constituents and temperature profiles from passive submillimeter wave limb observations of the Martian atmosphere: a feasibility study, Appl Optics, 44(12), 2438-2455, doi:10.1364/AO.44.002438] to be a good example of the approach that properly uses the Rodgers framework, and includes a recognition of

precision/resolution trades.

It was an oversight to not cite Urban et al.'s work in previous iteration. This has been corrected.

My final general observations is that the standard of English in this paper is rather poor. I have tried to indicate some specific suggestions for improvement, in the spirit of improving the manuscript that eventually emerges from the authors. However, I suspect that, in the long run, it would greatly benefit from a careful scrubbing by a copy editor.

2.2 Specific points

Abstract line 1: "We are planning" is vague. What state of readiness is this mission concept in? Is this just a proposal or a planned-proposal or is it a confirmed mission? If it's in the formulation stage, is there a specific funding agency and/or mission opportunity being targeted.

The state of readiness now is that components are being built. The building of key parts of the platform and of the instrument is funded. It is not a confirmed mission, however, as we are still trying to find a partner to piggy-back to space with. It is likely, also, that the first iteration of this mission will attempt landing with the same instrument as we describe. The text has been updated to say the instrument is being built.

Line 2/3: "The sensor will measure ++emission from++ atmospheric ... peroxide ++in order++ to retrieve... and ++the++ changes ++therein++ over time".

Fixed. Thanks!

Line 4: "levels of success" is rather un-scientific. I suggest "with various degrees of precision and resolution"

Adopted. Thanks!

Lines 5-9: Why not summarize the wind results also.

Done. Thanks!

Line 9: "... the vertical resolution clearly suffers". I don't know how you can say that as the narrative makes no effort to quantify the vertical resolution as it stands

Removed. Thanks!

Line 11: "We are in the works to" - as with the abstract, this is very vague, please clarify along the lines discussed above.

Done. Thanks!

Line 16: You fail to describe why the fact that the radiation is passively emitted is an advantage. Are you comparing to some active (e.g., radar, lidar?) sensor, or are you comparing to solar occultation (with its sparse coverage) or observations of sunlight scattered from the limb (limited to daytime conditions only).

We added that the measurements are "independent of the local time", which, e.g., solar occultation measurements are not, as the reviewer points out.

Line 19: "have" → "has"

Fixed. Thanks!

âĂŤ- Page 2

Line 3: Comma needed before "but" and an "are" needed after it.

Fixed. Thanks!

Line 4: "oxidize" → "oxidizes"

Fixed. Thanks!

Line 5/6: This sentence is very poorly worded. I suggest something like "Sandel et al. (2015) showed that a non-constant oxygen mixing ratio profile is needed to explain

solar occultation observations above 90km", assuming that's actually what their paper stated.

Updated. Their work does not show variation. It shows the VMR is higher at higher altitudes than at lower altitudes. Our intention was to point out that at some altitude range between ground and 90 km, the ratio are increasing, while measurements closer to ground are more similar. The GCM model we get VMR from does not have this increase, and does not reproduce ground measurements either. Thanks!

Line 7: "We will be able to see..." should be "The instrument we describe here will be able to see..."

Updated. Thanks!

Line 11/12: This is where one should talk about precision vs. resolution trades.

We have reduced the complexity of the retrieval simulation further by removing any correlation distances from the base simulations. In a short note on precision versus resolution at the end we bring this back into the fold, but we do not think it warrants mentioning in the introduction so have left the text unchanged.

Section 2.2: As discussed above, you need to give more information on the instrument scan. Over what vertical range is the tangent point scanned? What is the vertical spacing of the limb views and how long does the vertical scan take. How far does the tangent point move horizontally during that scan (a measure of the effective horizontal resolution).

We have added this to the results section. Thanks!

âĂŤ- Page 4

Line 15: See the earlier discussion on the term "detection limit". Also, I completely fail to see why the simulation with 100x less O2 is desired or even valid. Why focus on such an unrealistic case? I guess if the system is linear enough then the true amount *doesn't matter, but then, still, why pick an unrealistic value.*

Term removed. To clarify, we never used 100X times less O_2, this was an unfortunate formulation. The retrieval system is not linear for the other species but our statements were to broad regarding how we set up the simulations (it should have read "trace gases bar O_2" before, and the updated text takes this into account). Thanks!

âĂŤ- Page 5

Section 2.4

As discussed above, the authors have taken rather a bizarre route to performing their analysis. While equation 1 is valid in itself, their discussion of it, and their "changing e_y to Ju" (which is related to the computation of the averaging kernel, though they do not describe it that way) is out of family with the traditional approach. The authors make no effort to explain what is meant by "the response of the retrieved parameter to the system". I take it to be the factor plotted in the right hand part of each panel in figures 5-7, and assume it's the area under each averaging kernel row, but I'm not sure. As stated above, this discussion needs to be updated to make proper reference to vertical resolution (and/or degrees of freedom for signal), ideally in the context of their averaging kernels.

We now define the averaging kernel. We also add degrees of freedom to the plots. We further define the measurement response and what we consider a 'good' response. Thanks!

Also, the authors seem to be computing the precision on Level 2 products through some kind of Monte Carlo approach (at least that's what the Figure 5 caption states, although incomplete information is given, what distribution is assumed for e_y, for example). I have no idea why the authors took that approach rather than simply computing the covariance of the precision directly using the standard approach (see equation 3.19 of the Rodgers book, page 50). This is readily derived from their equation 1. Further,

the dotted lines in Figures 5-7 are completely meaningless, as they simply represent one realization of the distribution whose standard deviation is (ignoring vertical resolution issues) given by $s_x/\sqrt{1000}$. If they'd chosen 1,000,000 realizations instead the dotted line would be closer still to zero, but what does that tell us.

Yes, we realize the equation multiple times. The dotted lines have been removed. Thanks!

Line 17: Clarify "assuming independence". I think you mean independence between the various families of terms here (wind, temperature, vmr etc.), not within the terms themselves (as you do have a vertical covariance within each species, as described on line âĹij20).

Yes. Thanks!

Line 18: Insert "radiance" between "diagonal" and "error"

Done. Thanks!

âĂŤ- Page 6

Line 2-3: Again, restructure this to talk about the averaging kernels instead.

The subsection is changed greatly at this point. Hopefully the reviewer agrees it is improved. Due to this, language corrections below are left without a response from us, while we apply the terminological suggestions.

Line 5: Suggest you change "error retrievals" to "precision estimation" or similar.

We change to use therms like "error estimations" or "expected errors" instead of "error retrievals".

Line 25: "is" $\rightarrow$ "are"

Line 26: Please quantify "much longer"

We only discuss changing integration time for O_2 now, and we quantify the time necessary for achieving the discussed 'targets'.

Line 28: "or expect" → "to avoid"

âĂŤ- Page 8:

Figure 5 (and 6 and 7): As discussed, replace (or augment) the right hand part of each figure with averaging kernels and/or their full width at half maximum. Dispense with the dotted lines, which convey no useful information.

Done. Thanks!

Line 1 (1st line below caption): Please define what is meant by "The lower limit of detection for water [vapor not gas]"?

Water should read gaseous water throughout the text now.

Line 2: Where am I supposed to get the 20 ppbv number from in these figures? Is this what you're trying to do with the dotted line? I fail to follow. Even if the dotted line meant anything useful (which it doesn't) are you seriously asking the reader to see it having a 20 ppbv value anywhere, when the ticks on the x-axis are 5 ppmv! Again, is it really a lower limit? A monthly zonal mean would be able to pick out the abundance with far better precision.

We agree this was bad. The numbers we present in the current version are hopefully clearer to see now.

Line 3: Define "better" is this in absolute vmr or some kind of fractional sense?

âĂŤ- Page 10

Line 6: This is the first time you've mention degrees of freedom, please introduce it properly.

This is added to the error analysis subsection.

Line 7: The discussion of a 5m/s vertical wind again implies to me that the authors

do not understand that precision is not the same as "detection limit". That said, I agree with their assessment that vertical wind (which I presume is much slower than horizontal) will not be measureable from nadir.

Please also note the supplement to this comment:
https://www.geosci-instrum-method-data-syst-discuss.net/gi-2017-50/gi-2017-50-AC1-supplement.pdf

Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., https://doi.org/10.5194/gi-2017-50, 2017.