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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interference errors in infrared remote sounding of the atmosphere" *by* R. Sussmann and T. Borsdorff

Anonymous Referee #1

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General Comments

The authors demonstrate a new method for characterizing trace-gas retrieval errors in optimal estimation-based retrieval algorithms due to non-target species. The approach is to expand the state vector to include all possible trace gas species which affect the measured spectra and then interpret the cross-species elements of the generalized averaging kernel matrix. This is a reasonable and fairly direct approach and is worthy of publication. However, in its current form, I find the manuscript too long and lacking focus. Both casual readers and those familiar with the topic will find it extremely difficult to access the main points in this paper.

As a remedy, I suggest that the authors take one of two possible strategies. The first

strategy would be to simply condense the paper by at least one third. If the authors choose this path, I would recommend that they move much of the material in Sections 2 and 3 to an Appendix (or Appendices) and emphasize a single case study (like that in Section 4) in the main body. Despite the length of the current manuscript, there is actually not a great deal of analysis. Concentrating on a single case study would permit more analysis of the features of the interference averaging kernels, for example. The alternative strategy would be to break up the paper into two papers: one describing the theory, the other demonstrating an application. In either case, I strongly believe that the paper requires stronger editing and should be reorganized to emphasize the main points of this research.

Specific Comments

1. Introduction states that optimal estimation methods have been applied solely to microwave observations. This is untrue.

2. In places, text is unclear, and even grammatically incorrect. For example, the following 'sentence' appearing on page 13030 is not actually a complete sentence: "For example, temperature profiles retrieved at the same time as the target species, which may be used in order to minimize errors from insufficient knowledge of the true temperature profile at the time of observation."

3. Strictly speaking, what authors call 'interference errors' should be considered as a subclass of 'forward model parameter errors' (as defined by Rodgers) rather than an entirely 'new class of errors' (p. 13033). By definition, forward model parameters include all those parameters which 'influence the measurement, are known to some accuracy, but are not intended as quantities to be retrieved' (from p. 44 of Rodgers book 'Inverse Methods for Atmospheric Sounding'). Profiles of non-retrieved trace gases are clearly such parameters.

4. In Section 3.3, the authors use an optimal estimation-based constraint matrix for the target gas, but use an ad-hoc Tikhonov constraint matrix for the interfering species.

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This inconsistency is never fully explained. Is it because the authors believe that there are better in-situ statistics available for CO than for the interfering species? Ad-hoc constraint matrices (which include ad-hoc fitting parameters) should only be exploited as a last resort; they are not based either on observational evidence or any physical reasoning.

5. Section 4.1 seems much longer than necessary.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 13027, 2006.

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