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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics and error estimations of stratospheric ozone and ozone-related species over Poker Flat (65° N, 147° W), Alaska observed by a ground-based FTIR spectrometer from 2001 to 2003" *by* A. Kagawa et al.

A. Kagawa et al.

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We thank the referee, Aldona, for her helpful comments and suggestions.We appreciate the effort you have made and the time involved to improve our manuscript.We have carefully considered your comments and will correct our manuscript as much as possible in the revised version.The following are our responses to your comments.

General comments

1. However, the authors omit many of these key references, which place their general work - and also their specific results - in a broader context.

Response: Thank you for your suggestion. References to these key papers were missing. We will include them in the revised version and include discussion of their relationship to this study.

Specific comments

1. Section 1:Page 3, paragraph starting on line 3: A reference to Rinsland et al., 2003 (JGR) should be added and briefly mentioned as an example of a study of the stabilization of Cly using FTIR data from multiple NDSC/NDACC observation sites.

Response: We are going to mention the study by *Rinsland et al.*, 2003 (JGR) in the revised version.

2. Page 3, paragraph starting on line 14: More recent references would be helpful in the introduction, both to the recent (optimal estimation-based) FTIR work mentioned in the General Comments above, and for the general atmospheric processes outlined for mid-latitudes.

Response: We are going to include the recent FTIR work you recommended in the revised version. As for the recent general atmospheric processes outlined for mid-latitudes, we are going to include the *WMO*, [2003] study and references therein.

3. Page 4, paragraph starting on line 7: In the beginning of the introduction, mid-latitude chemistry is alluded to, but later on in the intro (and summary) it is stated that Poker Flat is between mid-latitudes and the Arctic. Then, the following statement is made: “Because it is outside the polar vortex for most of the winter and spring, the gas phase and heterogeneous chemistry over Poker Flat is not affected by polar ozone loss,

except for transport of diluted ozone from the polar vortex.“ This statement appears somewhat at odds with a recent paper by one of the co-authors (Kasai et al., 2005, Adv. Space Res.), where it is reported that “The winter to spring decrease of CO column over Alaska seems consistent with a strato-mesospheric vortex full of descending CO-rich air in winter and a CO-depleted air in spring when upward transport bring low CO from the mid-to-low stratosphere at high Northern latitudes.“ Thus, the stratospheric versus strato-mesospheric conditions over Poker Flat need to be addressed in more detail - if the authors have done back-trajectory or PV studies in the past, this work needs to be included or referenced. The location of Poker Flat in a mid/high latitude boundary region is precisely what is interesting, as the authors point out in the summary, but extra care is required in the interpretation of future results from this FTIR observation site, and this needs to be acknowledged.

Response: Yes we agree with your observation here. We have PV data available for the Poker Flat location using UKMO data. Because discussion of any perturbation to stratospheric species cause by the polar vortex is needed when considering seasonal cycle behavior, the condition of polar vortex activity during the 2001 to 2003 spring periods will be described in the revised version in the introduction and in section 7. Though these are preliminary calculations, PV values larger than 28 [10^{-6} m^2 kg^{-1} K s^{-1}] at 475 K over Poker Flat occurred 20 % of the time 2001, 2 % in 2002, and 3 % in 2003 in March and April (61 days). We also confirmed that these high PV values were somewhat correlated on days when there was a perturbation in the gas species in this study. Detailed analysis of PV and various species for every single day is beyond the scope of this present study, but we will include the condition of polar vortex in 2001–2003 spring period and note the possibility of perturbations to species in the several ten percent range in the revised version. For mesospheric CO, the polar vortex had a possible effect on the mesospheric CO amounts due to the influence of mesospheric dynamics. Any correlation between mesospheric CO and polar vortex activity is less clear. There is a clearer correlation between mesospheric winds and mesospheric CO (rather than PV) which is currently the topic of another study [*Jones et al.*, submitted to

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JGR]. We will include an explanation of the link between CO in the upper stratosphere and mesosphere and dynamics in more detail in the revised version.

4. Section 2: P7, L13: It is not clear whether the ILS retrieved from the HBr cell measurement is only used as a diagnostic of instrument performance. Given that the authors use SFIT2 v.3.7, is it the “Simple phase error parameter” (IPHASE) that is being retrieved? Also, how often are HBr cell measurements made, and in what configuration (with the sun or a blackbody or globar source)? Details of instrument performance and stability (i.e. ILS monitoring) should be added to Section 2, while details of ILS retrievals (e.g. SFIT2 vs. LINEFIT), and how that information is used in trace gas retrievals (if at all) should be added to Section 3.
In other words, the authors need to describe the facilities and procedures that exist to measure the instrumental line shape of the FTS. How stable is the instrument over time? How exactly is this information included in or accounted for in the retrievals?

Response: Measurement of HBr cell spectra is performed about once every 3–6 months when there are frequent atmospheric observations and less than once a year during periods when there are not frequent observations using glow-bar source. LINEFIT9 was used for ILS retrievals. The subsequent atmospheric retrievals are performed using information of modulation efficiency and phase error derived from our HBr measurements (i.e., from LINEFIT9 output). We will include a description of the method used to infer the ILS information in the revised version.

5. Section 4: P8,L23: While the retrieval parameters and spectral fit microwindows may not be the same (or even very similar), it is nevertheless appropriate to also compare the O3, HCl and HF characterization results to those of Schneider et al. (2005, ACP). Furthermore, O3 results should also be compared with the detailed study presented in Schneider et al. (2005, JQSRT). HNO3 characterization results may additionally be compared to the more recent work of Wood et al. (2004, JGR).

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Response: Thank you. I will include comparison with above reference in detail in the revised version.

6. Section 5: Section 5.3: The error values reported in this study should be placed in the context of those reported by some of the investigators mentioned in the General comments, in addition to the work of Barret et al (2002, 2003, 2005), e.g. Schneider et al. (2005, JQSRT; 2005, ACP) for O3, HCl, and HF. The newer study of Wood et al. (2004, JGR) can also be used for HNO3. While it is not possible to simply compare the numbers (because every FTIR group makes some different assumptions in their retrievals, characterization and error analysis), including the numbers from other studies in Tables 4a-d (and perhaps highlighting the major differences in assumptions) would be helpful to the average reader. The discussion of the similarities/differences should be expanded. Furthermore, the authors estimate ILS errors by varying the Effective Apodization Parameter (EAP), and therefore changing the modulation efficiency of the FTS. While this is a very good start, it is fair to point out that the current state-of-the-art in the NDSC/NDACC FTIR community is to estimate both modulation efficiency and phase errors of an FTS by using the LINEFIT algorithm of Hase et al. (1999, Appl. Opt.). Schneider et al. (2005, JQSRT; 2005, ACP) present a detailed analysis of the impact of modulation efficiency and phase errors on profile retrievals. In their 2005 ACP paper, phase error is the larger of the two, while both are still less than line intensity error (see Table 2). The authors should discuss the limitations of their approach, and any work already done to switch to higher SFIT2 versions, which allow one to use the modulation efficiency and phase error parameters produced by the LINEFIT algorithm from cell measurements.

Response: There are two issues that are somewhat separate; the use of LINEFIT9 for the determination of the ILS to correct deficiencies in the forward model of SFIT2, and second, the estimation of error terms in our error budget. For the former issue, SFIT2 version 3.7 had the inclusion of a tabular input of both apodization and phase terms from an external program like LINEFIT9. In section 5.2 of our paper, we described the

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use of the effective apodization parameter as a means of estimating possible effects of ILS errors on subsequent concentration profiles. This needs to be clarified in the text, but we also note the suggestion of the referee to use the method detailed by *Schneider et al*. (2005, JQSRT; 2005, ACP) that also includes phase error, an important error term that needs to be included in our error budget. Error values reported from other NDACC sites will be included in the discussion of the revised paper. These error values from other studies will be included in table 4 if an equivalent comparison is possible.

7. Section 6: P14,L10: It is difficult to judge the improvement after smoothing in the lower and middle stratosphere due to the large values below 10 km.

Response: The explanation in the paper was confusing. The text "left panel (profile comparison)" was missing from the paper as an example of the improvement. You can see better agreement after smoothing around 25 km in left panels of figure 3(b) and (c). This will be included in the revised text.

8. P14,L27: Schneider et al., 2005 (JQSRT) performs detailed FTS-O3-sonde comparisons, which account for the sonde errors. How does this work compare/differ? Please expand the discussion to include the relevant numbers from Barret et al. 2002/3 (Table 4 and/or Fig. 8) and from Schneider et al. (Fig. 15)

Response: We will include that in the revised version. Thank you.

9. P15,L20: "HITRAN 2004 data are about 4% lower than the respective HITRAN 2000 lines." Something seems reversed in this paragraph... If the authors use HITRAN 2004 as default for the analysis in Fig. 5 (yes?), then switching to HITRAN 2000 would INCREASE the columns in Fig. 5 by about 4%, therefore widening the gap between FTS and TOMS. Please double-check the wording here.

Response: The authors used HITRAN 2004 as the default, not HITRAN 2000. The simple argument here is that if the linestrength is changed to a higher value when

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computing the absorption cross sections, then for the same measured absorption, the amount of gas that produces this absorption must be lower, not higher. This was confirmed by rechecking the experimental analysis between the two different linelists.

10. Fig. 6: The slope and intercepts of Fig. 6 would give more quantitative information about the biases and differences in sensitivity between FTS and TOMS. Another thing to consider is that (at least for a mid-latitude site) 11% of the O3 total column is found below 10 km, where the sensitivity of the ground-based retrieval drops off quickly (Fig 2a). The retrieval information in this region is provided at least in half by the apriori (dofs = 0.5 between 0-12 km in Table 3a) and could be a source of non-negligible bias in the total column amounts if the a priori is incorrect by a large amount. Section 6.3: Based on the discussion, it does not appear that the high-resolution HALOE data were smoothed by the FTIR partial column kernels before being compared. Given the non-ideal shape of the FTIR kernels for O3, HCl, and HF, this casts some doubt on the discussion of FTIR-HALOE biases, even though the a priori profiles of the g-b retrievals were HALOE-derived.

Response: Thank you for your suggestions. We will include in the revised version the point that limitation of retrievals in tropospheric volume mixing ratio from gb-FTS may induce a systematic error between TOMS and FTS. Also, concerning the HALOE comparison, the referee is correct in their assumption about HALOE profile smoothing (or lack thereof). We will incorporate FTS averaging kernel smoothing on the HALOE data and compare again the resulting profiles in the revised version for proper comparison. The detailed comparison undertaken in the original manuscript was carried out using original HALOE data because the values of species in this study are dominant in stratosphere and shapes of averaging kernels in stratosphere are relatively similar. However, given that we have the FTS averaging kernels at our immediate disposal, and that this comparison seems not to be adequate to some extent for direct comparison between the data sets, it seems only reasonable to do the full treatment of the HALOE data with appropriate smoothing.

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11. P17,L21: “Overall, the gb-FTS O3, HCl, and HF stratospheric columns are well correlated...” -> The R values of the scatter plots for HCl and HF are rather low ($R2 = 0.29$ and $R2 = 0.42$, respectively), therefore, I would agree that the qualitative features of the two datasets are similar (even though there appears to be a phase shift for HF); however, “well-correlated” seems too strong a statement. In addition to known HALOE biases (and possibly the authors not smoothing the HALOE profiles by the FTIR kernels), sampling differences will be responsible for a significant portion of the observed scatter. Plotting the ratio of HCl/HF may shed more light on the similarities and differences between FTS and HALOE.

Response: We agree with the referee’s assessment of the HALOE–FTS correlations. While ozone can be described as well correlated with an R^2 of 0.79, the HCl and HF data need further investigation. We are therefore following the referee’s suggestion and looking at the HCl to HF ratio as well as other indications of dynamical behavior (PV) and Poker Flat’s relationship to the polar vortex edge.

12. Section 7:Section 7.0: The authors indicated the use of monthly a priori (and monthly a priori covariances) (Section 3). This complicates the interpretation of the seasonal variations of the retrieved profiles shown in Figure 9. The change in the retrieved profiles should be compared to the change in the monthly apriori profiles used in the retrievals in order to account for this effect. If a single apriori profile was used, the seasonal cycles would be attenuated, but entirely attributable to changes in the real atmosphere.

Response: Yes, this is important practice. When we checked the difference between retrievals that used monthly and single a priori profiles for ozone in 2002, the difference was small even for partial columns. Therefore the calculated seasonal cycle, in terms of total and partial columns, can be attributed to changes in the real atmosphere for ozone in 2002. We could conclude from this that the influence of monthly a priori to seasonal change is not significant for all species. However, we will calculate this effect

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for other species and mention in the revised version this point.

13. Section 8:“The retrieval errors were estimated in detail and used as the basis for discussion of seasonal and inter-annual variability in stratospheric ozone and ozone-related species.” -> The magnitude of seasonal variations in the partial columns (Section 7) was not compared to the error values (Section 5). Finally, the wording of the other conclusions (TOMS, HALOE comparisons, etc.) may need to be revised, as per the detailed comments above.

Response: A discussion of seasonal cycles compared with error values was missing in our manuscript. Thank you for the suggestion. In the revised version, we will include the magnitude of the seasonal variations in the total and partial columns compared with the estimated error values. The wording of the other conclusion will also be revised according to the modification commented above.

Technical corrections

All technical corrections as suggested will be fixed in revised version. Thank you very much.

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