

Woiwode and co-authors present remote sensing observations of dichloromethane conducted using the GLORIA instrument on the HALO aircraft during the PHILEAS campaign. They highlight observations of filaments hundreds of kilometers in horizontal extent with enhancements of hundreds of ppt, many times the typical background. These filaments are found mostly in the troposphere although at least one instance in the stratosphere is highlighted and based on back-trajectory analysis likely originate from East and/or South Asia several days prior. As the authors indicate these filaments represent an important source of chlorine to the upper troposphere – lower stratosphere (UTLS) although the precise mechanism by which they cross the tropopause remains uncertain. Observations of such plumes using in situ measurements – including from the same flights – have been published previously, but their further characterization by remote sensing better illustrates their extent and represents a valuable contribution to broader understanding. Overall, these results are worth addition to our understanding and should be published after addressing technical concerns regarding the quantitative retrieval. In particular, multiple lines of evidence appear to indicate a possible systematic negative bias which needs to be addressed though it might not actually exist.

The authors are careful to note, that their remote sensing observations are likely to typically average over more dilute air masses and therefore unlikely to observe average concentrations as high as the highest found in plumes by in situ sampling. This is indeed the case and is a point well taken. My concern rather is that their observations appear to be systematically lower than in situ observation in background regions and indeed average negative at the lowest levels especially in the stratosphere. Examining Figures 6 and B1 my concern is not that the GLORIA observations are lower during leg A, but rather that they are lower during legs B and C, and especially that especially in Figure 6c they appear to be systematically lower than in situ observations across a wide range of mixing ratios.

Figure 4c and 5c further show systematically negative mixing ratios, which the authors attribute to increased sources of error in the retrieval, but it must be stressed that these would need to be systematic in nature and significantly greater in magnitude than those highlighted in Fig. 2. Prior to even seeing the results I had concerns about a lack of detail about the retrieval which I highlight with specific comments below.

I have the following major comments regarding the retrieval:

- 1) In this ν_9 microwindow the CH_2Cl_2 absorption has limited structure and therefore is potentially vulnerable to improper retrieval of broadband features. Of particular concern, H_2O and CCl_4 have non-zero baselines in this region roughly comparable to the quoted offset uncertainty quoted later and potentially strong gradients in the vicinity of the tropopause above which the retrieval most consistently tends negative.

Temperature more generally is also potentially a factor here. It is also notable that this is reasonably near the edge of the detector, does the offset performance already account for this? Can the authors offer an indication of how accurate they find the retrievals of these species to be in this micro window? Do they show expected gradients? The authors are already filtering data when the vertical resolution is worse than 1500m and for clouds are they applying any quality assurance based on the retrieval of other absorbers?

- 2) A more quantitative comparison of in situ and GLORIA observations accounting for typical uncertainties in both would be helpful. As previously stated I agree with the authors' arguments as to why there is even likely to be disagreement in filaments or plumes, but I disagree with their characterization of background regions showing better agreement and would like to see this contention better substantiated. Even accounting for the hundreds of kilometers of likely horizontal extent of the filaments light paths are likely to sample more dilute air for the filaments, but this argument starts to fall apart when measuring concentrations closer to the regional background. Can the authors conduct a t-test or F-test of altitude segregated data in background regions to confirm the backgrounds are at least compatible?
- 3) Regarding the lines of sight and pointing accuracy, what leads to sharp gradients in pointing uncertainty? As I understand it, the different fields of view are fixed relative to each other so I would expect smoother variability. Alternatively, is this an effect of filtering for vertical resolution? Is it possible that creates a bias in this regard? Or is it a side effect of sensitivity in the vicinity of the local minimum? I am also broadly mildly skeptical of the accuracy of what is communicated by the insets showing the viewing geometries. For lower viewing angles I expect the atmosphere to become opaque unless there is some detail of the window I am failing to consider.

I have the following additional minor and technical comments:

Line 43-44: I suggest the authors rephrase the conditional here or add an additional reference. By the nature of VSLS, the prior findings of Hossaini et al. 2015 may no longer hold true and Dubé et al., (2025) show a consistency of the bottom-up CI-VSLS emissions with the persistence of measured LS Cl_y but studies such as this one are still needed to fully substantiate the link. Some relevant studies are included later in the introduction.

Line 85: Some mention of relevant latitude ranges for the ASM and ASMA would greatly aid the non-expert reader. Isentropic flow from the surface of East China does not intercept the stratosphere until well poleward of the tropical jet. Communicating more clearly that the cyclone and/or anticyclone greatly extend the relevant catchment would help the reader. The next paragraph gives some examples but there is no overview. Appreciating that the

mechanism is incompletely understood, the authors still need to communicate more succinctly or clearly how the meteorology connects with the work to be presented. This comes together around line 100, but right now, one needs to already have some familiarity with the regional meteorology to fully follow until then. By the end of the paper, a connection is clear for the tropospheric filaments while it remains to some degree mysterious for the LS filament. The set up and follow through for this is not completely communicated.

Line 174: This is ~3km along the flight track, correct? For the multidimensional data this should be clear.

What are presumably subsections of Section 2.2 are all labeled as 2.2.

Fig. 1: I'm guessing this is a side effect of figure result of limited figure resolution, the magenta line for CH_2Cl_2 appears to have variable width. Is this incompletely resolved rovibrational structure, or just a trick of the eye?

Line 209: How does the spectrum of ClONO_2 appear in this window? How comparable are features to CH_2Cl_2 ?

Figure 2: Is “noise” here the random component of uncertainty and some function of measured radiance? That should be made more completely clear.

Line 279: unnecessary capitalization of North here. Does “Aleutian trough” here refer to the Aleutian low? I understand it can act as a trough guiding flow but have not seen this precise terminology before.

Figures 3, 4 & 5: Unless it is a trick of the eye from the color scale cropping, the color scales are not quite the same across panels; they should match. At least in the caption and preferably on the figure, there should be an indication that altitude decreases away from the flight track in panel (b). Also, unless the horizontal viewing distances are approximately to scale (which I don't think they are) that should be made clear to the reader. For the inset in (b) I reiterate a mild degree of skepticism for the accuracy of the depiction of lower angles, I'm not sure it needs to be repeated here either.

Line 327 “extend” rather than “extent”, “Southeast” unnecessarily capitalized.

Figure 6: This makes more apparent the altitude mapping on the maps, but the color scale has only limited differentiation for most of the data portrayed, a different color scale might be more appropriate.

Section 5: Without necessarily using CLaMS can the authors give some indication of how F09 filaments map to those in F08? Is it the same filament or a different part of the larger phenomenon?

Section 5.3: Why was the source simulation run all the way back to 1 May, five months prior? Presumably much of the source at that time horizon is entirely outside the domain. CH_2Cl_2 has a lifetime of 50-120 days or so but most features appear to be from the last week regardless.

Regarding the interpretation of source mapping back to TIB and NIN, these regions as defined in Fig. 9a can have dramatic orographic lofting (indeed Fig. 8d even highlights several examples of this). While the Tibetan plateau in main sits mostly at 4-5 km altitude much of the margin between TIB and NIN is near or even exceeds 8km altitude. More details on how the source was initialized would be helpful as depending on the boundary layer definition used over TIB and the margin, trajectories following the typical eastward path off the plateau might get tagged as a matter of course. Combining Fig. 8 c and d, dramatic lofting occurred 4-5 days prior which would correspond to almost exactly with the TIB-NIN boundary followed by presumably a separate event of air from ECH entering at the eastern margin of TIB based on the map.

Figure 9: This figure is challenging to read and follow. A visual indication of the plume location and extent is needed to link the results in relevant panels to the observations. Perhaps a 200 or 250 ppt contour from panel k on the other panels. A clearer indication such as increased font size for the region each panel corresponds to would be helpful.

Line 533-534: Rather than having an origin beyond the two-week back trajectories, the lack of a clear source region could also just be a limitation of the back trajectory analysis' ability to capture transport in the ASM and ASMA as the authors already highlight. I'll reiterate my interest regarding the link between flight F08 and F09. Are the author's contending that the presence of plume 2 in the vicinity of plume 1 is possibly entirely coincidental?