

Author's response

for the manuscript submitted by Chabrilat et al. to EGU sphere
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We thank the two reviewers for their constructive and helpful comments. Please find below the comments in **bold**, our replies in *italic*, and the manuscript modifications in **red**.

Reviewer #1

Minor comments

1.) Length of paper: Overall, the paper is a bit long. I understand that it is important to present and discuss the model performance for several different species. However, making the text more concise and reducing the number of figures could improve readability, potentially increasing both the paper's readership and its impact. I don't have special recommendations here but would just encourage the authors to think about such potential improvements.

We appreciate the reviewer's careful and thorough reading of the manuscript, which is evident from the constructive comments provided. We have carefully considered a major revision to shorten the text in response to this suggestion. However, we anticipate that most readers will access the paper via a web browser and navigate directly to sections of interest using the automatically generated Table of Contents.

The manuscript was deliberately structured to provide a comprehensive and balanced evaluation, which both reviewers have welcomed. Although lengthy, this evaluation will be useful to assess the strengths and weaknesses of the stratospheric composition products provided by IFS-COMPO to CAMS. A substantial reduction in length, whether through text cuts or relocating figures to the Supplementary Material, would risk compromising this balance or result in only marginal gains in overall length. For these reasons, we respectfully propose to retain the current structure and level of detail.

2.) I'm wondering why the model moist biases in the extratropical lowermost stratosphere (below about 100hPa) are not discussed at all (around L542). These are the largest biases in the profiles shown in Fig. 5, and are similar to known moist biases in climate models (e.g. Charlesworth et al., 2023, <https://doi.org/10.1038/s41467-023-39559-2>), and in IFS have recently been shown to contribute to UTLS cold biases (Bland et al., 2024, <https://doi.org/10.1002/qj.4873>). I'd find it good to discuss these issues briefly here.

The following paragraph was added at the end of section 5.2:

Most climate models suffer from large moist biases in the extratropical lowermost stratosphere, i.e. below about 100hPa, likely due to difficulties modelling transport of water vapor near the tropopause with a strong gradient (Charlesworth et al., 2023). This issue also impacts humidity in the lowermost stratosphere of IFS, contributing to a cold bias in the NWP-oriented configuration (Bland et al., 2024) and explaining the large overestimation shown by Fig. 5 in the mid-latitudes below 100hPa.

3.) Scorecard grading: I really like the summary of results in the scorecard in Sect. 7. But I'd suggest to be somewhat more careful with giving particularly high scores here, given the remaining biases in parts of the profiles. Such high scores could be misleading if quick readers don't look into specific details in the related subsections. A few examples where I'm sceptical about the choice of score are:

- Fig. 19, U.S.: CH₄, H₂O, O₃, ... show significant biases above about 10hPa (Figs. 4, 5, 9), so that I'm unsure whether "good performance" is suitable here.
- Fig. 19, Tropical M.S./O₃: Also in the tropical profile (Fig. 9) the bias increases above 10hPa, such that I wouldn't rate the performance "very good".
- Fig. 19, Mid-lat. M.S./H₂O: For H₂O the mid-latitude correlation in Fig. 5 is very low, so that also here I'm wondering about the "good performance".
- Abstract, L30: "very good performance for O₃, HC₄, N₂O and H₂O..." perhaps too strong given the remaining biases in parts of the profiles.
- Conclusions, L1019: "very good performance for CH₄, N₂O and H₂O" I find too positive.

Related to these comments, it's not obvious to me that ACE-FTS is the better reference dataset for stratospheric water vapor (as chosen in Fig. 19 grading). MLS also provides a very good stratospheric water vapor product, and compared to MLS the IFS biases are generally larger.

We attempt to assess the initial performance of new forecast products (here stratospheric species beyond ozone). This is less straightforward as providing a scorecard for relative improvements between two consecutive versions (e.g. Eskes et al., 2024). We aim for an objective attribution of scores by selecting numerical criteria on bias, standard deviations and correlations (table S1) but those criteria are subjective themselves. You are correct in pointing out that initial scores should not be too optimistic, as this would prevent highlighting future model improvements in the evaluation of future model cycles.

We have carefully re-examined our criteria while considering these examples. As indicated by the comment about stratospheric water vapor, the issue did not lie as much in the choice of criteria as in the choice of only one reference dataset to apply them. The second column of Fig. 19 indicated the dataset chosen for performance evaluation, but this was not mentioned in the text and the rationale for this choice was not explained. ACE-FTS was chosen in most cases because it agreed better with the model, leading to more optimistic scores than allowed by a visual inspection of figures 4 to 9.

The revised manuscript thus keeps the same scoring criteria but requires two datasets in agreement to attribute the "very good" scores. As indicated by the Table S1 and the new table S2 in the Supplement, this is achieved by computing a simple score for each dataset (using the same criteria as before) before computing their sum for the species available in both datasets. A "very good" performance assessment thus requires the availability of, and very good agreement with, both datasets. This is outlined in the Supplementary material (see revised table S1 and new Table S2) and explained in the text as follows:

The regional scores are determined objectively from the absolute values of the Normalized Mean Bias (NMB), Standard Deviations of differences between model and observations (STD) and corresponding correlations, using criteria chosen to segregate between the four proposed scores while prioritizing bias performance (see Table S1 in the Supplement). **These scores are computed separately for each reference dataset (second column: "A" for ACE-FTS; "M" for Aura-MLS) and added for the species where both datasets are available. The total score provides a combined performance assessment, requiring the availability and agreement with both ACE-FTS and Aura-MLS to allow "very good performance"**

assessment (see Table S2 for details). The assessment for N₂O in the lower and middle stratosphere relied only on ACE-FTS because Aura-MLS shows suspiciously large disagreements at pressures larger than 10 hPa while difficulties were reported in the retrieval of Aura-MLS N₂O v4 (see section 3.3).

The resulting scorecard (Figure 19) has been simplified and updated accordingly:

							Winter-spring L.S. (30-100hPa)			
Species	Ref. data	Tropical L.S. (70- 150hPa)	Tropical M.S. (6-50hPa)	Mid-lat. M.S. (10- 100hPa)	U.S.	Polar M.S. (6-30hPa)	N.P	S.P.	Ref. data	
CH ₄	A	n	+	+	n	n	/	/	/	
H ₂ O	A,M	--	+	n	n	+	++	+	my BRAM3	
HCl	A,M	-	n	-	+	+	+	y -	my BRAM3	
ClO	M	0	0	-	-	-	+	y n	m BRAM3	
N ₂ O	A,M↑	-	+	+	-	n	+	m +	my BRAM3	
HNO ₃	A,M	0	-	-	--	-	+	y n	my BRAM3	
N ₂ O ₅	A	0	--	n	n	n	/	/	/	
NO _x /NO ₂	A	-	n	-	-	n	+	m +	m MIPAS-REAN01	
ClONO ₂	A	0	n	n	/	n	/	/	/	
BrO	H	/	/	/	/	+	/	/	/	
BrONO ₂		/	/	/	/	/	+	m +	my MIPAS	
O ₃	A,M	+	+	++	n	n	+	y +	my BRAM3	

Note that all explanatory annotations were moved to the caption or to the main text due to a comment by the second reviewer.

As intended, the revised scorecard is less optimistic for the species which you commented about:

- **CH₄**: the performance is downgraded from “good” to “neutral” in the tropical L.S. and from “very good” to “good” in the tropical and mid-latitudes M.S.
- **H₂O**: the performance is downgraded from “good” to “neutral” in the mid-latitudes M.S. and in the U.S.
- **HCl**: the performance is downgraded from “good” to “neutral” in the tropical M.S. and from “neutral” to “poor” in the mid-latitudes M.S.. Interestingly, the revised score is upgraded from “neutral” to “good” in the polar M.S. due to agreement between both datasets.
- **N₂O**: the performance is downgraded from “very good” to “good” in the extra-polar M.S. and from “neutral” to “poor” in the U.S.
- **HNO₃**: the performance is downgraded from “neutral” to “poor” in the mid-latitudes and polar M.S.
- **O₃**: the performance is downgraded from “very good” to “good” in the tropical M.S. and from “good” to “neutral” in the U.S. and polar M.S.

The abstract was updated accordingly:

A scorecard assessment of chemical forecasts in the stratosphere of IFS-COMPO Cy49R1 highlights ~~very~~ good performance for O₃, CH₄, N₂O, and H₂O and ~~good-or~~ adequate performance for HCl, and ClO, and-for BrO and BrONO₂ in the polar lower stratosphere.

...and the conclusions as well:

...the model already delivers ~~very~~ good performance for CH₄, N₂O and H₂O...

Specific comments:

L138: How is the volcanic injection of sulphate species treated in the model? Would be good to mention here or point to the relevant place in the paper.

This helpful comment led to further clarification of the development history of IFS-COMPO, as the Pinatubo simulation experiment (rd.i9vv in Table 2) required a minor improvement of volcanic injection which was developed after the release of Cy49R1. Strictly speaking, the evaluation of this experiment thus concerns Cy49R2. This led to a minor modification of the title of the paper and the title of section 2.3, where “IFS-COMPO Cy49R1” is replaced by “IFS-COMPO Cy49”. The treatment of volcanic injection is now described, first in section 2.1 (for Cy48R1):

The volcanic injection in Cy48R1 concerns only SO₂ and is carried out only over a single grid cell, i.e. each volcano is treated as a point source. The injection specifics (amount injected, latitude/longitude of the volcano, times of beginning and end of injection, minimum and maximum injection altitude) are prescribed in a model namelist. The model determines the matching grid cell and model levels and distributes the injected amount equally between the model levels.

...and at the end of section 2.3 (for Cy49R2):

Volcanic injection was further refined in IFS-COMPO Cy49R2, allowing injection over areas that comprise multiple grid cells and enabling the injection of water vapour alongside volcanic sulphur dioxide. These enhancements support the modelling of the impacts of the Hunga (2022) and Pinatubo (1991) eruptions, respectively.

Throughout the text, the cycle numbers were corrected from Cy49R1 to Cy49R2 where necessary or simplified to “Cy49” where appropriate.

The injection data for the Pinatubo simulation experiment was described in section 4.3 and has been clarified:

For the Pinatubo eruption, a total of 14 Tg of SO₂ was injected on 15th of June 1991 between 18 and 24 km altitude (Sukhodolov et al., 2018). To better take into account the explosive nature of the eruption and local dynamical processes not described by the model, the injection was distributed over a 300 × 300 km area centered on the Pinatubo. The additional impact of the Cerro Hudson eruption is captured by also injecting 2.3 Tg of SO₂ on 15 August 1991, over a 300 × 300 km area centered on the Cerro Hudson.

L212 (Fig. 1): I don't understand the distinction between SO₂ in CB05 and BASCOE. Please clarify in caption or text.

This comment also led to fruitful discussions between the co-authors, resulting in a major revision of Fig.1. While some fields are duplicated between the NWP core of IFS and its COMPO extension (e.g. GO3 and O3; q and H₂O), there is no such duplication between the CB05 and BASCOE modules. These modules compute increments for the same fields, but differently depending on the location of each gridpoint in the troposphere or in the stratosphere. It was thus misleading to distinguish between SO₂ in CB05 and SO₂ in BASCOE. Fig.1 was revised to distinguish the conversion processes activated in the troposphere (module CB05) from those activated in the stratosphere (module BASCOE). This revision also led to a correction of the production process for sulfate in the troposphere, which is converted from SO₃ rather than SO₂.

Here is the revised Fig.1 and its caption:

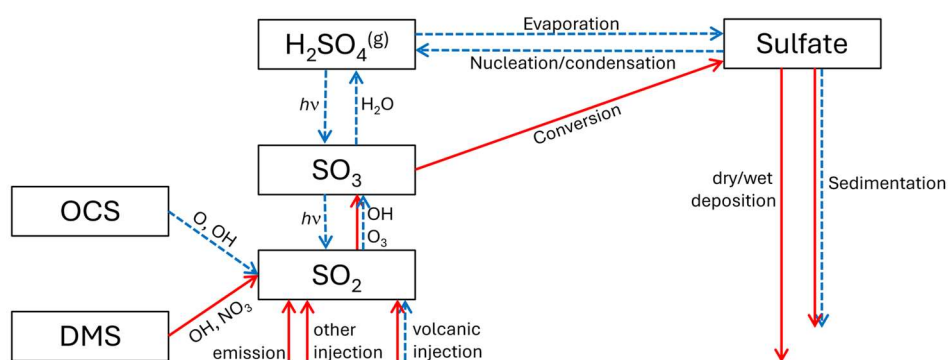


Figure 1. Architecture of the stratospheric sulphur cycle of IFS-COMPO as implemented in Cy49R2. Processes may be activated in the troposphere (red arrows), in the stratosphere (dashed blue arrows) or in the whole column (double arrows).⁴

L225: The variable "c" in Eq. 11 needs to be explained.

c is an adjustable parameter and its adjustment is explained on line 235-237. This has been clarified at L.225.

L403: I agree that the highest mean age values in the stratosphere are below 10 years. However, age spectrum tails extend well beyond. Hence, the statement "oldest air encountered in the stratosphere" is not correct and should be changed.

Done. We simply clarified by using the correct name i.e. "mean Age of Air":

IFS-COMPO was thus spun up during 10 years before this test case, i.e. for a longer time than the **largest mean** Age of Air encountered in the stratosphere (Chabrillat et al., 2018).

L507ff: How is the upper boundary condition treated? Can't this also be a source of bias in the upper stratosphere? Please add some explanation and discussion here.

The upper boundary condition is simply "null flux" for all tracers. This is justified by the location of the uppermost model at 0.01 hPa, i.e. in the mesosphere and far above the highest layer of interest and evaluated in the paper (upper stratosphere, 1-10 hPa). This has been clarified in the text:

In the upper stratosphere, i.e. at pressures lower than 10 hPa, the N₂O biases between IFS-COMPO and ACE-FTS increase quickly to reach or exceed 50% at **the upper limit of our evaluation (1 hPa pressure level)**. (...) **Since the uppermost model level is at 0.01 hPa pressure, i.e. in the mesosphere and approximately 30 km above the upper limit of our evaluation, the upper boundary condition is not expected to play a role in this disagreement.** This suggests that a common process...

L524: Adding age of air tracers to IFS would indeed be very interesting for future work.

Indeed: this led us to repeat this suggestion in the first bullet of the conclusions.

L890: What means "By elimination..." here?

This discussion paragraph was not clear and it did not contribute much to the evaluation. It was thus deleted from the revised version.

Technical corrections:

L72: ... CAMS was upgraded → *done*

L101: Lagrangian → *done*

L142: blank between "aerosols as" → *done*

L197: Cy48R1 - there are also other places where the "R" is lower-case (e.g. L201, L375, etc). Please check the entire manuscript again. → *done*

L307: one "solar" too much. → *done*

L403: "oldest age" sounds awkward, better "oldest air" or "largest/highest age values", etc.
→ *done*: for a longer time than the **largest mean** Age of Air

L415: Would change "Let us compare..." to "In the following, we compare ...", or similar.
→ *This short "linking" sentence sounded awkward and was not necessary. It has been removed.*

L431: Would change "It will be interesting to see how..." to "It is a particularly interesting question how ...", or similar. → *done*: It is an interesting question how...

L664: blank missing "inthe". → *done*

L691: number missing: "~150 and ~hPa". → *done*: this sentence has been corrected and clarified by following increasing pressures (rather than altitudes). It now reads:

The vertical profiles of simulated extinctions match relatively well the retrievals, especially the constant or slow increase of retrieved extinction with increasing pressures from ~5 hPa to ~30 hPa and the stronger increase from ~30 hPa to ~150 hPa.

L748: "in the two control runs". → *done*

L804: blank missing "afterwardan", and better "afterwards ..." → *done*

L884: blank missing "quitesimilar" → *done*

L894: Just simplify to: "To conclude, IFS-COMPO ..." → *done*

L939: "stratospheric" → *done*

L962: blank missing "theagreement" → *done*

L983: blank missing "thisunderestimation" → *done*

L987: "Northern" → *done*

L1044: "spring" → *done*

Figures 4, 5, 6, 7, 9: Is the legend labelling of red solid and dashed lines correct? I guess the solid line should be Cy48... (not Cy49...), as for the blue lines?

→ *Indeed, the red solid lines are for Cy48R1 and not Cy49R1. This is now corrected.*
Thanks for spotting this!

Reviewer #2

General comments

This manuscript presents a thorough evaluation of the stratospheric chemistry aspect of the IFS-COMPO Cy49R1 system. These updates will help the Copernicus Atmosphere Monitoring Service (CAMS) move towards a better stratosphere-troposphere chemistry scheme. The authors detail recent improvements, specifically focusing on advances in the heterogeneous chemistry part of the stratospheric chemistry. They attempt to evaluate the updated scheme using the Mt. Pinatubo-related perturbation as a test case.

These enhancements and detailed documentation will be highly beneficial to the large scientific community that utilizes CAMS data and will serve as a crucial guide for future model development. The manuscript's primary strength lies in the documentation of the updated scheme with some evaluation, rather than new scientific discoveries. This perfectly aligns with the scope and aims of Geoscientific Model Development. The authors have effectively detailed the upgrades and assessed their impact on stratospheric composition, making this a very suitable contribution to the journal. Overall, I recommend the manuscript for publication with minor modifications.

We thank the reviewer for these positive comments.

Comments

1. Decluttering Figures:

Many figures in the manuscript are currently difficult to read due to an excessive amount of text. To improve clarity and readability, I strongly recommend that the authors move explanatory text, detailed descriptions, and other information from the figures into the figure captions or the main body of the text. This will allow for larger tick labels and legends, making the figures much easier to interpret at a glance.

We guess that this comment addresses the figures in section 6 (time series in the polar lower stratosphere) because their legends were too long, and the figure in section 7 because it was formatted as a scorecard. The figures in section 6 have been re-plotted with smaller legends, and different proportions to enlarge the tick labels of the axes. Here is an exemple with the first such figure i.e. figure 11:

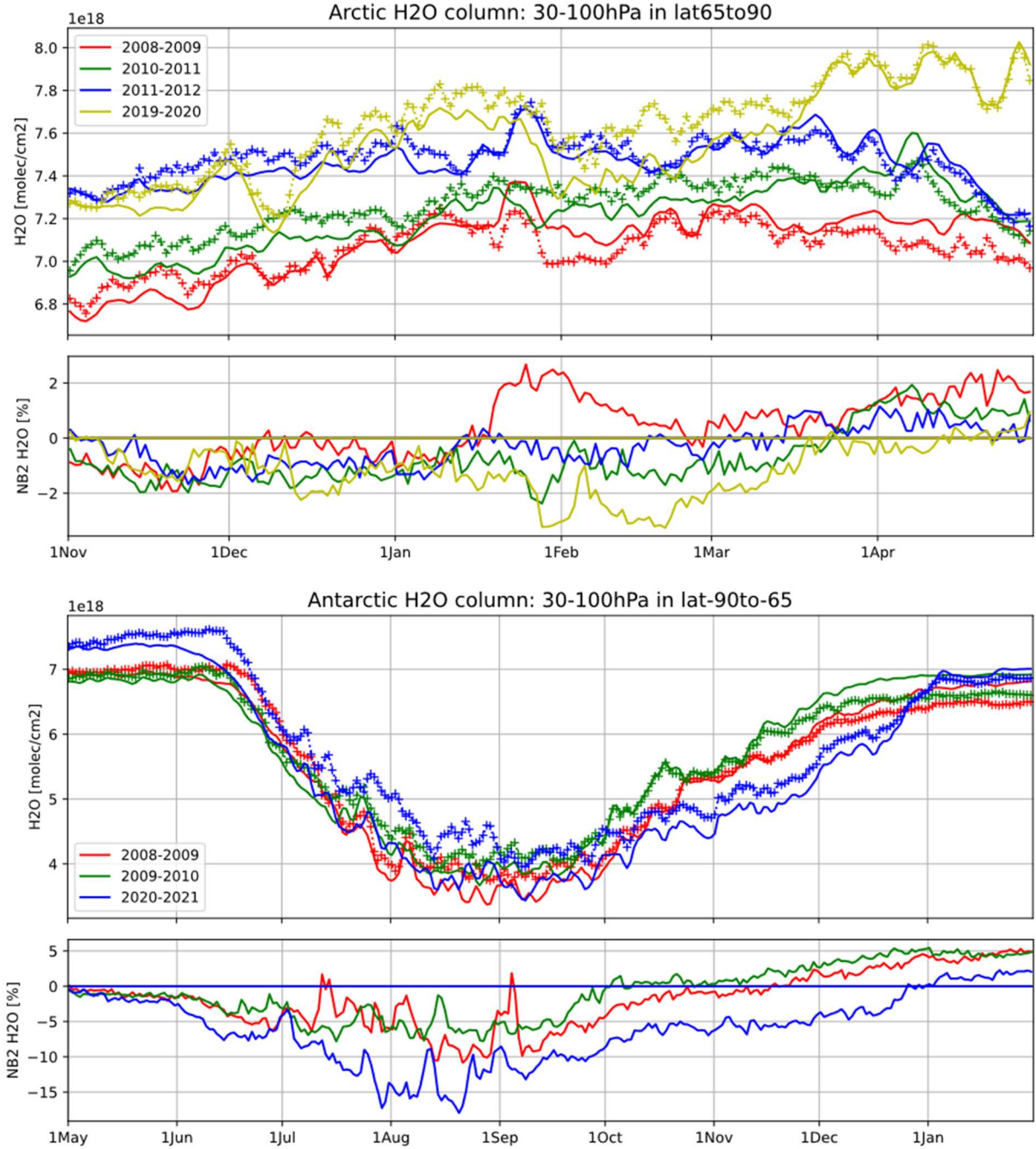


Figure 11. A time series of the H₂O partial columns (30-100hPa, upper panels) in IFS-COMPO Cy49R1 chemical forecasts (solid lines) and in the BRAM3 reanalysis (crosses), and the corresponding normalized biases (lower panels). Comparisons are shown for both the Arctic (65°N-90°N, top) and the Antarctic (90°S-65°S, bottom) for the same years and using the same color scheme as in Fig. 3.

The scorecard figure was simplified, and all explanatory text was moved either to the caption or to the main text in section 7:

The regional scores are determined objectively from the absolute values of the Normalized Mean Bias (NMB), Standard Deviations of differences between model and observations (STD) and corresponding correlations, using criteria chosen to segregate between the four proposed scores while prioritizing bias performance (see Table S1 in the Supplement). These scores are computed separately for each reference dataset (second column: “A” for ACE-FTS; “M” for Aura-MLS) and added for the species where both datasets are available. The total score provides a combined performance assessment, requiring the availability and agreement with both ACE-FTS and Aura-MLS to allow “very good performance” assessment (see Table S2 for details). The assessment for N₂O in the lower and middle stratosphere relied only on ACE-FTS because Aura-MLS shows suspiciously large disagreements at pressures larger than 10 hPa while difficulties were reported in the retrieval of Aura-MLS N₂O v4 (see section 3.3).

The polar lower stratospheric scores summarize in a more subjective manner the ability of the model to capture the timing and rate of changes during winter-spring seasons, i.e. the intraseasonal variability on timescales from weeks to months, as well as the interannual variability, as evaluated in section 6 with two reanalyses of stratospheric composition (see last column and section 3.3 for details). Some regional scores are labelled with a combination of letters *b,s,e* to denote the statistical diagnostics (NMB, STD and Correlation, respectively) which prevent them from reaching a higher score. Similarly, the polar L.S. scores are labelled with a combination of letters *m, y* to note difficulties capturing intraseasonal variability (monthly timescales) and/or interannual variability (yearly timescales).

The 13 gas-phase species included in Table 3 are grouped into families, namely: hydrogen source (CH₄) and reservoirs (H₂O), chlorine reservoir (HCl) and reactive radical (ClO), nitrogen source (N₂O) and reactive gases (HNO₃, N₂O₅, NO_x, ClONO₂), bromine reservoir (BrONO₂) and radical (BrO) and, finally, O₃ which is the primary stratospheric target of CAMS. The score for BrO is based on the bias of the stratospheric column in winter-spring 2008-2009 and 2019-2020 with respect to at the UV-Vis spectrometer at Harestua one Arctic station (“H” in the second column; see section 6.4).

							Winter-spring L.S. (30-100hPa)			
Species	Ref. data	Tropical L.S. (70-150hPa)	Tropical M.S. (6-50hPa)	Mid-lat. M.S. (10-100hPa)	U.S.	Polar M.S. (6-30hPa)	N.P	S.P.	Ref. data	
CH ₄	A	n	+	+	n	n	/	/	/	
H ₂ O	A,M	--	+	n	n	+	++	+	my BRAM3	
HCl	A,M	-	n	-	+	+	+	y -	my BRAM3	
ClO	M	0	0	-	-	-	+	y n	m BRAM3	
N ₂ O	A,M [†]	-	+	+	-	n	+	m +	my BRAM3	
HNO ₃	A,M	0	-	-	--	-	+	y n	my BRAM3	
N ₂ O ₅	A	0	--	n	n	n	/	/	/	
NO _x /NO ₂	A	-	n	-	-	n	+	m +	m MIPAS-REAN01	
ClONO ₂	A	0	n	n	/	n	/	/	/	
BrO	H	/	/	/	/	+	/	/	/	
BrONO ₂		/	/	/	/	/	+	m +	my MIPAS	
O ₃	A,M	+	+	++	n	n	+	y +	my BRAM3	

Figure 19. Scorecard for the performance of chemical forecasts in the stratosphere of IFS-COMPO Cy49R1 against observations and chemical reanalyses for 13 gas-phase species in five regions of the global stratosphere and for the winter-spring polar lower stratosphere. The five regions of the stratosphere are defined by latitudes and pressures as follows: tropical lower stratosphere (L.S.): 30°S-30°N, 70-150 hPa; tropical middle stratosphere (M.S.): 30°S-30°N, 6-50 hPa; mid-latitudes M.S.: 60°S-30° and 30°N-60°N, 10-100 hPa; upper stratosphere (U.S.): 2-6 hPa in the Tropics, 2-10 hPa in the extra-Tropics; polar M.S.: 90°S-60°S and 60°N-90°N, 6-30 hPa. The winter-spring polar L.S. distinguishes the South Pole (S.P., 90°S-65°S) and the North Pole (N.P., 65°N-90°N). The performance is evaluated as “++” (very good), “+” (good), “n” (neutral i.e. acceptable), “-” (poor) or “--” (very poor). For the five regions of the global stratosphere this assessment relies on objective criteria derived from section 5, and for the winter-spring polar L.S. it summarizes section 6 (see text for details and reference datasets). meaning of the score symbols and the definitions of regions are given in the legend. The regional scores are assessed in terms of NMB, STD and correlations (see text and Table S1), while the scores for the Arctic and Antarctic polar lower stratosphere are attributed in terms of time variability (see text).

2. Climatology:

I failed to understand the mean biases of modeled trace gases in a climatological context. Please consider adding an additional figure that compares key long-lived tracers/reservoir species (e.g., O₃, HNO₃, H₂O, N₂O, HCl, CFC-11, CFC-12) against well-established, publicly available observation-based datasets. There are many selective line plots but it is very difficult to understand basic strength and weakness in the chemistry scheme.

I suggest showing 5 to 10 mean latitude/height or pressure cross-sections (e.g., see Chapter 6 in the CCMVal 2010 report; tracer-tracer correlation plots in that report also provide a better guideline to understand issues with transport/chemistry). This approach would allow for a clearer assessment of mean biases across different regions of the stratosphere. For example, a 2x5 type figure could be used, with the first column displaying modeled climatological values from the lower stratosphere to the upper stratosphere, and the second column showing absolute or percent differences with respect to SPARC Data Initiative products (e.g., Hegglin et al., 2021). Alternatively, the authors could utilize TCOM (TOMCAT CTM and Occultation Measurement based data), as described in Dhomse et al., 2023 (data for all these species is available on Zenodo). While the current manuscript includes comparisons with daily satellite retrievals (ACE-FTS, Aura-MLS) and reanalyses (BRAM3), an additional comparison against observation-based climatologies would help readers understand mean biases in the updated chemistry scheme.

This comment makes a lot of sense for the evaluation of a Chemistry-Climate Model. Unfortunately, this approach is not feasible for IFS-COMPO for technical reasons. This limitation constrains our whole evaluation approach and should have been explained in detail. This is addressed in the revised manuscript, primarily by the following paragraph added to the Introduction:

In chemical forecast mode, IFS-COMPO operates similarly to a chemistry–climate model with meteorological fields nudged toward climate reanalysis (Ménard et al., 2020; Davis et al., 2022). Climate model evaluation primarily relies on mean bias estimates from comparisons between decadal simulations and observational climatologies (e.g., Froidevaux et al., 2019). Such long runs are impractical for IFS-COMPO because it is derived from an NWP system and designed for much higher resolutions than climate models: even at low resolution, a decadal simulation would take at least six months. Therefore, our evaluation focuses on the “chemical weather” context, where simulations of roughly one year are compared either with individual observations prior to the derivation of the statistics, or with a reanalysis of the observations over the same period. The next CAMS reanalysis of atmospheric composition, named EAC5, will span more than two decades and be accompanied with a control run with no chemical data assimilation (Flemming et al., 2025). These future large experiments will allow climatological evaluations of IFS-COMPO.

Section 4 (set-up of the modelling experiments) now starts with the following sentences:

As explained in section 1, IFS-COMPO Cy49 is too computationally expensive to run the long simulation required for climatological evaluation. We focus instead on three separate case studies with IFS-COMPO experiments lasting 6 to 24 months and summarized in Table 2. These case studies have been chosen to exemplify the performance of Cy49...

The lower resolution used by the Pinatubo experiment is better explained as it is also due to computing costs:

Lasting more than two years, this experiment would be too costly to run at the T511 resolution currently used for CAMS. Hence the T255 resolution, corresponding to an 80 km grid cell, was selected instead.