Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-633-RC2, 2021

Comment on acp-2021-633

Anonymous Referee #2

Referee comment on "Swiss halocarbon emissions for 2019 to 2020 assessed from regional atmospheric observations" by Dominique Rust et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-633-RC2, 2021

Comments to manuscript acp-2021-633

Swiss halocarbon emissions for 2019 to 2020 assessed from regional atmospheric observations by Rust et al.

The paper presents the results of an observation/modelling activity aimed at estimating emissions, at the national scale, of a wide range of halocarbons which are ozone-depleting and/or radiatively active gases.

This activity is recognized as relevant from a policy perspective because useful for the validation of national emission inventories and to ascertain the countries' compliance to the international agreements.

The paper is well written and clear, and the research is based on an outstanding and well-established observation activity. However, I have reservations about the methodology used for data analysis and the interpretation of results.

Concerning the methodology, my first question is about the station network. It is well known that reliable regional modelling requires a dense network of stations, which in most regions is not available. As stated in the introduction, in Europe, the AGAGE network continuously measures halocarbons. Beside Jungfraujoch and Mace Head, the network includes two additional sites (Mt Cimone and Ny-Alesund). Moreover, Taunus station in Germany started its monitoring activity in the period covered by the present study. In total, within the study domain, there are three more sites measuring halocarbons in addition to those used by the authors for their analysis.

Considering that the Swiss territory is located between two complex and strong pollutants' source regions, Germany to the north and Italy to the south, and considering that both Mace Head and Tacolneston are far away from sources that might affect the signal at JFJ and BRM and are not able to trace transport from the south, it would be important if the authors could justify the choice of using a less dense network.

Have the authors performed comparative tests to determine the sensitivity of the receptor to the source using different sets of stations (including those mentioned above)?

Concerning the presentation of results (section 3.2), I do not see any result obtained using the Bayesian inversion at JFJ, neither a comparison between results obtained by Bayesian inversion at BRM and at JFJ. Since the comparison of the results obtained with the two methods (TRM and BI) at BRM highlights relevant differences for many of the compounds considered, with the TRM mostly over-estimating with respect to the BI, it would be useful to ascertain if the same deviations are observed comparing the two methods at JFJ.

Comparing the averaged BRM-TRM/BRM-BI fluxes with values obtained at JFJ by TRM only is less meaningful than comparing the average of two methods at BRM with the average of two methods at JFJ.

Therefore, I would like to ask why fluxes evaluated through the Bayesian Inversion at JFJ are not reported in this paper.

Concerning the interpretation of the results (page 17), the authors state that there is little consistency between BRM and JFJ data for a subset of three chlorinated gases. However, looking at percentage differences in Table 2, the lack of consistency seems to affect many compounds considered in the study.

In the conclusions (page 22), the authors state that BRM data provide valuable information for the validation of halocarbon inventories. I agree that the use of a not-remote station located in the middle of the Swiss territory at an altitude of 700 m, allows the authors to investigate emissions from the Swiss boundary layer with higher reliability.

However, given the large differences in fluxes evaluated using two different methods, the interpretation of results is quite difficult, and there is a risk that the analysis, rather than supporting the importance of observation-based methods, might lead to some doubts for most of the compounds considered in the study, except for the three most emitted HFCs (134a, 125 and 32) or for the sum of the HFCs.

Specific minor comments

Pag 8, line 256: the reference Reimann et al., 2020 to be checked or added

Pag 10, line 344: I would use “distributed” rather than calculated

Pag 13, line 416: please consider adding the reference Keller et al. (2011) “Evidence for under-reported western European emissions of the potent greenhouse gas HFC-23”