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Interactive comment on “Technical Note: A simple procedure for removing temporal discontinuities in ERA-Interim upper stratospheric temperatures for use in nudged chemistry-climate model simulations” by C. McLandress et al.

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Reply to Reviewer 2

We thank the reviewer for his or her comments, which are repeated below in italics.

This note describes an adjustment procedure for removing global-mean temperature

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discontinuities in ERA-Interim reanalysis data. These data are being used by various modeling groups to constrain the dynamical behavior of chemistry-climate models. The authors show that using unadjusted data can lead to unphysical behavior of calculated chemical species whose mixing ratio depends strongly on temperature, such as ozone in the upper stratosphere.

The paper is acceptable for publication in ACP after minor revision, as detailed below.

Specific Comments (page, line):

(25803, 1) “nudge or relax” Please be more specific. What is done in the models is to linearly combine the computed state with the reanalysis state at each time step. This is tantamount to applying a linear relaxation of the model-computed fields to the reanalysis data. The magnitude of the effective relaxation coefficient that results from this procedure depends on the coefficients used to blend the model and reanalysis states and the size of the model time step. It is probably worth noting, then, that the effective relaxation coefficients depend on the modeler’s choices of these parameters.

We fully realize that nudging is simply a Newtonian relaxation of the model fields to the reanalysis, but did not want go into a discussion of that in our note. However, we do feel we could have been clearer about what we meant by nudging. So in the abstract we have added “(relaxed)” after the first occurrence of “nudged” and in the 2nd paragraph of the introduction we now say that the nudging is done using “Newtonian relaxation”.

As to noting that “the effective relaxation coefficients depend on the modeler’s choices of these parameters,” we don’t see this as necessary since our paper is not about the

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nudging procedure itself.

(25806, 7) "The deseasonalized time series is then fit..." A couple of predictors related to the quasi biennial oscillation (QBO) are often included in fits like this. Perhaps such predictors are superfluous in the upper stratosphere (1 hPa), where the amplitude of the QBO becomes small. What about at other levels? Have the authors determined that fitting to the QBO is not needed even in the middle stratosphere, e.g., at 7 hPa? Or is it the case that any QBO signal is diluted to the point of irrelevance in these global data?

The QBO certainly affects upper stratospheric temperature, but the direct effect is through the induced meridional circulation, which to a first approximation (as with the Brewer-Dobson circulation) does not affect global mean temperature. See Figure 5 of the Baldwin et al. (Reviews of Geophysics, 39, 179-229, 2001) review paper, where there is a strong temperature response to the QBO in the upper stratosphere but there are comparably sized positive and negative anomalies at the same altitude. The QBO also affects ozone in the upper stratosphere, but it's mainly via NO_x around 10 hPa and mainly via temperature around 5 hPa (Tegtmeier et al. J. Geophys. Res., 115, doi:10.1029/2009JD013010, 2010). For these reasons we did not expect to find a strong signal of the QBO on global-mean temperatures in the upper stratosphere.

To demonstrate that the impact of the QBO on our results is negligible, we included the timeseries of tropical winds in our multiple linear regression analysis using monthly average tropical winds from the Freie Universitat Berlin (<http://www.geo.fu-berlin.de/en/met/ag/strat/produkte/qbo/>). Two separate approaches were tested: (1) fitting a single timeseries of the winds at 40 hPa and (2) simultaneously fitting two terms using winds at 30 and 50 hPa. As expected, neither approach significantly improved the fit, with the variance of the residual to the fit decreasing by less than 10%

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at all levels. Most importantly, the inclusion of the QBO term had a very small effect on the calculated offsets, with the change resulting from the addition of the QBO term being less than 0.1 K in all cases except one, where the change was 0.18 K. Since the impact of the QBO on the global mean temperature offsets is very small, and there is no obvious physical mechanism for such an effect, we see no need to include the QBO fitting term in the calculations presented in the paper.

A brief mention of this is made in the revised manuscript when we discuss the fitting procedure.

That point aside, somewhere in this paragraph it should also be mentioned that the fits are performed individually for each of the segments of the time series separated by the discontinuities (same as was done when removing the seasonal cycle).

But on page 25806 line 24 of the submitted manuscript we do state “the fits are computed over the three periods given above,” which we thought was clear. Nevertheless we have modified this sentence, and now say “the fits are computed separately over ...”

(25806, 18-24) “A simpler approach ...” This discussion of a potential alternative procedure for defining the offsets is out of place here. It should follow the description of the actual procedure adopted, say after (25807, 4).

Agreed. These three sentences now make up a separate paragraph just after the current one.

(25807, 7) “apply the offsets to the first and second time periods” It might be better

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to be more specific and write something like “apply the offsets in sequence, first to the 1985-1998 segment of the time series and then to the 1979-1985 segment, while treating the post-1998 segment as the (unaltered) reference. Note that referencing the adjustment procedure to the post-1998 period is an arbitrary choice...” [which I presume is the case]

We have modified this sentence accordingly and have noted that the choice of the reference period is arbitrary. However, we did not say the offsets were applied in “sequence”, because they are not. The offsets for the first two periods are applied at the same time.

By the way, it is never mentioned how these global-mean offsets are applied as a function of latitude and longitude. I presume the same offset is applied everywhere - no?

Yes. Since our model is spectral, the temperature offsets are applied to the global mean harmonic. We now note this when we first discuss the nudged simulation using the offsets in Section 3.1.

(25809, 6) “as expected, the largest component is the annual cycle” The “largest component” of what? The calculated offset?

Yes, the calculated offset. This has now been added.

(25810, 17) “affects photochemical constituents” More specifically, “temperature-dependent chemical constituents”. Presumably, constituents that are affected mainly by transport, or by photochemical processes that are not sensitive to temperature, will

not display the same extreme pathology as ozone. And, by the way, although this has not been discussed, there must be an underlying assumption that making a globally uniform temperature adjustment does not have a major effect on dynamics. Some discussion of this point is probably called for.

Agreed. However we have also changed “meteorology” to “temperature” earlier in this sentence since the adjustments do not affect the meteorology (see next paragraph), so we just change “photochemical constituents” to “chemical constituents.”

Regarding the reviewer’s second point about an underlying assumption: Since the temperature adjustment is applied only to the global mean, the impact on the horizontal wind fields is expected to be quite small. (Significant changes in the winds would only occur in the presence of spatial gradients in temperature through geostrophy). The adjusted temperatures should therefore be consistent with the reanalysis winds to a very good approximation. We have added a sentence in the 4th paragraph of the introduction explaining this.

(25811, 5) “results in a continuous ozone trend” Better to say “results in continuous ozone evolution”. There is little linear trend in the years near 1998.

We now say “results in a smooth transition of ozone across 1998.”

(25812, 10) “ozone starts to decline” OK. But what happens after about 2000, when ozone begins to increase? Are we seeing here the effect of curtailed emissions of

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ODS, per the Montreal Protocol?

Yes, this is now mentioned.

(25813,7) "this technical note describes" ==> "this technical note has described"

Yes.

ACPD

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