

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2022-240

Anonymous Referee #1

Referee comment on "Bayesian assessment of chlorofluorocarbon (CFC), hydrochlorofluorocarbon (HCFC) and halon banks suggest large reservoirs still present in old equipment" by Megan Jeramaz Lickley et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-240-RC1>, 2022

Review for "*Bayesian assessment of chlorofluorocarbon (CFC), hydrochlorofluorocarbon (HCFC) and 2 halon banks suggest large reservoirs still present in old equipment*" by Lickley et al.

This manuscript presents an extension to earlier work, where ODS banks, and their emissions, are estimated in a statistical framework using uncertain knowledge about emission profiles and atmospheric mole fraction measurements. This study is timely, clear and concise, and will be of broad interest to readers of ACP. I have only a few suggestions, with the majority concerning clarifications in the text and some additional discussion. I have one concern over the treatment of what is termed 'reported production' and how this may impact the conclusions drawn, which I outline below. I hope to see the eventual acceptance and publication of this manuscript once my comments have been addressed.

Comments:

- **Production-related emissions and leakage rates:** From my understanding, reported production is the net production, i.e. reported after any losses during the production process, or the "sellable production". As such, at least in theory, cumulative production should equal consumption for CFCs. This means that total production, before any production losses, would be larger than that reported, i.e. Total production = $P/(1-DE)$ using the notation in the paper (where P is reported production), and production-related emissions would be equal to $DE \cdot P/(1-DE)$. What I take from the manuscript is that, currently, the production-related emissions are quantified instead from the production that has been reported after the losses have occurred. I don't think this will alter the conclusions of the paper, but it may close some of the gap and reduce

some of the reported bias, as it may mean a few percent more production is added to the bank.

- **Discussion of discrepancies in production:** Non-dispersive (and therefore not required to be reported) production exists, for example when gases are produced as by-products. An example CFC-115 contamination in the production of HFC-125 (see Vollmer et al 2018). I don't believe this is a reason for the discrepancies, and there's no evidence to support or deny this, but should be discussed in addition to dispersive and feedstock related production.
- **End-of-life emissions:** The impact of end-of-life emissions needs further discussion. How would a change in the emissions rate due to disposal change the conclusions drawn? Some information exists surrounding post-disposal emissions, e.g. from the US EPA (see e.g. page A-262, Table A-127 of <https://www.epa.gov/system/files/document/s/2022-04/us-ghg-inventory-2022-annex-3-additional-source-or-sink-categories-part-a.pdf>). I imagine this would be too difficult to include in any analysis (and I'm not sure if there is sufficient information to do this) but there is a need to expand the discussion around long-lived banks.

Technical comments:

Abstract, line 19: "...must be carefully accounted for in order to evaluate ongoing compliance with the Montreal Protocol". This could be interpreted that these future emissions fall under controls of the Montreal Protocol, even though emissions from the bank are not controlled. Perhaps better is suggest the importance is to evaluate nascent production vs. banked emissions, or similarly you could change the 'impact' to be for something like stratospheric ozone recovery.

Abstract, line 22: "model a suite" rather than "the suite"

Abstract, line 27: "production for dispersive uses" or similar, as production will be higher than that reported due to e.g. by-product emissions and point of production losses.

Abstract, line 32: Delay ozone recovery in reference to what? Current projection generally take banks into account.

Line 38: *stratospheric* ozone depletion

Line 48: On the use of "we developed...". "we" here does not envelop all co-authors, so perhaps better to refer to it either in the passive, or "Lickley et al 2020, 2021 developed..."

Line 55-57: It would be clearer to continue to use the term 'reported production' rather than just 'production' here.

Line 75: Are these cumulative emissions derived using a top-down or bottom-up method?

Line 86: It's generally called "Bayes' theorem" rather than "theory"

Line 127, eq. 3,4: Subscript italics should be saved for variables, i.e. 'Total' should not be italic.

Line 141, eq 5: It is not clear where the constants A come from. Is it from the SPARC 2013 reference? If not they should be included in the manuscript.

Line 149: Does the term 'fire extinguishers' here refer to all forms of fire and explosive suppressant? Halons are used in many other applications that only fire extinguishers. If this is only a general term used by AFEAS then this should be made clearer.

Line 157: It's not clear what functional form the correlation takes (this is also not clear in the supplement), only the prior set on the correlation parameter is specified. I.e. is there an assumed linear covariance? Or something like autocorrelation?

Line 173: What's a 'medium value'? Should this be median?

Line 189: There should be a full stop instead of a comma before 'therefore'

Line 196: NOAA needs defining

Line 422: The reference to Vollmer et al. 2018 has a formatting error

Table 2: It would be very useful to also include the absolute difference in the estimates (with uncertainties) here in Gg, in addition to only the percentages, to get a better sense of the importance of these various compounds.

Figure 3: Is there a significance to the circle size in Fig 3? If this is to distinguish between WMO 2018 and TEAP 2009 only then perhaps choose a different shape or line style.