

Interactive comment on “Multi-year comparisons of ground-based and space-borne Fourier Transform Spectrometers in the high Arctic between 2006 and 2013” by Debora Griffin et al.

Anonymous Referee #3

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This paper describes comparisons between multi-year trace gas retrieval datasets from two ground-based Fourier Transform Spectrometers located in the high Arctic. The ground-based retrievals are also compared to satellite retrievals from the ACE-FTS (v3.5). The trace gases are split into “stratospheric” (O₃, HCl, HNO₃, HF) and “tropospheric” (CH₄, N₂O, CO, C₂H₆) species for the purposes of the analysis. The authors use the longer of the two ground-based datasets to assess long-term variation/increases of these gases.

Overall, these results represent an interesting long-term dataset. The validation of the ACE-FTS v3.5 retrievals also has value. The paper is appropriate for AMT. The methodology is sound (although I have a few questions in places – see below). I

C1

recommend that this work be published after minor revisions.

Major comments

The paper describes comparisons between the ground-based radiometers and the ACE-FTS v3.5 retrievals. When broken down by year, the number of co-incidences seems rather too small to provide a rigorous validation of the satellite products, especially for the so-called tropospheric species. The abstract states that there is no significant increase in the mean differences over the eight years of the comparison. “Significant” has meaning in a statistical sense. Statistically, what magnitude of change would be significant, given the numbers of points involved in the comparisons?

Are there other studies that have been used to assess the stability of the ACE-FTS retrievals over time? If so, they should be cited here. If not, then this should be stated.

I would have liked to have seen a scatter plot that shows the ground-based retrievals for all years compared to the ACE-FTS retrievals for all years, perhaps with different colors for measurements inside and outside the vortex. Is there a reason why such a figure is not shown?

Figures 5 and 6 could use improvement. The molecular labels on the plots are hard to see and the plots appear to be quite low resolution.

The text could use some editing to prune redundant text, since there is a lot of repetition. Some of the figures need to be better quality. Specific comments are provided below.

Minor comments and typos/grammar

Page 2, line 8: PARIS-IR should be spelled out at the first mention in the abstract

Page 2, last sentence of the abstract: This is an odd sentence. “Increased (gases) near PEARL” makes it sound as though there is some kind of spatial gradient, and stating that these are observed in the PARIS-IR implies somehow that the increases were not

C2

observed in the Bruker dataset. Suggest changing this to read something more like, "Increases in (gases) have been observed in the PARIS-IR dataset, the longer of the two ground-based records."

Page 3, Lines 1-3: There is something not right about the grammar in this sentence.

Page 3, Line 18: Does this paper really focus on the retrieval of partial and total column values, or should you rather say that you focus on the analysis of the retrievals?

Page 3, line 22: CO and C₂H₆ also have industrial sources. C₂H₆ is also associated with oil and natural gas extraction. CO also has a non-negligible biogenic source, which is becoming an increasingly large component of the overall budget as anthropogenic emissions decrease in the developed world (see, for example, Hudman et al., GRL, 2008). These also ought to be mentioned here, if you state biomass burning as a source. If you are going to be consistent, you might also state the sources for CH₄ and N₂O. Since this is not a paper focused on sources, perhaps you don't need to spend space talking about sources, but to only mention biomass burning without mentioning other sources could be misleading.

Page 3, lines 30-34: Lists of numbers in the text are difficult to read and I am not convinced that listing all the numbers here is instructive. There is no information here that would let the reader know whether these numbers can be compared directly to the numbers in the Tables in this work or not. If it makes sense to compare these numbers to the numbers from this work, then they should be listed in a table, not in the text. If the numbers listed here from previous work cannot be directly compared to the numbers from this work, say, because they are from different latitudes and/or use different coincidence criteria, then there doesn't seem much point in listing them. It would perhaps be more instructive to have a table with these numbers for the ACE v2.2 comparisons, with some brief description of the latitude regime, number of cases etc involved. Alternatively, you might choose not to show the numbers, but just to discuss the important points about what has and has not been done in terms of validation of

C3

both the v2.2 and v3.5 ACE-FTS datasets, and how this study adds to the existing body of knowledge.

Page 4, lines 2-4: Again, this list of numbers is tough to follow and I am not sure what to conclude from it. Are these the numbers that are most directly comparable to the numbers in the Tables in this work?

Page 4, lines 10-13: See comments above. Why list % differences for v2.2 validation but not for the v3.5 studies that you cite here? A consistent approach is needed.

Page 4, line 15: The wording should be updated here. You are not performing a comparison of multiple trace gases, you are performing a comparison between the satellite and ground-based data for multiple trace gases.

Page 4, line 19: Again, care should be taken with the wording. Strictly speaking, the method and criteria do not reduce the biases.

Page 5, lines 7-8: "1/3 of the beam is directed into PARIS-IR and 2/3 of the beam into the Bruker". For those not familiar with the instrumental set-up – what does this mean?

Page 5, lines 8-9: "During the campaign, the satellite-based ACE-FTS took measurements near Eureka. . ." The way that this sentence is written implies that the ACE-FTS took special observations for the purpose of the campaign, targeted for the Eureka location. Was this the case, or was the satellite just making the observations that it would have made regardless of whether or not there was instrumentation on the ground at that location at that time? The same question applies to the way this is worded in page 8, lines 12-13.

Page 5, lines 19-20: The wording here is unclear. Does it take 7 minutes to acquire 20 spectra? Also, the last sentence in this paragraph seems redundant.

Page 5, line 30: "depending on the filter range" – This is unclear. Are the filter ranges changed from time to time, or does the "two or four co-added spectra" depend on the filter range? (Some filter ranges require four co-added spectra, due to the instrument

C4

noise characteristics, while other ranges with lower noise need only two?)

Page 6, lines 25-26 and page 7/8: Why use a daily profile, rather than using the profiles from the different model time steps? I did not understand the description of the estimation of temperature errors in the retrieval. What do you mean, “averaged radiosondes”? Does this approach account for errors due to variability of the temperature profile with time? Please provide further explanation.

End of page 6/start of page 7: “A forward model is used to generate a model atmosphere from this a priori information...” What does this mean? In my mind, a forward model usually refers to the calculation of radiances, given the input atmospheric state.

Page 7, line 24: “the total column averaging kernel is forced to 1 at all altitudes.” I don’t know what this means and what the justification is. Please provide some further explanation.

Page 9, line 24: Technically, smoothing does not improve the intercomparisons. Consider rephrasing this to just say instead that accounting for the difference in vertical resolution between the two instruments is necessary in order to assess biases between the retrievals.

Section 3.2: Why use 0.5 x (PARIS + Bruker)? Why not just use one of the instruments as the reference? Also, there is a lot of repetition within this section as well as repetition of material from previous sections. This could use some editing.

Page 10, line 23: “no significant bias”. How do you determine what is significant here? Presumably, the significance of the bias should be somehow related to the atmospheric variability of each gas, and probably also to what you might want to use the retrievals for? For example, CH₄ is significantly less variable than CO. What will these ground-based trace gas retrievals be used for, aside from ACE-FTS validation?

End of Section 3.2: Are you saying that you conclude that SFIT4 provides more accurate results than SFIT2? If so, what would be the likely reasons?

C5

Section 4: There is a lot of repetition in this section. Given that it was expected that a tighter coincidence criterion would result in closer agreement between ground-based and satellite measurements, a lot of space is devoted to this issue in both 4.1 and 4.2. This could be shortened considerably.

Page 13, lines 21-34: If all these numbers are in the Tables, is there a need to list them here in the text? It is much easier to look at a table.

Page 14, lines 10-14: The explanation of partial/total column differences for HF is not very clear. Is the underlying issue here that the a priori profile used for HF is on the low side, but the observed values tend to be enhanced? Please clarify.

Page 15, line 31: What about the C₂H₆? Could the difference in C₂H₆ be due to plumes seen by the ground-based instrument but not by ACE-FTS? Have you looked to see whether the enhancements in CO observed from the ground-based instruments were coincident with enhancements in C₂H₆?

End of page 16, start of page 17: “Thus, there is the potential for a bias...”. I did not understand the two sentences at the end of this paragraph. Please find a way to rephrase to make the point clear.

Acknowledgements: What is DMP? Is this spelled out anywhere?

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C6