All referee comments are given in ***bold italics***, replies in plain font.

Anonymous Referee #2

General Comments:

Since your period of observation is rather short (6 months), you are unable to provide any information about the seasonality of emissions. I suggest you include the potential for seasonality in your discussion. Others (Hu et al 2017; Grazioisi et al 2015, Xiang et al, 2014) have suggested that emissions of some HCFCs and HFCs show seasonal variations, with higher emissions in summer compared to winter. That, coupled with the fact that the Finokalia source sensitivity region was different between winter/spring and summer (your Figure 2), suggests that you could be missing a component of the mean annual emissions (or even the mean for 6 months) because you are less sensitive to emissions from some areas in summer, when higher emissions are more likely. This might be particularly true for Egypt.

We added an analysis and discussion of seasonality in the revised manuscript following the suggestions of both referees. The limited observation period in in the Eastern Mediterranean and the seasonality of the transport patterns in the same area make it difficult to derive certain results on seasonality here.

You ran sensitivity studies in which the uncertainties associated with prior emissions were varied, and other sensitivity studies that explored different covariance treatments. However, you did not test the sensitivity of the magnitude of the prior emissions or distribution of priors. I think you should either include model runs with higher and lower priors, or provide some justification for why this is not needed. Is simply changing the covariance treatment and prior uncertainty enough?

As also suggested by referee #1 we added two sensitivity runs that explore the sensitivity to the absolute prior emissions. The results indicate only a minor dependency on the prior emissions with a few exceptions. See also reply to referee #1. We included a discussion of these two new sensitivity runs in the revised manuscript.

Can you comment on the sensitivity to background assignment? Is the choice of background more or less important compared to treatment of covariance? Did you try different background methods other than REBS?

The temporal baseline variability for HFCs and HCFCs is generally small and the pollution peaks above baseline to estimate emissions are comparably high. Of course, the baseline is important to calculate regional emission estimates (see for example Brunner et al., 2017). However because we chose the setup such that the baseline is readjusted during the inversion, the algorithm is not similarly sensitive to the initial background assignment and the treatment of covariance is more important in this case. Because of this, we did not go into further detail concerning the background method and only used REBS, which was successfully used for similar studies already in the past.

Minor: It would be helpful to see (in the Supplemental) a time series similar to figure 3, but expanded over a few days or weeks. This would show more clearly the duration of "pollution" events at the different sites and provide a qualitative picture of the "signal". This would also provide some information about correlations among different halocarbon species. For example, you see what looks like pollution events for HFC-143a at Finokalia in December, but do not see corresponding pollution events for HFC-125, even though some refrigerants, such as R404A, are a blend of both compounds.

Our experience from other sites is that even though that blends are important sources, pollution events can not be compared generally. In the case mentioned here for December no pollution event can be seen for HFC-125, because the measurements could not be analysed correctly due to an overlapping unknown peak and, hence, were flagged invalid and excluded from further analysis.

Nevertheless, to underline the duration of pollution events and the correlation between different compounds, a plot like figure 3 concentrating just on the observations in June (best data coverage) was added to the supplementary material and is discussed in the revised manuscript.

Specific comments:

Line 50-51: CH3Cl is probably an exception to the statement that all long-lived halocarbons are potent greenhouse gases (CH3Cl lifetime is ∼ 1 yr, but 100-yr GWP is only 11). Consider changing "all" to "most" or "many".

We changed this to "most" in the revised manuscript.

Line 71: prefer "base value" to "original value"

Changed to "baseline value" and described how this baseline values are determined as requested by referee #1 too.

Line 139: Is the air handling system (pump, drier) also similar to what is used at CMN? It would be helpful to specify here.

Although some minor parts may be slightly different, the instruments at CMN and the one we used are practically identical. We now specified this in the paragraph as well.

Line 202: Why not release particles at 3400 or 3500 m, closer to the actual site JFJ elevation? Is this not possible?

It is possible to release particles on any chosen height. The determination of a lower release height than the actual height of the JFJ site in the model environment is based on the smoothed terrain because of the limited resolution of the underlying model terrain. To avoid releasing particles too high above model ground, a lower release altitude is used. For a more detailed description we refer to the same question asked by referee #1.

Line 301: Consider: "We followed three different strategies concerning the design of covariance matrices ......

Good suggestion which we like to use, thank you.

Line 429: I find it hard to see "satisfactory performance of the transport model" in figure 4. You might rephrase in terms of comparison to other studies, i.e. is this level of reproduction of variability typical for FLEXPART?

The performance visible in Figure 4 and statistically summarized in Table 2 is very typical for this kind of application of the FLEXPART transport model to regional-scale halocarbon simulations. We added a set of references to publications that had achieved similar performance in the past.

Line 467: remove comma in "driven by, an increase"

Removed in the revised manuscript.

Line 495: remove comma in "which shows, that"

Removed in the revised manuscript.

Line 472: Update Brunner et al 2016 to Brunner el al 2017 as this paper is now available.

Because the article has meanwhile been published and changes made to the discussion paper do not affect our manuscript, we now cite the published article and changed citations and the bibliography of the revised manuscript accordingly.

Line 573: Do you mean "their mean values OR the analytical a posteriori ...."?

Referring to Figure 7, the first measure we use is the range of the mean values of the a posteriori emission estimates (box) of the incorporated BASE and sensitivity runs. The average of these mean values is depicted with the thick horizontal line. The error bars give the analytical uncertainty (95% confidence level) averaged over all uncertainty inversions.

In the text and table, the values are given as the average of all the mean values of the BASE and sensitivity runs. The uncertainty is the respective maximum/minimum value derived from all the mean values of these 5 inversions AND the analytical uncertainty (95 % confidence interval).

Line 602: remove comma in "regions, defined by"

Removed in the revised manuscript.

Line 619: maybe a comment on domain emissions compared to global total (Simmonds et al 2017) Section 3.4.2: You might consider comparing Brunner et al 2017 HFC-125 estimates for Italy (1.2 Gg with your 1.05 Gg estimate)

A comment to both global and Italian emissions in Simmonds et al. 2017 and Brunner et al. 2017 were added to the revised manuscript.

Line 680: comma not needed in "fact, that"

Removed in the revised manuscript.

Table 3 Caption: Line 1102: " in in Mg yr-1" Duplicate word, and I think you mean "Gg yr-1"

Changed in the revised manuscript.

Figure 8 caption: Is "average mean" redundant? Are uncertainties 2-sigma here also?

It's the average of all 5 mean values, given by each individual inversion, similar to the definition used in Figure 7, therefore not redundant. Again we use the "structural uncertainty (given by the range of mean values of each inversion run) and the "analytical uncertainty" (given as 95% confidence interval/2-sigma uncertainty) to define a total error here.

Figure 9 caption: Better to cite Harris and Wuebbles (2014), as GWP were calculated in Chapter 5 of the 2014 Ozone Assessment, rather than in Chapter 1, Carpenter and Reimann

We are now citing Harris and Wuebbles (2014) instead of Carpenter and Reimann in caption of Figure 8.