Atmospheric Measurement Techniques Discussions

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Comment on amt-2020-505

Anonymous Referee #3

Referee comment on "First ground-based Fourier transform infrared (FTIR) spectrometer observations of HFC-23 at Rikubetsu, Japan, and Syowa Station, Antarctica" by Masanori Takeda et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2020-505-RC3, 2021

This paper describes the derivation of total column and dry-air mixing ratios of HFC-23, a compound of interest to the atmospheric science community because of its non-negligible contributions to radiative forcing, its long lifetime, and because controls exist on its emissions. Accurate retrievals of this chemical would be important for providing useful independent assessments of its atmospheric burden and how that has changed over time. It is clear that the challenge in providing accurate retrievals is a difficult one, given the very small absorbance that is involved, and because a number of other gases are potentially interfere. The authors have clearly considered many of the factors complicating this retrieval, but the manuscript could use some additional clarifications and considerations before publishing.

Suggesting that the apparent seasonality represents emissive influences is premature, in my opinion, without any discussions of sensitivities and likelihoods. Before this assertion seems at all possible, one would have to explore a number of things (inversion analysis not necessary):

- Seasonal wind patterns reaching the site. Do they vary in a way that is potentially consistent with transport from potential source regions in Spring and not in the others? How about 2018-2019, where the seasonality seems much less pronounced. Given that emissions continue during these years, and perhaps predominantly from a region (China) close to Japan, one might expect larger seasonality in the more recent results, not reduced seasonality such as is observed.
- While it is very good to see the discussion on a factor changing seasonally that affects the retrieved information (temperature), but its influence is insufficient to explain the unusually high amplitude that there is no hint of in the surface data. This is very puzzling. Why, for example, is the seasonality in the NH result much less in 2019-2020? Is it because the newer instrument is less susceptible to interfering influences, so in fact not the result of such a large emission?

- The seasonal changes in total column and mole fraction are on the other of a factor of 2. If this were truly an emissive signal, the consistency with which is observed during spring would enhanced mole fractions over a large region of the mid-latitude NH throughout an entire season and, hence, very large emissions. What emission magnitude would this demand, and it is reasonable given the global emissions derived for these years? Some qualitative discussion of these possibilities is warranted to determine if the hypothesis is, or is not, reasonable.

On trends. It is not clear to the reader why it is important to express and assess trends for multiple periods and some further explanation on this point is needed, especially because it isn't apparent that there is a change in atmospheric growth rates corresponding to the chosen dates. Is it because of concurrent changes in instrumentation? Is it relevant to be deriving trends for significantly modified instruments? Especially over a span of time during which very few retrievals were made? The main conclusion is that this methodology provides accurate tracking of HFC-23 atmospheric mole fraction trends, yet there are significant differences in results and trends that aren't well caveated in this main conclusion.

Other details:

- 19, line 14. This text is missing DJF, I believe: "indicated in Figure 8 that the retrievals at Rikubetsu have a negative bias".

An indication of the accuracy for the CH4 pre-retrieval isn't provided, but would be useful to understand to know if it is accurate or has biases that might affect the HFC-23 retrieval.

- 12, lines 3-22 needs to more clearly written. Explain at first the different approaches that are available, and the terminology, so that the average reader will understand.

Consider subsetting data in some figures in an additional panel to allow the reader a better view of results that are relevant. Make clear, if true, that results in Figure 7 only include retained data with acceptable RMS values.

Abstract describes the work quite well, although more specificity and clarity is needed in lines 29-33. What is the size of the negative biases? Why are results from only some months used in the NH? On p.2, lines 4-5, having the capability to measure trends begs the long-term difference in NH results, [MORE HERE??]

- 2, line 22. Increasing emissions of HCFC-22 do not necessarily mean increasing emissions of HFC-23, given that HFC-23 is associated with the production of 22, not its emission, which is delayed by use in appliances. Focus on the tie between HCFC-22 production with HFC-23 emissions.
- 2, line 23-24. ODPs have been calculated for HFCs, so they are in fact non-zero. The destruction arises from changes in the thermal structure of the atmosphere. Consider rephrasing "because they do not contain ozone depleting halogen atoms".

p.3, lines 12-13. A citation is needed here. Are these truly mandated? Or are they aspirational goals to reduce emissions. Have India or China ratified the Kigali Amendment?

- 4, lines 8-9. This point does not seem all that relevant given the HFC-23 is a long-lived gas who's mole fraction is fairly evenly distributed.