The authors are grateful to the reviewer for repeated careful reading of the manuscript and useful comments. We tried to resolve or eliminate controversial issues and carefully worked at the text and adding. Below are our responses to the reviewer's comments.

L34, this work does not 'control' implementation …
L38 change to …'suggests optimized'…
L48 change to …qualitatively…
L63 …transports them to…
L88 referring to a phenomenon as 'so-called' connotates that to some degree, the term is inaccurate. In this paper its simply better to be accurate. This should be re-written.
L115 'Until recently… ' is not true or clear. Please see early papers by Rinsland for data back to the 1970's
L125 the statement 'Spectral resolution of these measurements did not allow halocarbons to be measured from the surface'. Is flatly false.
L153 change informativeness to "information content"

We agree with the above comments and have corrected the text of the manuscript.

L155 while using relative transmission values may be accurate its more customary and informative to use relative absorption when discussing absorption features – this should be changed. Further these absorptions of ~ 10, 25 and 50% are not 'small'.

Indeed, such level of absorption cannot be called small, even for large zenith angles of the Sun. We have removed the mentioning of CFC-12 here, rephrased the text for CFC-11 and HCFC-22, and have also given values for solar elevation angles 50 ° - 96 and 95%, respectively.

First, the absorption of CFC-11 and HCFC-22 is not strong. Even for solar elevation of about 15°, transmission of solar radiation caused by CFC-11 absorption is greater than 90 %, for HCFC-22 is close to 75 %. For solar elevation of about 50°, these values are estimated as 96 % and 95 %, respectively.

L196 was the unsatisfactory channel modeling really due to the scatter in the retrievals?

Channeling value higher than 2% is an exceptionally large value, the usual channeling values are an order of magnitude less. These values were found in our spectra only in the first months of measurements in 2009 due to the lack of experience with such measurements. Indeed, exceptionally large channeling values increase the scatter of the results derived. We have not performed any numerical estimates and thus cannot present them. Later we improved the measurement technique so that the channeling values were usually less than 1% even with a non-standard filter. These values of channeling are successfully compensated by the SFIT4 software.

L278 "considering on the most" changed to "considering the most"
L280 "The neglecting continuum" change to "The neglection of the continuum"

We have accepted these comments and have made corrections to the text.

L165+ Major: The choice of state vector constraint is fine, the description of why and statements made to contrast with a fictitious 'OE' constraint is false and would easily mislead a reader. Further why no comparisons with a profile scaling retrieval? It's perfectly stable especially for low information content features. This point continues to be a major flaw of the text.

In the paper (Polyakov et al., 2018) we used OE and later, in the current research, we concluded that T-Ph regularization approach is preferable for using for deriving the halons from our FTIR spectra measurements. In the discussed manuscript, we did not want to raise the issue of comparing OE and T-Ph at all, which is rather complicated question and requires the special research. Therefore, we decided to exclude all questions related to this comparison from the manuscript. We are simply presenting the results derived using the T-Ph method. The profile scaling retrieval method, unfortunately, cannot be fully used within the SFIT4 shell, we explained this in the manuscript by adding the following text:

Since DOFS is close to unity for all three gases, we can consider a profile scaling approach for solving the inverse problem. However, it turned out that although the SFIT4 core solves the problem, a python script for performing the batch processing and estimating errors does not work in this case. Moreover, if profile scaling is used for all gases considered (see Table 1), the mass processing is not performed. If at least one gas (i.e., H2O) is retrieved as a profile, then mass processing is performed, but error estimates are not calculated. We compared the two approaches by analyzing all spectra measured in 2018 (measurements over 80 days) for CFC-11 retrieval. The average difference between the TCs derived by profile scaling and T-Ph approaches for this set of measurements is 0.016x10^15cm-2 or 0.33%, and SD of the difference is 0.012x10^15cm-2 or 0.26%, that is significantly less than measurement errors estimated. Therefore, to avoid problems with batch processing and error analysis, we chose the T–Ph approach.

About L: 300- Fairly Major: this discussion is mostly irrelevant. The climatology of these species is not complicated. It is reasonable enough to construct a constraint with WACCM apriori data. In fact, they are computed for SFIT and readily available.

Of course, such a construction of matrices is possible, we used it earlier. But, as we indicated above, we do not want to discuss the issues of comparison of OE and T-Ph in our manuscript, so we have removed all discussion related to the OE method.

Major: Section at 271: This discussion is interesting but inadequate. The authors only state a difference in TC from a couple tests and do not show or prove it. The spectra in the appendix do not in any way show a difference in the column amount. These slowly varying curves should have an effect on the broad region not dissimilar to the optical filter envelope – although these can change day to day. But the solar viewing instrument uses relative absorption so the absolute transmission is not in general, a concern. That is not to say the continuum has no effect, rather as the author assumes, it may well have an effect. What is required here is demonstrated proof.

We have prepared and add to the manuscript the analysis of the contribution of slowly varying absorption components, Fig. 2 and Eq. 1,2. We performed calculations for 2018 with and without taking into account the water vapor continuum, demonstrating that the magnitude of the influence of the continuum is comparable to the magnitude of seasonal variability of CFC-11

(Fig. 1), and since it is directly related to the atmospheric humidity, which has a seasonal variation, it can distort the seasonal variation of CFC-11.

This is especially true if the authors wish to make a statement that the technique be widely adopted in the NDACC as they do in the conclusions.

We have removed this statement.

This reviewer has now reviewed the document twice. The document has improved in the second version. The tables are improved the discussion of the trends is good. Still, in this second version, there are major issues the need to be corrected. There are too many minor grammar mistakes to list. The document requires a complete review as to wording and grammar. The value, efficacy and methodology of the continuum accounting needs to be described. The discussion of constraint needs to be more accurate.

We have added the text to the manuscript in accordance with the reviewer's comments, checked the text, and removed controversial topics that really should not have been discussed in this paper.