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Atmospheric Chemistry and Physics Discussions

Interactive comment on "NOGAPS-ALPHA model simulations of stratospheric ozoneduring the SOLVE2 campaign" *by* J. P. McCormack et al.

Anonymous Referee #1

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This paper describes an evaluation of a new NWP system for the stratosphere, applying different parameterized ozone chemistry schemes. The evaluation includes comparisons with space-borne ozone profiles and air-borne LIDAR observations in two case studies in which the results are compared with the ozone forecasts from ECWMF. Given the difficulties of ECMWF (both the reanalysis, ERA40, as well as the operation data, OD) to represent several important aspects of the (large-scale) stratospheric circulation, the introduction of a data assimilation system (DAS) that focuses on the stratosphere may be very interesting and potentially useful. Further, exploring the use of ozone in weather forecasting systems is important, given its potential use by the scientific community. Therefore this model evaluation is relevant for publication in ACP. However, before publication the following comments should be addressed.

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GENERAL COMMENT

It is known that DAS winds suffer from inaccuracies in the wind fields, which are particularly manifested in a too strong residual circulation in the stratosphere. This is likely due to limitations of the assimilation procedure, and hampers integrations, particularly on seasonal and yearly timescales, but probably also in shorter-term forecasts (see specific comments). To my opinion, this is currently the most important problem with DAS winds, at least concerning NCEP and ECWMF products.

I would like to authors to outline what their aim is with this new system. More specifically, the authors should emphasize more clearly the benefits and improvements relative to the ECMWF products. This is especially relevant, since this new system focuses particularly on the stratosphere. In the light of your concluding remark that your new system is well suited for stratospheric tracer transport, longer-term integrations would have been more appropriate to justify such a conclusion. Instead, the authors decided to use short periods only to test their "initial performance of the new initialization". The evaluation is therefore directed more towards differences between GOES4 and ECMWF than to differences between NOGAPS-ALPHA and ECMWF. Sections 4.2 and 4.3 provide such examples (see specific comments). I encourage the authors to make clear why they focus on these short times scales, since it obscures a "real" comparison between ECWMF and NOGAPS-ALPHA and thus the benefit of NOGAPS-ALPHA. A much more interesting or "real" evaluation would be a comparison of the large-scale circulation between both ECMWF and NOGAPS-ALPHA. This can either be performed by comparing the TEMs and/or by performing an age-of-air type experiment. Figure 5 is already a good first step, which should be extended. It is very interesting to have a new NWP that focuses more on the stratosphere given the problems of current widely used meteorological data assimilation systems, such as NCEP and ECWMF. It would therefore be a pity if the introduction and evaluation of a new model does not address one of the most important shortcomings of current DAS.

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SPECIFIC COMMENTS

Abstract: The conclusions in the abstract seem much stronger than in the Conclusion section and both should be synchronized.

Section 2.6: The ozone climatology (r0 and Σ0) is taken from Fortuin and Kelder (1998). Does this choice lead to inconsistency of the parametrization if the climatological temperature field (T0) remains unchanged? For example, the coefficients depend on the ozone concentration itself. From experience I know that the temperature deviations of the model temperatures with climatological levels can be substantial, creating numerical instability of the parametrization. We tried to solve this with "some" mean state representation of the temperature, either derived from the ECWMF or from another source. Have you experienced this and if so, how did you solve it? Are all ozone parametrizations using the same ozone climatology?

Section 4.1: Could the authors be more specific with the word updated?

Section 4.2: There are some issues with the ozone data assimilation in the ECMWF. The assimilation using total ozone column data is restricted to 40S-40N. The reason is that the KNMI fast-delivery product (from GOME) has a low bias outside this region, while those based on SBUV/TOMS7 have a high bias, particularly over the south pole (TOMS version 8 is has quite improved). In addition, it was recognized that ECMWF persistently contained a high bias at high northern latitudes in wintertime. The reason for the high bias is suggested to originate from the too fast general circulation in ECMWF (see general comment). For all these reasons it has been decided (somewhat arbitrary to my opinion) to assimilate the observations in the 40S-40N region only. This is a much more restricted area than up to the polar regions, as stated by the authors. If in GOES4 the data is assimilated throughout the model domain, the difference in assimilation area is most likely the reason for this discrepancy, rather than the lower spatial sampling rate. This is a clear example how the model NOGAPS-ALPHA evaluation is affected by the GOES4 performance (see general remark). It would therefore

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Interactive Comment

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be interesting to compare both systems within the 40S-40N region.

Sections 4.3: I like this experiment (Figure 10 and 11). The most striking aspect to me is large difference in model performances between figure 10 and 11, especially given the fact that the sampling locations of both POAM and SAGE are very close. This particularly concerns ECMWF and the use of CD86, but also other runs. Could you explain this?

Could the good agreement of the ECMWF with SAGE between 50-100 hPa not be due to coincidence, as a result of the large overestimation aloft combined with downward transport?

You state that ECMWF and NOGAPS-ALPHA exhibit the same ozone tendency when CD86 is used. But is this not to be expected when the same ozone climatology is used?

Concerning the model experiment in Figure 12, you conclude that the difference between CD86 and CHEM2D can impact ozone in the lower stratosphere in several days. But I was wondering whether this conclusion can be drawn on basis of these experiments. There was no run with equal initialization and different parametrizations. The runs with CHEM2d and CD86 were performed with different initializations.

Section 5.4: Perhaps Figure 19 and 20 are the most interesting for comparison between ECMWF and NOGAPS-ALPHA. I do not understand why the NOGAPS +114 hrs forecasts are so much different from ECMWF. I would like to see the differences between Figure 19 and 20 examined in more detail. Why do the ECMWF fields look so much smoother while the ECWMF results contain a higher spectral resolution than NOGAPS-ALPHA? Is it the chemistry that yields such differences? If so, a sensitivity experiment with different chemistry parametrizations would be needed.

I'm somewhat confused with the mix between hindcast and forecasts. Sometimes both wording is used for the same experiment. As far as I understand all experiments are

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forecasts.

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