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Interactive comment on "GFIT2: an experimental algorithm for vertical profile retrieval from near IR spectra" *by* B. J. Connor et al.

Anonymous Referee #2

Received and published: 7 February 2016

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Review of "GFIT2: An experimental algorithm for Vertical Profile Retrieval from Near IR Spectra" by Connor et al

This paper discusses an application of the Optimal Estimation technique to retrieve chemical profiles of CO2 to a previously total column-only retrieval software package. The manuscript focuses exclusively on the 1.6μ CO2 band that is employed by the TCCON network of ground-based spectrometers to make data products commensurate with NIR GHG satellites e.g. OCO-2 and GOSAT. Further the discussion focuses on empirically mitigating the effect of systematic spectral artifacts at the retrieval stage where the (valid) assumption is that the required full physics (e.g. HITRAN and/or spectral line shape calculations and their required parameters) are not readily forthcoming.

The paper concludes that to date their effort is insufficient to make a practical improvement to the existing total column retrieval. The paper in scope and conclusions reads more as a work in progress that may with further exploration provide the substantial improvement desired. Especially the exploration / use of other spectral regions which is noted as a possibility would be an addition to the current body of knowledge on the retrieval of CO2. This reviewer would recommend continued further work rather than a publication at this time of the current limited and negative result. Further if the current submission is to be pursued there are several needed revisions / expansions for the current document to go forward.

Specific issues:

General:

1. Exclusive use of typical test cases precludes a depth of understanding for the reader as to the pervasiveness of each result. Repeated use of the same cases helps focus on specific effects pertinent to specific changes. But for some results a mean + standard deviation plot of retrieved profiles for instance would easily provide more information and use existing data and is really needed.

2. There are not sufficient references. Many statements based on other work need to be referred.

3. The ordinate of plots in Figs 1 & 2 are in kilometers but plots of the profiles are given vs pressure, this should be consistent.

4. There is no plot of the spectral region with delineated spectral components thus the reader has little reference for the actual specific fit and hence an illustrated description of the fit issues being discussed.

5. Additional plots of the fits and residuals that give rise to the profiles retrieved in each step or test would add to an understanding of the incremental improvements (or not).

6. The OE technique relies on a balance between the a priori data and its variance (Sa) and the spectral (new) information and its uncertainty (Se). This paper makes no mention of the Sa constraints. Its necessary to at least state all the parameters used in the tests even if they are kept constant.

7. Between the lack of references, plots and statement of a priori data the effort undertaken is unnecessarily and inappropriately opaque.

Specific:

Page 2:

1. Line 12: "For one, it can be shown that such an algorithm has more uniform sensi-

tivity to CO2 as a function of altitude". Please provide details of this calculation (that provide kernels to Fig 1 & 2. In particular, the Sa, Se, solar zenith angle. Was this for a synthetic spectrum? These are provided to the reader as actual kernels yet appear somewhat idealized? Please clarify.

2. Line 21: "And since profile retrieval attempts to extract more information from the spectrum than profile scaling, there typically needs to be a priori constraints to keep the retrieval stable." Many previous papers have illustrated this, one or more need to be referenced.

Page 6:

1. Line 13: grammar: . . . avoiding the inversion. . .

Page 7:

1. Eqn 5 is not legible

Page 8:

De-weighting methods are central to this work. Previous work needs to be referenced.

1. Line 6. This de-weighting method is implemented in existing algorithms and needs to be referenced.

2. Line 13: This de-weighting method based on previous residuals is also not new, please reference.

3. Line 20: same as above.

Page 19:

1. Line 6: . . .that may be fortuitous. . ., this is the retrieval falling back to the a priori since there is no information. Using "may" seems in appropriate.

Page 20:

1. Line 9: . . .which we based on the diagonal of Se. . . Is it not that Fig 8 is the new Se based on the residuals of the previous column fit? Also please explain 'based' on. How is the curve in Fig. 8 actually calculated?

Page 21:
1. Line 9: ...runaway oscillation... This is extreme, nearly 10% off at the surface. Some explanation of the source would be illustrative for the reader.

Page 32:
1. Line 5: As no tests were preformed on the ILS and extensive measurements are performed as part of the quality control effort of the network it is surprising or not very credible that this is mentioned as a possible source of oscillations.
2. Line 19: Similarly, the instruments are limited to OPD of 45cm which curtails the altitude sensitivity derived from the pressure broadened spectral lines. Higher OPD or lower wavenumber spectral regions could be explored.