Review of the manuscript "Potential environmental impact of bromoform from Aspargopsis farming in Australia" by Jia et al., ACPD, 2021.

The paper presents modeling experiment to evaluate how one of the proposed feeding management alternatives to reduce CH4 emissions from ruminant livestock (i.e., Aspargopsis farming) could impact on the stratospheric ozone layer due to the by-product formation of bromoform (CHBr3). This species is a very short-lived species (VSLS) with a mean lifetime of 17 days in the atmosphere, and consequently, the CHBr3 impact on stratospheric ozone depends on the superposition of source strength and location with the efficiency of convective transport. The paper propose a multiple set of realistic local and global scenarios, as well as the occurrence of some improbable extreme episodes affecting the Australian coast, to evaluate a representative range of the overall ozone depletion potential (ODP) of bromoform emissions from oceanic and terrestrial cultivation approaches, and compare them with the impact of coastal natural bromoform emissions. The work is very well-planed and provides a realistic and clear evaluation of the magnitude of one of the environmental consequences of promoting Aspargopsis production in Australia, and determine that even in the worse possible scenario, the negative impact of the additional farming-released bromoform are very small in comparison with the natural contribution from the ocean. The methodology and results are generally well presented, although some clarification is required as described below. I suggest the paper is accepted for publication after the following issues have been solved:

Main Comments:

1a. Ozone Depleting Potentials (ODPs): Concept and Implications

Section 2.5 briefly describes the ODP concept and how it has been adapted to evaluate the ODP impact of VSLS due to their variable distribution in the troposphere. However, given the importance of the ODP fields used to determine the bromoform ODP-weighted emissions presented in this work, I found that more details (and results discussion in Section 4) should be given. In particular, the authors based their analysis on the ODP spatiotemporal study performed by (Pisso et al., 2010) using

the same FLEXPART model, but no mention is provided about other approaches to determine the Stratospheric ODP (SODP) for long-lived species that are know to affect both tropospheric and stratospheric ozone (Claxton et al., 2019; Zhang et al., 2020), and why it is important to distinguish the tropospheric and stratospheric ozone impacts of CHBr3. Page 15, Lines 326-329 is the only place in the text where I found explicit mention that the product gas contribution of VSLS degradation is not being considered, which is reasonable as the proposed methodology considers only the exponential decay of the emitted source gases. However, this should be at least highlighted again in the conclusions and if possible, an estimation of the magnitude of the neglected tropospheric impact of VSLS product gases and/or how the modeling ozone changes depend on the treatment of VSLS product gases (i.e., Fernandez et al., 2021) could be given.

1b. Ozone Depleting Potentials (ODPs): Methodology

The ODP for bromoform is computed by comparing the ozone destruction of CHBr3 compared with the ozone destruction produced by an equivalent mass of CFC-11. However, no CFC-11 sensitivity is mentioned to have been performed for this study. Thus, it is not clear if Fig. 7 is a direct result of the modeling simulations performed in this work, or it is taken from Pisso et al., (2010). If the later is the case (which I believe it is), then, this should be expressed more clearly in the text and proper reference to this study should be given in the caption of Fig. 7. Page 14, Lines 311-313 explicit says that “ODPs for VSLSs are calculated by means of combining two sources of information: one corresponding to the slow stratospheric branch and the other to the fast tropospheric branch of transport”. First, how the tropopause location is determined in the study? Second, is it possible to quantify the contribution of these two branches, and could this be taken as an approximation of the tropospheric and stratospheric influence of CHBr3 farming emissions? Note that one of the main results of the paper is that ODP-weigthed CHBr3 contribution from Aspargopsis farming would be, at most, less than 1% of the natural CHBr3 value (i.e., the Ziska_Coast scenario); thus properly showing how the ODP values were computed for this particular VSLS should be clear.

Minor Comments:

GENERAL: The number of significant digits used when reporting numbers should be revised throughout the hole text.

P2,L36: What do you mean by “the remains are relatively small”?

P2,L37: “less than 0.016%” … is this significant different to less than 0.02%?

P2,L39: “by 0.48%” … of its initial value, or up to 0.48%?

P4,L88: “In consequence, the environmental impact of CHBr3 … *needs to be explored and elucidated*”. As detailed in the main comment, the authors should explicit mention that VSLS influence both the troposphere and stratosphere, and that here only the stratospheric impact is considered.

P4,L94: I found the paper very informative not only to industry, but also to policy makers and the scientific community.

P5,L119: 3.4674×10^4. Does this number have 5 significant digits? Please clarify and make it consistent throughout the text.

P5,L128-130: How did you get the 30 times scaling factor to extrapolate from Australian Aspargopsis production to Global production? And how did you get the 1 Tg DW value? (I could not get that value by multiplying the informed data … I must have missed something).

Figure 1: The lat,lon region shown in the Figure is smaller than the rectangle used for computing the average of CHBr3 mixing ratio in Figs. 4 and 6.

P10,L233: Considering extending the subsection title so it includes the description of the different scenarios. In addition, by looking at Table 1 it is evident that the study was performed for meteorological conditions of year 2018 … But I could not find where in the text this is described (I might have missed it).

P11,L263 and Table 1: The total CHBr3 emission within the background scenario considers the well-established Ziska emission inventory, and is mentioned to consider “all 1ºx1º grid cells directly neighboring the coastline”, which accounts for 3109 Mg (Table 1). How large are the Ziska emissions for a small region of the size of area of Geraldton, Triabunna or Yamba? Similarly, how large are the Ziska_coast emissions if they are compared to the total Ziska emission on the Australian domain [10°-45° S, 105°-165° E] if both coastal and open-ocean grid-cells are considered?

P14,L311: The 20 days lifetime of the VSLS species considered in Pisso et al., (2010), should be mentioned here.

P15,L321-323: “In this study, we present the ODP-weighted emissions, which combine the information of the ODP and surface emissions and are calculated by multiplying the CHBr3 emissions with the trajectory-derived ODP at each grid point”. Does Pisso et al., (2010) provide independent ODP values for each model grid-point and individual trajectory? Please see my main comment regarding this point.

P15,L344 and Fig. 3a: The figure is fine, and is clear that the annual emission for the different growth periods are equivalent, but the text seems to imply that this is a new result of the study. However, these equivalent values is just a confirmation of the assumed condition that all farming scenarios for Australia must have the same total emission. This should be clarified in the text.

P16,L363: “which leads to emissions of 27 Mg (0.1 Mmol) CHBr3 per year for the targeted final yield”. How do you relate this 27 Mg CHBr3 per year with the aprox. 9 Mg CHBr3 annual emission derived from Fig. 3a? Shouldn’t this values be identical? Is it needed to multiply by the bromine atomicity of bromoform (3)?. Please make it clear.

Figure 3 caption: “… under different growth rates *and similar initial biomass and growth period*”. Please make the caption as informative as possible.

P19,L430: “and signals with comparable magnitudes are found at 15 km”. The magnitudes are comparable, but I expect this signals affect much

smaller regions due to the localized source. Is this the case? If so, please make it explicit for the reader.

Figs. 4 and 5: Is the color scale maximum value correct? i.e. 0.05 ppt for Fig. 4 and 0.10 ppt for Fig. 5? How large are the maximum values within the MBL? I would expect them to be much larger than the maximum value of the scale. The caption of Fig. 4 should also explicitly indicate that it refers to Global scenarios.

Figure 7: If the units of the scale is a relative value between 0 and 1, please make it explicit.

Figure 8: The bottom-most bar presenting values for the Global Emission, for which of the global scenarios apply?

Language editing comments:

GENERAL: A language style revision should be performed to the whole text (as well as figure captions), mainly on the unification of past, present and future terms (is, was, will) into a common verbal tense.

P2,L26-30: Split the sentence.

P2,L30: DW acronym is not used again in the abstract.

P3,L48: Two blank spaces.

P3,L64: rephrase “showed the most potential for CH4 production decrease”.

P9,L189: What do you mean by “as the farming aims at high yielding CHBr3 varieties”?

P10,L214: “the gradient is between” … it is computed between? It is computed considering …?

P15, L323-324: “The ODP-weighted emissions provide insight in where and when CHBr3 is emitted that impacts stratospheric ozone (Tegtmeier et al., 2015)”. Not sure if the sentence is properly written. Please rephrase.

P15,L329-330: “but has no large impact on the here presented comparison of global ODP-weighted CHBr3 emissions with farm-based ODP-weighted CHBr3 emissions.”. Please rephrase.

P19,L423: The authors use the terms “destroy” to refer to the impact of cyclone Joyce on the Australian coast. Please consider using a different wording (here and elsewhere).

P23,L488: remove “again”

P25,L517,520: (here and elsewhere). Use subindex for 3 in CHBr3.

P26,L525: “The local CHBr3 emissions from the Asparagopsis farms could be larger than emissions from coastal Australia.” The term “local” here is correct, but seems hidden in the sentence and could be reinforced.

References:

Claxton, T., R. Hossaini, O. Wild, M.P. Chipperfield, and C. Wilson, On the regional and seasonal ozone depletion potential of chlorinated very short-lived substances, Geophys. Res. Lett., 46(10), 5489–5498. doi:10.1029/2018GL081455, 2019.

Fernandez, R.P., J.A. Barrera, A.I. López-Noreña, D.E. Kinnison, J. Nicely, R.J. Salawitch, P.A. Wales, B.M. Toselli, S. Tilmes, J.-F. Lamarque, C.A. Cuevas, and A. Saiz-Lopez, Intercomparison between surrogate, explicit and full treatments of VSL bromine chemistry within the CAM-Chem chemistry-climate model, Geophys. Res. Lett., 48(4), doi:10.1029/2020GL091125, 2021.

Pisso, I., P.H. Haynes and K. S. Law, Emission location dependent ozone depletion potentials for very short-lived halogenated species, Atmos. Chem. Phys., 10, 12025–12036, https://doi.org/10.5194/acp-10-12025-2010, 2010.

Zhang, J., D.J. Wuebbles, D.E. Kinnison, and A. Saiz-Lopez, Revising the ozone depletion potentials metric for short-lived chemicals such as CF3I and CH3I. J. Geophys. Res. Atmos., 125, e2020JD032414. https://doi.org/10.1029/2020JD032414. 2020.