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

Anonymous Referee #2

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The purpose of the paper is to describe and benchmark a new version of the IFS model. This version has separate chemistry modules (mechanisms) for the troposphere and stratosphere, where the decision of which module to call is determined by the altitude of the grid box with respect to the tropopause. The stratospheric chemical mechanism comes from an assimilation system (BASCOE). The previous version of the IFS had tropospheric chemistry plus linearized stratospheric O3. The paper concludes that a new simulation that uses both chemical mechanisms (called CIFS-TS) has good stratospheric O3, NO2 and other reactive trace gases compared to satellite data sets.

A goal of the paper is to demonstrate that their method of using the tropospheric mechanism/solver for tropospheric grid boxes and the stratospheric solver for stratospheric

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grid boxes is a computationally efficient way to calculate the full chemistry of the atmosphere. The biggest problem with this paper is that they did not actually test this. To demonstrate their method, they need to have run a simulation where tropospheric and stratospheric reactions were solved TOGETHER and NOT split into 2 mechanisms. Those results could then be compared with their ‘split’ method. Ideally, this would show that their method was faster (how much faster?) yet produced essentially the same results. I recommend they do this and then rewrite this manuscript. This experiment would not only satisfy the stated goal of the paper but it would also eliminate the confusion in the comparisons (see below) regarding transport/advection differences between BASCOE-CTM and the CIFS-TS.

I don’t agree with the statement in the abstract that the new model configuration shows good performances of stratospheric O3, NO2, and other tracers. The figures chosen to demonstrate good representation of various stratospheric constituents in the CIFS-TS model generally show fair to poor agreement with observations. Stratospheric O3, for example, often looks worse (or at least no better) that it did in the CB05 (trop only) or BASCOE (strat only) versions. This new model does not appear to be an improvement over previous model versions.

The comparisons between CIFS-TS and BASCOE-CTM are confusing. When stratospheric species such as NO2, HNO3, and O3 are compared, the results are different. I thought the primary goal of the paper was to compare the chemical mechanisms, but since the results are rather different, there must be transport (or meteorological field) differences too. This is alluded to on page 7, lines 26-28. The transport/advection needs to be the same in the two simulations in order to compare the chemical mechanisms. It should be made clearer in the text what the differences are between CIFS-TS and BASCOE-CTM.

My overall recommendation is to test a combined (strat+trop) solver and compare the results to trop only, strat only, and the ‘split’ method presented here. The results will provide a good benchmark and will be easier to interpret if all experiments are per-

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formed with the same transport code and meteorological fields.

Other points

It would be helpful to add a table that lists the specifications of each of the models used and notes how dynamical fields are obtained (e.g., forecast, assimilation, . . .?), chemical mechanism, resolution, etc. For example, BASCOE is an assimilation system, but it's only the BASCOE stratospheric chemical mechanism that used here, right? And BASCOE-CTM means the assimilated (renanalysis) fields have been saved and then are being used in an offline chemistry transport model? Presumably it is the same offline model that the C-IFS forecast fields are used in? If what I am asking does not make sense, please take this as an indication that I am confused by the descriptions of the models.

Regarding 'tracer species' or similar expression found in many places, 'tracer' means a species that is unreactive and can be used to trace something, like transport. I think you mean 'trace gas' rather than tracer because that can be used in a general way to talk about any type of constituent in the model. Please search on 'tracer' in the document to identify where you mean trace gas or constituent.

p. 3, l.24. Are you saying the chemistry in the modules is parameterized? Or are you referring to the chemical mechanisms when you say 'chemical parameterization'? A parameterization for chemistry is not the same thing as a chemical mechanism. Some times 'chemical schemes' is used, which is fine for referring to the mechanism. This confusion occurs throughout the paper. Please check each occurrence of 'parameterization' to verify the right words were chosen.

p. 4, l. 15. The threshold temperature for NAT formation is pressure dependent. The manuscript indicates that 194 K was chosen as the threshold regardless of pressure. That would not be the correct way to calculate it.

p. 5, l. 31. I don't understand what is meant by O1D and O3P being described implicitly, as opposed to being treated explicitly.

p. 6, l. 24. ‘solar radiation reaches the stratosphere earlier than the surface…’ as written this sounds like it is referring to delay caused by the speed of light! I doubt this was intended; it needs better wording.

p. 7, Section 2.3.1. For JO3, the lack of a ‘jump’ in O3 may be because photolysis is unimportant (slow) near 100 hPa, so O3 is probably long-lived relative to the photochemical lifetime. JNO2 is much larger so I’m not sure why there isn’t a jump – can you explain this? It would be useful if you showed the simulated O3 and NO2 profiles in Fig. 1 to demonstrate the lack of a jump. What is the meaning of ‘JO3_TB’ in the title of one plot? No similar title for the other plot.

Section 2.3.2, l. 8. It’s unclear whether you’re saying NO, NO2, and H2O have the mass fixed applied or whether they are the few species where the mass fixed isn’t applied. How badly is H2O not conserved in the stratosphere? This will conceivably cause problems for stratospheric chemistry. It would be useful to see a 1-year time series of the H2O mass above 100 hPa.

p. 7, last 3 lines. This sentence says you are looking to identify differences in transport schemes. This confuses the issue of evaluating the chemical mechanisms (and their implementation). This evaluation should be performed using the same dynamical fields with the same model. If the advection schemes are also different, then we cannot actually test the impact of chemical mechanisms alone. And does ‘parameterization’ in line 28 refer to the different chemical mechanisms?

p. 8, l. 27, ‘first Science Satellite’?

p. 8, l. 30-31. Suggest to change to ‘…between 6-30 km agree to within 15% of independent …” For all the figures that are line plots (starting with Figure 2), the blue and black lines are hard to distinguish. Please do something with the line thickness and colors to improve readability.

Section 4, Model Evaluation p. 9, lines 14-19. This paragraph would benefit by a general statement of the purpose of this comparison. It appears the purpose is to show that the TS mechanism looks more like the observed total column O3 than does the trop-only code (with linearized strat O3). One would expect the TS O3 to be better than the linearized O3 of CB05, but there should also be a comparison with the strat-only code. Comparing with the O3 results in Fig. 6, I think the strat chem O3 columns would be lower than the TS mechanism. I guess they aren't the same because the BASCOE-CTM has different transport. Again, not having the same transport in all the simulations really interferes with a useful comparison.

p. 9, discussion of Figs. 3-4. I do not agree that there are meaningful, reduced biases in the TS version. The linearized O3 chemistry of the trop mechanism gives different results from the TS version, but not really worse. These figures show that TS is not an improvement over trop-only. I think the use of mPa for the O3 bias (lower panels) is misleading and probably minimizes the appearance of the disagreement in the middle stratosphere.

p. 9, discussion of Fig. 5. I cannot tell the difference between obs and CIFS-T lines in the figure. There is no line color/style for the observations in each panel's legend. The TS O3 agrees with one of the black lines (obs or CIFS-T??) near and below 100 hPa – sometimes – but the TS O3 consistently has poor agreement above 50 hPa. Why? Since the TS (red) line often does not agree with either black line – I see no basis for claiming good agreement. Additionally, Syowa is often near the vortex and has large daily variability. Were the simulated profiles used in this figure calculated from the same days of the month as the Syowa data?

p. 9, lines 31-32. If you made a difference plot between MIPAS and the simulations, then you might be able to say whether there is good agreement. As presented, the conclusion can't be drawn that there are 'small biases'. Near the tropical maximum the TS looks slightly better than the BASCOE-CTM. Again, assuming that some of the differences are due to dynamical fields or advection scheme, this comparison isn't very

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useful.

p.10, lines 5-9. What is the message here? The CIFS has a terrible high bias in nighttime NO2 and a large low bias in HNO3. Why is the CIFS simulation worse than BASCOE? There is no clear explanation here.

p. 10, lines 10-20 (Fig. 8). N2O and CH4 profiles do NOT assess vertical transport. Their profiles below ∼10 hPa represent a balance between the vertical and horizontal components of the residual mean circulation. That balance depends on latitude, that is, whether the profile is from the tropical upwelling region or somewhere in the midlatitudes (horizontal and vertical motions matter and so does mixing), or isolated inside the polar vortex (descent). Above 10 hPa, profiles are more strongly influenced by chemical loss so the 2 simulations should look very similar there. The CIFS-TS simulation tends to look worse than the BASCOE CTM or the observations between 10-50 hPa. This suggests circulation and/or mixing problems in the tropics and SH. O3 at 20 hPa is strongly influence by chemistry, not just transport. These paragraphs indicate a lack of understanding of transport circulation and its diagnosis, as well as any understanding of what controls stratospheric ozone distributions.

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