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Comment on acp-2022-240

Anonymous Referee #2

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-RC2, 2022

This is a nice extension of a previous analysis to include more chemicals and more processes affecting past and potentially future emissions of halocarbons from banks. The work is important and highly relevant to current issues about halocarbons and Montreal Protocol compliance. I had 2 main issues that need some discussion and exploration before the paper is publishable.

Some consideration of end-of-life processes (sensitivity or includes as a separate category) is needed for banks for which end-of-life emission might be substantially different from emission rates during use (particularly cc foam for CFC-11, perhaps also non-hermetic refrigeration for CFC-12 and HCFC-22). TEAP reports have suggested that this could be a significant influence on emissions in recent and future years, yet this process is not considered by the authors owing to their view of a lack of information (line 329). I'd suggest that some exploration or sensitivity analysis of the issue is important to increase the relevance of these results. The Bayesian approach provides optimized parameters for the past, and those parameters may not be relevant for the future when the relative contributions of end-of-life emission increases substantially. For the model to provide useful expectations of emissions in the future, it must accurately represent a future where emissions are dominated by processes not as prominent in the past, i.e., end-of-life.

It isn't clear if the AGAGE mole fractions being fit in this analysis are surface means or some representation of total atmosphere average mole fractions. I suspect that observation-based surface mean mole fractions are being used 'as is' to represent total atmosphere mean abundances and, if so, some further consideration is needed. For nearly all of these gases the bias between these two quantities was substantial in the past (up to 20% theoretically but likely less for most years), varies over time (reduced in recent years

as emissions are less), and might add substantially to the larger than reported production and estimated bank sizes argued for in the present analysis. Related to this point (vertical mole fraction gradients are substantial and time-varying) a more realistic and time-varying relationship between mole fraction and emission (equation 5) needs including if indeed surface mole fractions are what is included in the inversion analysis.

Details:

The abstract makes assertions that seem too strong given the substantial caveats mentioned at the very end (lines 325-334). These caveats seem outside the assumptions related to priors and lifetime that are mentioned or even hinted at in the abstract.

Line 69, accounting methods use reported information, but also estimates and assumptions about processes leading to emissions.

Figures 1 and 2 show observationally-based results and emissions for only a fraction of years for a number of gases. Inversions would seem to be less relevant and less accurate if performed on these limited data histories.

Citations of Assessment reports should be called out by the lead authors' names in most if not all places (not WMO 2018 or SPARC, 2013). It is only done the accepted way in a few instances in the manuscript.

Line 321-3. Are these really the only two possible explanations for 113?