

Interactive comment on “Impact of meteorological analyses and chemical data assimilation on modelled long-term changes in stratospheric NO₂” by L. N. Gunn et al.

Anonymous Referee #2

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The authors have used a three-dimensional chemical transport model (CTM) to assimilate observations of CH₄, H₂O, HCl, and O₃ to provide an improved simulation of the year-to-year variations and trends in stratospheric NO₂ between 1992 and 2000. They showed that assimilating long-lived tracers such as CH₄ improved the distribution of stratospheric NO₂ as measured at Jungfraujoch, Issyk-Kul, Tenerife, and Lauder. A conclusion of the study is that meteorological analyses have errors that make them unsuitable for CTM studies of long-term trends. The authors also conclude that assimilation of long-lived tracers, exploiting the tracer-tracer correlations in the CTM, offers a means for correcting for the impact of the transport errors in the analyses. They also suggested that the assimilation enables one to test the chemistry in the model. The

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use of tracer-tracer correlations of long-lived tracers in the assimilation is indeed a useful way to compensate for model errors. However, I cannot recommend the manuscript for publication in ACP in its present form. I do not believe that the main results are new or insightful. It is now well known that meteorological analyses should be used with care when examining long-term trends. Furthermore, it is also well known that ERA-40 provides a poor description of the circulation in the stratosphere, as evidenced by the young mean ages of air it produces. As discussed below, I would suggest that the authors revise the manuscript and conduct a more detailed analysis to demonstrate how the assimilation can be used to help understand the impact of model errors (transport and chemistry) on the distribution of stratospheric NO₂.

General Comments

1) Since it is well known that ERA-40 does not capture the stratospheric circulation well, the poor performance of the model in simulating the observed NO₂ is not surprising. Furthermore, Gil et al. (2008) showed that assimilating the long-lived tracers significantly improves stratospheric NO₂ in the model. The manuscript does extend the Gil et al. (2008) study by incorporating NO₂ observations from Jungfraujoch, Issyk-Kul, and Lauder. However, simply showing that the modeled NO₂ with assimilation is also better at these locations is a really a rather modest contribution beyond the Gil et al. (2008) study.

On page 12026, lines 20-23 the authors state that they “extend the observation-based study of Gil et al. (2008) by analyzing the modeled trends over the period 1992-2002, by studying a range of stations and by investigating the cause of the model improvements when assimilation of a long-lived tracer is included.” However, the authors did not present an investigation of the cause of the model improvements. It would be interesting if the authors did this and showed how the assimilation can be used to explain the differences in the spatial distribution of NO₂ observed at the four locations considered here. For example, the seasonal cycle as well as the increase in NO₂ between 1992 and 2000 is greater at Lauder than at Jungfraujoch, and the assimilation captures

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the variability well at Lauder. What are the errors that are being compensated for in the assimilation that leads to the improved NO₂? It would be interesting to see the change in the latitudinal distribution of NO_y after assimilation. Using the tracer-tracer correlations in the assimilation, can the authors explain why the ERA-40 fields resulted in a larger NO₂ bias in the southern hemisphere? It might be helpful to incorporate into the analysis aircraft data from the Airborne Southern Hemisphere Ozone Experiment and Measurement for Assessing the Effects of Stratospheric Aircraft (ASHOE/MAESA) campaign. The March and April 1994 ASHOE/MAESA data might provide an interesting case study to help understand what is happening over Lauder in the model with and without assimilation of the long-lived tracers.

2) The observed NO₂ column densities are larger at Issyk-Kul than at Jungfraujoch, even though they are at similar latitudes. In contrast, the modeled NO₂ columns, with and without assimilation, are similar at these two stations. Does this suggest that there is a bias in the NO₂ observations at Issyk-Kul? Or is there a deficiency in the model that is not compensated for in the assimilation? With a more detailed analysis the authors would be able to provide some insight as the cause of this discrepancy.

3) The authors state that the assimilation “allows a more direct test of the model’s chemistry.” I would suggest that the authors take advantage of this capability and conduct a more quantitative analysis of the model chemistry rather than the simple comparisons between the modelled and observed NO₂. In particular, the authors acknowledge that the model does not reproduce the large values in the ratio of sunset to sunrise NO₂ columns at Tenerife and Issyk-Kul in winter. Why is that? Furthermore, why are the ratios larger at Issyk-Kul than at Jungfraujoch? There are fewer measurements at Issyk-Kul, so could the high ratios there be a data artifact? On the other hand, the wintertime ratios at Tenerife and Issyk-Kul are not that different from the wintertime values at Lauder, and the model is able to capture the high ratios at Lauder. What is the source of this discrepancy between the two hemispheres in winter?

Specific comments

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1. Page 12028, line 25: When you say the all tracers in the model are “overwritten at the surface”, do you mean that you specify a concentration boundary condition at the surface for the tracers? This is unclear.

2. Page 12028, lines 28-29, and page 12029, lines 1-10: It would be helpful to the reader to include in the manuscript a brief description of the assimilation system and a more detailed summary of how the tracer-tracer constraints are employed in the assimilation.

3. Page 12031, line 2: The differences between run B and the observations are not always small. For example, between 35N-60N, at 56 hPa, the differences between run B and the observations after 1995 are comparable to the differences between run B and run A. In addition, it would be more useful to compare the model and observations at the observation time, rather than using an average of the data.

4. Page 12031, lines 5-6: In explaining the differences between the assimilation and the observations the authors stated that the “assimilation scheme considers errors in the observations and model before producing a best analysis.” Are they suggesting that the large discrepancies between the assimilated fields and the observations reflect large analysis errors? If that is the case, a discussion of the configuration of the assimilation system would clearly be helpful to the reader. Is this due to the observation errors or the background errors?

5. Page 12031, lines 26-27: The authors claim that the positive trend between 1992 – 2000 “is due both to increasing stratospheric NO_y due to increasing N₂O and decreasing aerosol following the eruption of Mt. Pinatubo in 1991.” My understanding is that these changes in NO₂ were due mainly to the recovery from Mt. Pinatubo. Indeed, Gil et al. (2008) suggested that there was no statistically significant trend in column NO₂ at Tenerife between 1993 – 2006, which would suggest that the increase in N₂O is not an important contributor to the changes observed between 1992 – 2000. Furthermore, it would be helpful if the authors actually calculated the trend between 1992 – 2000 and

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assessed its statistical significance.

6. Page 12032, line 19: Are the values in Table 2 the differences between the mean of the observations and the model or are they the mean of the differences between the observations and the model sampled at the observation locations and time? This should be explained in the text.

7. Page 12032, lines 26-28: Why are the observed NO₂ columns at Issyk-Kul greater than those at Jungfrauoch? Please see my general comment #2 above.

8. Page 12034, lines 7-9: Why does the model show less variability in the sunset to sunrise ratios than observed and why does it miss the enhanced ratios at Tenerife and Issyk-Kul? Please see my general comment #3.

9. Figures 1 and 2: The observations are indicated by solid black lines in the plot, not dotted lines as indicated in the legend. Please change the legend. Also, these figures are too small. It is difficult to see the individual lines in each panel.

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