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Geoscientific Instrumentation Methods and Data Systems Discussions

Interactive comment on "Mars sub-millimeter sensor on micro-satellite: sensor feasibility study" *by* Richard Larsson et al.

Anonymous Referee #1

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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.

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

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Discussion paper

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.

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.

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.

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.

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.

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.

==== Specific points

—- Page 1

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.

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

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

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

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

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

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).

Line 19: "have" -> "has"

—- Page 2

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

Line 4: "oxidize" -> "oxidizes"

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.

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

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

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).

—- 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.

—- 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.

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 ex-

ample). 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.

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 ∼20).

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

—- Page 6

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

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

Line 25: "is" -> "are"

Line 26: Please quantify "much longer"

Line 28: "or expect" -> "to avoid"

—- 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.

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

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.

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

—- Page 10

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

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.

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