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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Persistence and photochemical decay of springtime total ozone anomalies in the Canadian Middle Atmosphere Model" *by* S. Tegtmeier and T. G. Shepherd

Anonymous Referee #1

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General:

This paper is following the discussion of observed ozone anomalies as given by Fioletov and Shepherd (2003) and applies the methodology as a process-oriented diagnostic to CMAM output. The paper argues that they can demonstrate that this technique "is a robust diagnostic of model performance" on the basis that they get good agreement between CMAM and observations for the anomaly decay.

I have strong concerns about the attitude of the paper in trying to sell something that agrees well in a context of many visible and partially discussed model deficits as "a robust, process-oriented diagnostic". I have no problems with contributions assess-

ing critical model deficits, but if anything, this paper is actually demonstrating that the "ozone anomaly decay" is actually not a good tool to understand processes that are problematic in models. Certainly, I don't have any issues with the methodology as such, it is a useful technique in the context of ozone trend assessments of (seasonal) trends; I just doubt that this methodology is used here in the right context.

I would request a major change in the presentation and discussion of the results, making it much clearer to the reader what component of the model is actually validated with this approach. My suspicion is, that it comes down to the "mean photochemical lifetime" of ozone (a number reasonably well known), only slightly modified by the "quality" of the transport scheme.

Specific issues:

Even though I understand why the use of only annually varying SSTs might cause an underestimate in inter-annual variability, it is not clear to me why the annual cycle as shown in figure 2 is so severely underestimated (that might be explained somewhere in the CMAM literature). Nevertheless, I would assume the ozone change from October to January should be linked quite strongly to transport - but apparently this is not picked up by the proposed process-oriented diagnostic. This is presumably linked to the statement that the methodology chosen is not sensitive to how the anomaly was formed (or on what "background")?

It is also interesting to note that the proposed process-oriented diagnostic does not care about a wrong "set-up" of the chemistry (e.g. no heterogeneous loss). Even though I appreciate the validity of such runs as sensitivity studies, their use here clearly illustrates that the proposed process-oriented diagnostic is not a useful tool to assess models, because it does not pick up major deficiencies affecting ozone.

The summary starts with "The persistence ... is very realistic.", after the previous section closed with "mid-latitude anomalies decay rapidly ... large impact ... of the CMAM polar anomalies.". So, we are first presented with a nice summary with what is

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wrong in the model, but then the authors argue for a "very realistic model". I am left baffled! To argue on this basis that the chosen methodology is "robust and process-oriented" is not clear to me.

In summary, I don't think the manuscript should be published in this form. It needs a critical re-work under the aspect of what information a process-oriented diagnostic is required to provide in a certain context. I don't think we need process-oriented diagnostics that hide model deficits. The attitude of the paper should change - it should stress the usefulness of the methodology applied to trend assessments, but should provide a more critical assessment of the methodology used as a model diagnostic. A possible route may be: No don't use it as a process-orientated diagnostic; it is insensitive to most important deficits in the model - which is good because you may still be able to get some useful information about seasonal (dynamically induced?) trends from your model when you are aware of your model deficits (e.g. too small annual cycle, biases, limits in chemistry) and use the technique appropriately.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3403, 2006.

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Interactive Comment