

Interactive comment on “Tomographic reconstruction of atmospheric gravity wave parameters from airglow observations” by Rui Song et al.

Anonymous Referee #1

Received and published: 28 April 2017

The paper "Tomographic reconstruction..." by R. Song et al. describes a new variant of a tomographic retrieval which is custom tailored to a not-yet existing measurement system planned to be operated in a novel measurement mode. As a methodical pre-flight-study the scope of the paper fits well in AMT but a couple of details should be clarified prior to publication. In general, the paper is well written, well organized, scientifically sound, and as far as I can judge, all relevant literature has been referenced.

Scientific issues:

p2 l30: I am not quite sure if the term "oversampling" is adequate here, at least in the context of MIPAS (although MIPAS is not explicitly mentioned here, the Carlotti et al. reference hints at MIPAS). Von Clarmann et al., Atmos. Meas. Techn., Vol. 2(1), 47-54,

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2009, "The horizontal resolution of MIPAS", find that the horizontal resolution of MIPAS in terms of along-track information smearing is better than the horizontal sampling in terms of the horizontal spacing of measurement geo-locations. Thus there is undersampling, not oversampling. Either reword, or define clearly in which sense you use the admittedly ambiguous term "oversampling". Does it refer to the retrieval space or the measurement space, etc?

p5 l5 and p8 l15: On page 5 the atmosphere is assumed to be so opaque that any signal from the lower atmosphere and surface can be ignored. On page 8 the atmosphere is so optically thin that no self-absorption has to be considered. These two approximations seem to be in conflict with each other, except if the transmission jumps from 1 to 0 at the target air volume. I do not doubt that the approximations made can somehow be justified but I think that a little more discussion is needed to refute the apparent contradiction. Particularly in the sub-limb mode I expect that you either get considerable signal from altitudes below the target volume, or that the atmosphere in front/above the target volume is not really transparent.

p6, general comment: No statement is made how well the measurement geometry is known, and in consequence, how well the pressures at the tangent points are known. The measured signal does not only depend on temperature but also on the number of molecules along the ray-path (or more precisely: the transparent and semi-transparent part of the ray-path). How is this multi-variable problem solved? Or is the tacit assumption made that the actual measurement geometry and pressure distribution are perfectly known? The manuscript should be a bit clearer with respect to this.

Wording and presentation issues and technical corrections:

Abstract: About a third of the abstract are like an introduction. I would prefer an abstract which includes less general introductory information but more methodical information and/or results.

p1 l10: There is no "2-dimensional atmospheric state". Better say "allow for tomo-

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graphic 2-dimensional reconstruction of the atmospheric state".

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p1 I11: "As no real data are available" sounds too defensive to my ears. It is fully legitimate to present pre-flight studies and retrieval sensitivity studies in AMT. Why not simply "The feasibility of this tomographic retrieval approach is assessed using simulated measurements".

p1 I12: "much smaller" than what?

p1 I20: comma after "(GWs)"

p2 I14 "they include": not quite clear what "they" refers to. I suggest to reword.

p3 I2 "limited-angle tomography". This sounds as if it was a technical term but I have never heard it before. Please either define this technical term, or avoid it and use generic terms instead.

p3 I3/4: The technical term "target mode" is used here twice but defined only in line 18. I suggest to write "In Section 2 we present the observation strategy which we call 'target mode' observations. Then it is clear that you give this name to something new and the reader will not wonder if he/she has missed anything.

p3 I 16: Please define the term "sub-limb sounding".

p3 I 23: The description of the geometry is somewhat unclear. You talk about "limb sounding", not "limb imaging". This implies that during one limb scan the viewing geometry is changing all the time. It is thus not clear how multiple consecutive profiles can be obtained while a limb view is kept. The text would be much clearer if you distinguish between limb scanning (usually used as a synonym to limb sounding) or limb imaging (recording of multiple ray-paths at the same time) is used. The statement "The instrument will keep the limb view" currently has three possible different meanings: 1. A series of measurements is made using the SAME tangent altitude. 2. A profile of limb radiances is measured simultaneously with a 1D imaging device. 3. Tangent altitudes change while the limb is scanned, and the statement is just meant to tell me that

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it is not switched from the limb-scanning mode to the sub-limb mode. Please clarify. Perhaps use a weaker wording than "will keep this limb view"; perhaps say "The instrument will continue to measure under limb geometry for a period of time"; and finally clarify what type of "vertical profiles" are measured. I guess "vertical radiance profiles".

p5 l18: Is A1 really a "transmission probability" or do you mean a "transition probability"

p6 l2: Is the A1 here the same as in Eq 3: Please use different symbols for different designates, and/or use the same technical terms for the same designates.

p8 Eq 13: Since Sa^{-1} is not the inverse of a covariance matrix but a freely defined regularization term, I find it inadequate to use the symbol Sa here, which is usually applied only for probabilistic a priori covariance matrices.

p9 l2: Not clear what "It" refers to. The content suggests that it refers to the entire regularization procedure but grammatically it refers to the a priori data alone, which does NOT ensure that a unique solution can be obtained. I suggest: "The second term in the cost function (Eq. 13) ensures that..."

p9 l5: references to Levenberg, Marquardt, and the implementation actually used would be appropriate.

p9 l11: you may wish to add the term "unstable solution" here

p10 Eq 16: If you used the convention that $f' = K$, then the equation would be easier to recognize (and, of course, define K prior to its use).

p10 l20: For the non-specialist it would be helpful to clarify if you perturb the temperature field only or if you adjust pressure (and with this also absolute concentrations of species) hydrostatically.

p14 l17: Has the acronym LOS already been defined? With respect to the "poor horizontal resolution", see my comment on p2 l30: At least for MIPAS it is the horizontal sampling and not the resolution of the measurement itself which is limiting the horizon-

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tal resolution of the data product.

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Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2017-118, 2017.

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