Atmos. Chem. Phys. Discuss., 12, C7140–C7143, 2012
www.atmos-chem-phys-discuss.net/12/C7140/2012/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Impact of meteorological analyses and chemical data assimilation on modelled long-term changes in stratospheric NO<sub>2</sub>" *by* L. N. Gunn et al.

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Received and published: 19 September 2012

*Response to Comments on*

"Impact of Meteorological Analyses and Chemical Data Assimilation on Modeled Long-Term Changes in Stratospheric $NO_2$"

by L. Gunn et al.

We thank the referees for their comments and suggestions. Here are our responses.

**Reply to referee #2**

> *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 $NO_2$ is not surprising. Furthermore, Gil et al. (2008) showed that assimilating the long-lived tracers significantly improves stratospheric $NO_2$ in the model. The manuscript does extend the Gil et al. (2008) study by incorporating $NO_2$ observations from Jungfraujoch, Issyk-Kul, and Lauder. However, simply showing that the modeled $NO_2$ 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 analysing 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 $NO_2$ observed at the four locations considered here. For example, the seasonal cycle as well as the increase in $NO_2$ between 1992 and 2000 is greater at Lauder than at Jungfraujoch, and the assimilation captures the variability well at Lauder. What are the errors that are being compensated for in the assimilation that leads to the improved $NO_2$? 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 $NO_2$ 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)*



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*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.*

The manuscript does only include another 3 sites in extension to the Gil et al (2008) study but it also explains the assimilation in more detail and shows how we improve not only $NO_2$ but all $NO_y$ species. The 4 sites that are choose are some of the only sites that have such long record sets. This is another reason for their choice.

In response to the comment that we did not present an investigation of the cause of the model improvements this was the intended point of the validation against other independent data. This was obviously not clear in the text so we will explain further in the revised manuscript. We will also investigate further the spatial distribution differences in $NO_2$, as suggested.

Due to the time constraints we will not have the opportunity to include aircraft data to explain the differences between the northern and southern hemisphere. We will however investigate this and make clear our finds in the revised manuscript.

$>$ *The observed $NO_2$ column densities are larger at Issyk-Kul than at Jungfraujoch, even though they are at similar latitudes. In contrast, the modeled $NO_2$ columns, with and without assimilation, are similar at these two stations. Does this suggest that there is a bias in the $NO_2$ 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.*

Unfortunately it has not been possible to get hold of the principal investigator at the Issyk-Kul site to obtain more details during the revision process. There have been suggestions that the data at Issyk-Kul has not been bias corrected however it is very



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difficult to answer these questions without direct contact with the team who take the measurements. If we cannot establish this before the resubmission deadline we will remove the Issyk-Kul data.

> *The authors state that the assimilation allows a more direct test of the models 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 $NO_2$. In particular, the authors acknowledge that the model does not reproduce the large values in the ratio of sunset to sunrise $NO_2$ 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?*

We will investigate this and add a more detailed discussion (with the proviso that the Issyk-Kul data might be removed).

**Specific comments**

Will take onboard of specific comments and update the manuscript accordingly.

**References**

Gil. M., M. Yela, L.N. Gunn, A. Richter, I. Alonso, M.P. Chipperfield, E. Cuevas, J. Iglesias, M. Navarro, O. Puentedura, and S. Rodriguez, $NO_2$ Climatology in the Northern Subtropical Region: Diurnal, Seasonal and Interannual variability. *Atmos. Chem. Phys.*, **8**, 1635-1648, (2008).



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