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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: Impact of nonlinearity on changing the a prioriof trace gas profiles estimates from the Tropospheric Emission Spectrometer (TES)" *by* S. S. Kulawik et al.

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General comments:

This paper presents to my best knowledge the first thorough documentation of the retroactive exchange of *a priori* information in a set of retrievals without rerunning the nonlinear retrievals. This allows to constrain each single retrieval by its individual *a priori* and to use the *a priori* transformation to obtain a uniform dataset where artefacts due to changing *a priori* information are excluded. The authors conclude that for their TES retrievals the use of individual *a priori* information for each nonlinear retrieval

and subsequent transformation behaves well compared to nonlinear retrievals using uniform *a priori* information. This paper is scientifically sound, well organized and written and fits well in ACP, of which one of four activity fields is remote sensing. The paper certainly has methodical focus but it is far more than a 'technical note'. The paper fulfils, to my judgement, every requirement of a regular journal paper. Thus, I recommend publication as a regular paper in ACP.

Specific comments:

Abstract: The abstract is concise and well written. Is the transformation of profile estimates to a common prior really a common strategy? The recipe how to do this might be common knowledge, but has this knowledge really been applied sufficiently often to justify to call this a 'common strategy'? The conclusion of the paper refers to maximum a posteriori retrievals in a Bayesian sense (optimal estimation). This assumption should be mentioned in the abstract, because the conclusion might be different for other types of constraint, and the reader of the abstract does not necessarily know that TES is analysed with optimal estimation.

Introduction: The introduction motivates the study, summarizes the necessary information on the TES instrument and introduces the averaging kernel representation of the retrieval. Some more information on the retrieval details may be useful already here. Currently the reader is referred to other papers, and some information is given in Section 3.1.2. Of particular interest are the sequence of operations and the elements of vector **x** beyond the obvious ones (if any). Are there joint-fit quantities beyond the profile values of the target species (e.g. surface emissivity) or have these been retrieved in a preceding step?

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p 1264 l 5: “A is the averaging kernel”; I suggest “...averaging kernel matrix” (c.f. Backus and Gilbert, 1970, or Rodgers 2000). I suggest bold face A also in the text.

p 1264 Eq 2 and lines 12/13: some sub- or superscripts are missing (at least on my screan and print-out), both with the $\hat{x}$ and the x_a. The current version of the equation certainly is correct (finally: $\hat{x}$=$\hat{x}$) but trivial.

By the way: while the formalism of the transformation chosen here certainly is better suited for implementation, and is fully consistent with the concept introduced by Eq. 1, the equivalent transformation given by Rodgers (2000; Inverse Methods for... Eq. 10.48) might be more instructive for readers less experienced with the averaging kernel formalism. The *post facto* transformation as formulated by Rodgers (2000) can be understood without involving the averaging kernel representation of the retrieval, just on the basis of weighted means. Mentioning this representation may help to address a wider readership.

Another issue: Eq. 2 assumes that the old and the new *a priori* information are characterized by the same statistics, i.e. that the related $\mathbf{S}_a$ matrices are identical. Most times this assumption may be – at least in approximation – valid, but I think this issue deserves some discussion.

Method: The chosen analysis method is clearly documented. The chosen approach is appropriate.

p 1265, text after ordered list (lines 15–18): I would expect SSC and SU to be equivalent only in the linear case; further I would expect SS and US to be equivalent only if full convergence is achieved (the latter seems to be an issue, c.f. differences in

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data versions v003 and v004). How is convergence of a log(VMR) retrieval defined in cases when the signal would – due to measurement noise – require a negative VMR? Could it be that this issue has something to do with the "non-global minimum issue" discussed later?

p 1265 l 13: The term Jacobian, while certainly known by most retrieval scientists, is not defined in this paper, particularly because the formalism used here does not explicitly involve the Jacobian. I suggest to avoid this technical term here and to argue in terms of 'relative sensitivities of the signal w.r.t. ...' instead. This will make the paper better accessible to a wider readership.

A general issue w.r.t. Section 2 (See also general comment to Section 1): Beyond the target variables (i.e. the profiles of the target species), often further variables are included in the vector **x** (e.g. further atmospheric state variables or instrument characterization variables). Is this also the case with TES retrievals? If so, it should be mentioned, and this issue then deserves some discussion, because this adds complication to the use of Eq. 2. I am asking this, because many space experiments (ATMOS, MIPAS, ACE-FTS...) make use of 'joint-fit variables'.

Results: The results are presented in a traceable way and are thoroughly discussed.

p 1266 l 16: It is not clear to me why the Run ID is quoted, because it is not referred to later in the paper.

p 1266 l 16: 'target' is a very generic term. Wouldn't 'nadir' or 'subsatellite point' be more specific?

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p 1267 l 13: I do not quite understand what 'enhancements seen in the prior' mean. Meant: 'enhancements seen in the results after conversion of *a priori*'? Or 'regions where gas amounts were enhanced both in the original prior and the standard retrieval?'

p 1268 l 9ff and elsewhere: since the effect of the quality flags is discussed here, it would be nice to know what the quality flags are based on, e.g. χ^2, or whatever.

p 1269 l 9ff: Here the authors expect that a large *a priori* change would lead to a breakdown of the linear transformation (Eq. 2). It should be noted that a moderate *a priori* change in a situation of a saturated emission line can also lead to the breakdown. In other words: the amount of the *a priori* change is not an unambiguous criterion for linearity, i.e. for applicability of Eq. 2.

p 1269 l 17ff: This statement seems ambiguous to me: Do you mean "mean that a prior's change of less than 10% will end up as unbiased fluctuations..." or do you mean "10% of (each) prior's change (regardless how large it is) will end up as unbiased fluctuations (while the other 90% will cause a bias?)"? Or anything else? The first way to understand the statement seems to make more sense, but the second seems to be closer to the original text.

p 1270 l 22: Is there any justification for the averaging kernel threshold of 0.04, or is this an ad hoc decision?

Conclusion: The conclusion is fully supported by the results section. A summarizing statement (such as lines 12-14 of the abstract) may help to get the main message without getting lost in details.

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Technical corrections:

Since English is not my native language, I am not quite sure if my comments on wording and grammar issues are correct.

Title: Since ‘Impact’ is specified (‘of nonlinearity...’) shouldn’t it read ‘The impact’?

Title: I suggest to use either a compound noun (‘trace gas profile estimates’, ‘profile’ singular) or a genetive plural (with an apostroph: ‘trace gas profiles’ estimates’)

Abstract l 7: examine (remove ’s’)

Abstract l-2: remove blank between ’10%’ and ’of’

p 1263 l 6: ‘which’ $\rightarrow$ ‘who’

p 1263 l 8: ‘which’ $\rightarrow$ ‘who’

p 1263 l 9: ‘which’ $\rightarrow$ ‘who’

p 1263 l 16: I suggest blank before and after dash, before ‘2260 cm^{-1}’

p 1269 l 20: remove ‘the’ (third word).

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