

Referee #2's comments on "A rise in HFC-23 emissions from eastern Asia since 2015"
EGUSPHERE-2023-6

We thank the reviewers and the editor for their valuable time and efforts in assessing the manuscript. Their recommendation for publication in Atmospheric Physics and Chemistry is sincerely appreciated. We would also like to extend our gratitude to referee #2 for providing us with insightful suggestions and thorough comments on the revised manuscript.

The authors have made a commendable effort to respond to reviewers' comments and recommendations, and I find the paper to be notably improved. I'd suggest it is acceptable for publication once the following addition points are considered and/or clarified in the subsequent revision.

>>> We have taken almost all the reviewer's suggestions into account in the rerevised version, as we note in detail below. [In the following, Reviewer's comments are in bold Courier New and our responses and are in Time New Roman font]

My main issue in the initial version was clarity of writing and presentation. That aspect has been greatly improved in the revision, but there are some issues that would still benefit from further attention.

One overarching point to consider: from the Abstract, if HFC-23 is "mainly [not entirely] emitted.. as a byproduct in the production of HCFC-22", it seems necessary to acknowledge the possibility that the increase in HFC-23 emission from China after 2015 is related to an industrial process separate from HCFC-22 production and the HPPMP. If the mitigation amounts reported by China in recent years are only provided as percentages of emissions related to HPPMP-covered activities (i.e., not absolute magnitudes, see line 60), then the multiplication of total eastern-Chinese HFC-23 emissions by these factors, as done by the authors, may be inappropriate (please clarify origin of numbers on line 62). While an exploration of the possibility that other processes could contribute HFC-23 emissions outside of the HPPMP after 2014 is outside the scope of this paper, the authors should consider if a caveat on this point should be included with their main conclusions suggesting the failure of the HPPMP.

>>> Yes, we agree with the reviewer that the HFC-23 emissions related to HCFC-22 by-production would not be the entire HFC-23 emissions, and thus to address the reviewer's comments given here and below, we have clarified throughout the manuscript that our discussion on the rise in HFC-23 emissions from eastern China after 2015 primarily focuses

on the emissions associated with HCFC-22 co-production, following the approach of the previous study on the post-CDM increase in global HFC-23 emissions (Stanley et al., 2020). The numbers listed on line 62 were calculated based on the reported, annual HCFC-22 productions in China, yearly co-production fractions (%) of HFC-23 to the HCFC-22 production, and reduction percentages of co-produced HFC-23 for 2015, 2016, 2017 and 2018 in the whole of China (listed in Line 60). These reported numbers were in UNEP, 2018 and TEAP, 2021. The reduction percentages for China were reported to UNEP under China's HPPMP.

We have added a few words to lines 62-63 to clarify the above two points "According to China's HPPMP, the *expected* HFC-23 emissions from *HCFC-22 production* in the whole of China should have been 7.5, 1.0, 0.3 and 0.3 Gg yr⁻¹ in 2015, 2016, 2017 and 2018, respectively."

As commented by the reviewer, an exploration of other possible processes that could contribute HFC-23 emissions outside of HCFC-22 production and the HPPMP activity is beyond the scope of this manuscript. Therefore, considering the substantial lack of pre-existing knowledge on such possibilities, mentioning other potential processes in the conclusion that could contribute to the unexpected increase in HFC-23 emissions, apart from HCFC-22 production, would be both vague and potentially distracting.

On the newly added section 2.3, some clarifications are needed related to the use of the terms HFC-23 emission (amount released to the atmosphere) vs HFC-23 co-production associated with HCFC-22 production (i.e., HFC-23 amounts available for emission depending on the presence or efficiency of mitigation; terms such as "potential emission" seem clearer). Be sure that you intended the first two sentences to refer to HFC-23 emissions, or amounts that were released, and not HFC-23 production (potential emission). Given the apparent misuse (I believe) of the word "emission" in the third sentence, I'm concerned that the authors meant for these first two sentences actually refer to co-production rather than emission. The third sentence mentions unabated emissions, so this refers to co-production or potential emission rather than emission per se.

>>> A good comment. As explained in the text, the term HFC-23 emissions in the first sentence of section 2.3 are the amount released to the atmosphere, and for non-Annex I countries that do not report national emissions to the UNFCCC, they have been derived from the reported HCFC-22 production multiplied by co-produced HFC-23 percentages (Stanley et al., 2020; Simmonds et al., 2018). As mentioned in the second sentence, they refer to the "expected emissions" when no abatement activities would be applied. However, since we agree with the reviewer that the term potential emissions seem clearer than the expected emissions, we've changed it to potential emissions in line 165.

We've also clarified this point by changing "emission factors of co-produced HFC-23" to "co-production rates of HFC-23" in lines 81, 163, 167, 245 in the text.

Same point can be made elsewhere in the manuscript, e.g., Figure 5 caption "No abatement emissions of HFC-23 are determined ...multiplied by yearly emissions factors of co-produced HFC-23" ? Non-abated emissions aren't derived from "emission factors", aren't they better described instead as being derived from co-production rates?

>>> The term "emission factors" had been used in previous studies (e.g., Stanley et al., 2020), but as noted in our response to the above comment, the term has now been replaced with "co-production rates" throughout the text and the figure captions in the revised manuscript.

Line 152, should it be "detrended monthly means"?

>>> We have modified the text (lines 52-53) for clarify to read "...was determined by a month-to-month extrapolation between one standard deviation values of all the background monthly means similarly to the background concentrations derived at each measurement time over...."

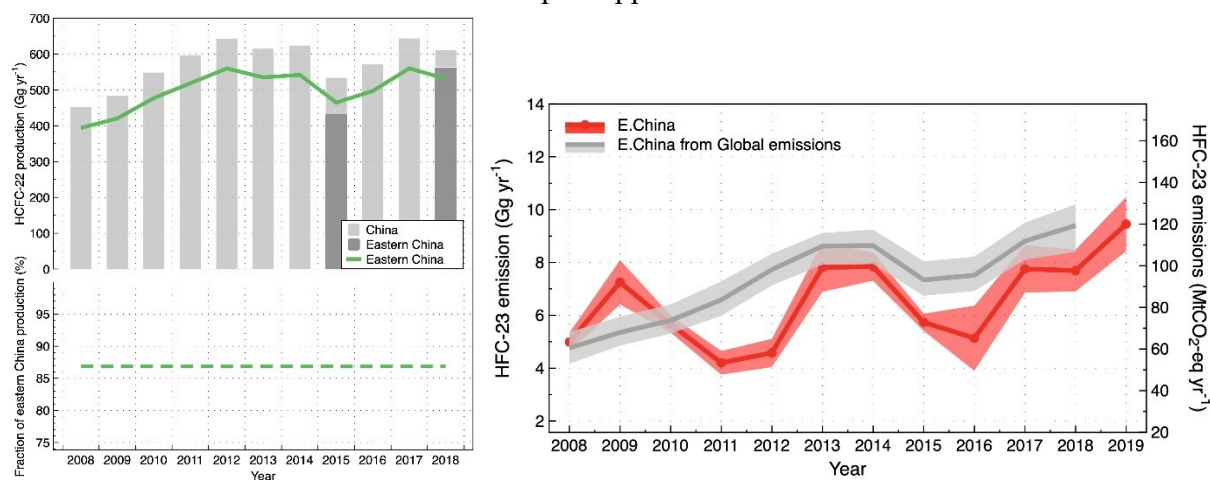
Line 153, for reference, please provide the average enhancement mole fraction so the reader can understand the relative error associated with variability in the background mole fraction.

>>> We have added the 2008-2019 average of the enhancement concentrations of 2.92 ppt in line 153.

Line 175, comment on the implications on your conclusions of deriving HCFC-22 production in years other than 2015 and 2018 with a different approach than expressed by eqn 3 (for example, a constant production fraction over time at the mean of the rates suggested for the reporting years 2015 and 2018). Related to that, the solid green line in the upper panel of Figure S3 needs explaining in the Figure's caption. Also, a spelling issue in the caption: "Inferred" not "Interred" seems likely.

>>> As suggested by the reviewer, we had tested with a constant production fraction of HCFC-22 over time for eastern China determined from the average of the reported rates in 2015 and 2018 (the first figure shown below), and using the fraction of the HCFC-22 production in eastern China against the global total production, we had calculated the eastern China HFC-23 emissions by scaling down the global top-down HFC-23 emissions. The results in the second figure below showed that the post-2015 HFC-23 emissions were identical within the uncertainty to those determined by the current exponential approach (Eqn. 3, Figure S3 and Figure 6), and the emissions prior to the year 2015 were only slightly higher and thus, became more similar than those for the current approach to our observation-inferred emissions (red line) for eastern China. In light of the similarity between the resulting HFC-23 emissions using exponentially varying vs. time-constant HCFC-22 production fractions for eastern China, we decided not to include and discuss the results from a time-constant fraction in the original

manuscript. This choice was based on the consideration that applying a constant value determined in 2015 and 2018 back in the past appeared less realistic.



>>> The misspelled word has been corrected. Thank you.

Line 225, It's hard to assess the assertion that emissions in this study are consistent with previous ones for the same years, because Figure S4 shows different quantities (all of China vs the eastern China estimates from this study) and therefore agreement might not be expected (this needs mentioning here) and actually isn't seen for all studies despite different quantities being compared. Also, add to the figure S4 caption an indication of the method used in each study (Atmosphere-based or inventory based) .

>>> A good suggestion. We have added a word "top-down" in the caption of Figure S4 for clarity to read: "...in comparison with previous top-down estimates of...". We have also updated the corresponding text (line 225) by adding the word "top-down".

Line 273: Figure S5 seems to contain no new information compared to the main Figure 5, so seems unnecessary and confusing as to why it is included.

>>> Agreed. In the previous revision, we had modified Figure 2 to include total emissions from eastern Asia and to show that the observed increase in eastern Asia emissions was primarily driven by emissions from eastern China. Therefore, we have removed Figure S5 because it is easy to understand that the comparison of the eastern Asia HFC-23 emissions to global emissions is very similar to the case for eastern China shown in Figure 5.

Line 286: I'm guessing that ~400 Gg/y of HCFC-22 production with unabated HFC23 emission (Emission of HFC23 / production HCFC-22 ratio of ??) during 2015-2019 would be needed to explain the HFC-23 emission excess, not what your wording seems to suggest: that 400 Gg of unreported HCFC-22 actually took place.

>>> A point well-taken. We have revised the text in line 286 now to read: “Approximately 400 Gg per year of HCFC-22 production with unabated HFC-23 emissions was produced during 2015–2019”.

It would be helpful to indicate if the global estimates in Chapter 2 of the WMO 2022 Ozone Assessment are the same as those shown in the present work (and why not updated to 2020?) .

>>> The global estimates in Chapter 2 of the WMO 2022 Ozone Assessment are identical to those discussed in the main text of this study and shown in Figure 5, except their extension to the year 2020. When we submitted this manuscript in January, the 2022 Ozone Assessment had not been released yet and only the Stanley et al. paper was available, so the updated figures could not be included in this manuscript.