

Interactive comment on “C-IFS-CB05-BASCOE: Stratospheric Chemistry in the Integrated Forecasting System of ECMWF” by V. Huijnen et al.

Anonymous Referee #1

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The paper describes an update of the C-IFS model, including a stratospheric chemistry scheme in addition to the existing tropospheric scheme, and using the tropopause to switch between the schemes. This method is also used by other models, and the authors consider the possible inconsistency at the transition zone between the domains.

The paper is fairly short and concise, although there are a few issues that should be improved or made clearer.

I therefore think minor revisions are necessary, although my questions on the relatively short simulation period may require more effort than the other comments.

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Abstract

The abstract should contain some more on the motivation for including stratospheric chemistry in CAMS. Is forecasting skill part of the motivation?

It is stated that this is a first step, which makes me wonder how far away the next step really is.

1. Introduction

Page 1, Line 25: “Also analyses and forecasts of stratospheric ozone directly impact the forecast capabilities of surface solar irradiance” is a bit difficult to understand. Suggest changing to e.g. “Also, the amount of stratospheric ozone directly impact the forecast capabilities of surface solar irradiance, making good stratospheric ozone forecast important”.

Page 3, Line 5: “We have developed a strategy”: Perhaps better to say “We present here a merging ...”. There is no need to state that there is a strategy, and the work done is the actual merging.

2.1 Stratospheric chemistry

Page 4, Line 15: Are the surface area densities (SAD) fixed? No size distribution?

On the fact that you do not do sedimentation, but parameterise it using exponential decay of HNO_3 and H_2O , is this a good approximation?

If you have a situation of very cold temperatures over a long period, the PSCs may sediment out and the SAD will be reduced. Will the parameterisation cause too large

denitrification/dehydration? Figure 9 could perhaps indicate this?

2.3 Merging procedure ...

Page 5, Line 24: 40 hPa as tropopause is very low. Does this occur often? To my knowledge, the tropopause pressure is seldom lower than 80hPa.

Using 40 hPa may be of small consequence, and may even cause O3 production to be better represented.

Page 5, Line 26: “Specificaly” -> “Specifically”.

Page 5, Line 34: 10-day decay rate in the stratosphere. What happens to the lost species? Are they assumed to be converted to aerosols?

Page 6, Line 11-14: It would be very interesting to see how chemical composition changes when compared to this test. I think some more info should be given on this, as it could explain chemically why you use two domains.

2.3.1 Merging photolysis rates

Page 6, Line 26-27: How is the interpolation/merging done? Some weighting for different layers?

Page 7, Line 1: The merging only affects 4 layers, where some are in the troposphere, so I am not surprised the over-all impact is small. Perhaps it could be noted that JO3 does not seem to be used in troposphere?

Did you check hourly composition at these grid boxes?

2.3.2 Merging tracer transport

This is perhaps not really a merging.

Regarding stratospheric H₂O tracer: So you do not use tropospheric H₂O (from q) as source of stratospheric H₂O? I would expect some boundary condition is needed, at least in the tropics. How is this treated?

3.1 Observational data ...

Page 8, Line 21: “weighted part”: What kind of weight?

Page 8, Line 23: What is the model output frequency? It would be helpful to specify this and whether you use instant model values.

4 Model evaluation

Page 9, Line 14-20: The equatorial low bias and high bias at NH mid-lat could indicate too fast transport away from Equator?

Figure 2: The simulated time period is not long enough to make clear whether the bias will build up. This should be further explained.

Ozone hole: Do you overestimate PSCs (as commented earlier) and hence halogen activation?

Page 9, Line 20-32: Is the comparison done by extracting the same profiles (location and time) as in sondes? Should be specified.

The sonde comparison should include some variability in sondes (e.g. as standard

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deviation), and possibly also from the model (Fig.3-5). (In Figure 3-4 this could be placed in either upper or lower row.)

Figure 6: How is OH in IFS vs BASCOE? Do you have less HNO₃ in stratosphere because of hotter photochemistry? Some thoughts/explanation should be given for the higher low-SZA NO₂ in IFS than in BASCOE. NO₂+NO₃ <-> N₂O₅ at night? Photochemistry?

Page 10, Line 3-4 (Figure 7): NH higher NO₂: Could a possible reason be that SCIAMACHY assumes that a too high fraction of the column is located in the troposphere?

Page 10, Line 19: What about too fast horizontal transport? Is CH₄ fixed at surface?

5 Conclusions

Page 11, Line 3: I generally do not think 1.5 years is enough for evaluating the chemistry and chemistry. A possible drift in the O₃ column (Fig. 2) should be investigated. If chemistry is adjusted in an assimilation system, a drift will probably not be very prominent or important, but used as a CTM 1.5 years is short.

Page 11, Line 9-11: “a larger error” -> “larger errors”, “was” -> “were”, and fix rest of sentence.

Page 11, Line 12: Why is it necessary to do this first step? It seems another step is expected.

Page 11, Line 18: What use would assimilation of long-lived species have in the IFS?

Page 12, Line 6: While monitoring capabilities may be important and interesting in the stratosphere, and also provide global products of species which are not globally observed, it would be interesting to hear the implications for forecasting.

Is it not only O3 that is important for radiation calculations? Better stratospheric O3 could improve stratospheric temperatures?

Appendix

Table A1 should be sorted on names.

References

DOIs are missing for some references; please update.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-40, 2016.

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