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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Response of the Antarctic stratosphere to warm pool El Niño Events in the GEOS CCM" *by* M. M. Hurwitz et al.

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Re: ACP–2011–170 ("Response of the Antarctic stratosphere to warm pool El Niño events in the GEOS CCM")

We kindly thank this reviewer for his/her helpful comments. Our responses directly follow each comment.

M. M. Hurwitz and co-authors

*** In general, this manuscript appears to be strongly connected to Hurwitz et al 2011 and this is clearly outlined by the authors, but in some parts the authors are too quick in justifying their motivation/description (for example see discussion of figure 4). I would suggest to let the manuscript become more autonomous (and not as if it were a sort of Hurwitz et al part II)

We agree with this reviewer that, while the present paper extends the work of Hurwitz et al. (2011), one should be able to be read the two papers independently. To this end, we compare model diagnostics with the MERRA reanalysis, instead of referring readers to Hurwitz et al. (2011). We feel that most of the diagnostics are clearly justified and defined. However, we have expanded the discussion of the streamfunction and Rossby wave source diagnostics (and the respective figures) in the revised manuscript.

*** Having to deal with a larger size than reanalysis data, why not extending this analysis to the CT El Nino too?

Many previous studies have examined the observed and modelled response to CTEN events (e.g., Randel et al., 2009). Thus, we have focused our attention on the response to WPEN events in the Southern Hemisphere. To our knowledge, the modelled stratospheric response to WPEN has not been shown previously in the literature.

Randel, W. J., Garcia, R. R., Calvo, N., and Marsh, D.: ENSO influence on zonal mean temperature and ozone in the tropical lower stratosphere, Geophys. Res. Lett., 36, L15822, doi:10.1029/2009GL039343, 2009.

*** Introduction: could you add some reference to the works that relate the strength of the southern polar vortex to the phase of the QBO (e.g., Butchart and Austin 1996; Baldwin and Dunkerton 1998)?

Thank you for this idea. We reference Baldwin and Dunkerton (1998) in the revised manuscript. We guess that the two WPEN/QBO-E events during the analysis period (1978-1996) strongly contributed to the sensitivity of the Antarctic vortex to the phase of the QBO in austral spring and summer, as found by these authors.

Baldwin, M. P, and Dunkerton, T. J.: Quasi-biennial modulation of the southern hemi-

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sphere stratospheric vortex, Geophys. Res. Lett., 25 (17), 3343-3346, 1998.

*** Table 1: year 2002 is classified as a easterly QBO year, but according to the QBO data (look at figure 1 on this webpage: http://www.geo.fuberlin.de/en/met/ag/strat/produkte/qbo/index.html) it seems to me a westerly year. Am I wrong?

2002 is not included in Table 1.

*** The choice of excluding the years 2002-2003 is not clearly motivated. The ENSO of 2002 was not affecting the SSTs of SON of year 2003, so why not including this year in generating the boundary conditions? Moreover, if the ENSO of 2002 had a role in the wave events from the troposphere leading to the major warming over Antarctica, this fact again cannot justify the choice of excluding year 2002 or 2003 in the boundary conditions. It would certainly be more consistent either to include year 2003 in the boundary conditions (if there was an WPEN ENSO as reported in Table 1), or eventually to exclude it in both the boundary conditions and the MERRA composites. (for example Table 2: the eddy heat flux in the WPEN/QBO-E composite is different. It would be more consistent to recalculate it by excluding the "special" year 2003 in MERRA? The same holds for the other fields: T, OLR : : :)

We did include the 2003 WPEN event in the MERRA composite. We chose to force the WPEN simulation with the two strongest WPEN events: 1991 and 1994.

Also, we were consistent in our exclusion of the 2002 SON season from our analysis. We did not include the 2002 WPEN event (coincident with the westerly phase of the QBO) in our MERRA analysis because eddy heat flux was unusually strong in September 2002 (i.e., Newman and Nash, 2005), likely biasing high latitude stratospheric conditions in late 2002.

*** I would not stratify ENSO neutral with QBOE and QBOW, I would consider the ENSO neutral without stratification

Though we find that the modelled SH response to ENSO neutral is insensitive to the phase of the QBO, consistent with Hurwitz et al. (2011), we compare ENSON/QBO-E to WPEN/QBO-E composites to so as to show the 'pure' WPEN – ENSON response.

*** Do composite years change if using the 30hPa level in MERRA too?

The composite responses were most consistent when we used the 30 hPa level in the model and 50 hPa in MERRA. The 30 hPa level in MERRA lags the 50 hPa level by several months.

*** Figure 2: could you show the time-series of the cut at 50 hPa (in both MERRA and the model) and the cut at 30 hPa?

Yes. We have added a panel showing the modelled timeseries at 30 hPa and 50hPa, and the MERRA timeseries at 50 hPa.

*** Table 2: could you describe it also in terms of anomalies of the eddy heat flux?

We prefer to show the composite mean values, for easy comparison with other models and datasets.

*** Could you add some specific comment about the role of the representation of the climatological stationary waves (in the model) in the stratospheric response to the convective heating at high latitudes? (see for example Fletcher and Kushner, J climate, 2010)

Is the reviewer referring to Fletcher and Kushner (2011)? We have examined the representation of the stationary wave patterns in October/November at SH high latitudes, in the ENSON and WPEN composites. Geopotential height anomalies are similar and in phase in the two GEOS CCM simulations. In MERRA, geopotential height anomalies in ENSON are in phase with the WPEN/QBO-E composite. The ENSON and WPEN/QBO-W composites are somewhat out of phase, perhaps damping the high latitude response to WPEN/QBO-W events and explaining the weaker planetary wave response (see black squares in Figure 3) during these events.

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Fletcher, C. G., and P. J. Kushner: The role of linear interference in the annular mode response to tropical SST forcing, J. Climate, 24, doi:10.1175/2010JCLI3735.1, 2011.

*** It is not clear why the 'Vera et al 2004' region (red box on figures 5 and 6) should be the same in the model as well as in the observational datasets: the mechanism identified by Vera et al (enhanced convection –> stronger poleward wavetrain in the SH in that region) is not verified in the model.

The stationary wave pattern in the model is well matched with observations (note that this is an important improvement from previous versions of the GEOS GCM/CCM). Thus, we would expect the modelled region of enhanced convection to be collocated with the observed region. The red boxes provide a visual guide for readers when comparing plots. We find that OLR is reduced (i.e., enhanced convection) in WPEN as compared with ENSON in the 'red box region' (revised Figures 6a and 6c). This enhanced convection is associated with a stronger poleward wavetrain in the SH (revised Figures 5a and 5c).

*** Figure 5b-5d shows that there are biases in the QBOe-QBOw (WPEN) OLR also at equatorial latitudes, implying possible biases in the convection. How do this could affect the representation of the QBO itself? (for example, if there is not enough wave flux by resolved waves due to biases in the reprint of convection there could be no downward propagation of the westerly winds from down to 100 hPa ?)

While these are interesting questions, they are beyond the scope of this paper. Revised Figures 6b and 6d do not provide enough evidence to judge whether or not the modelled convection is biased; however, they do provide evidence of the effects of the QBO phase on tropical convection during WPEN events. Revised Figure 6d shows that OLR differences are negligible throughout the tropics, suggesting that, during WPEN events, convective activity does not depend on the phase of the QBO.

*** Figure 6: the RWS: at which level is shown? Could you describe in Figure 6 also the RWS in other regions and not only in the red-box area (cyclonic, anticyclonic: : :)?

Could you spend few words for clarifying the choice of this diagnostic (have you looked at the lat-lon wave activity)?

The Rossby wave source diagnostic shown in revised Figure 7 is an average of the 250hPa and 300hPa levels. We use RWS diagnostic to confirm the findings of Vera et al. (2004): that the enhanced planetary wave activity during WPEN events directly results from the enhanced convection in the central South Pacific. We have focused our discussion on the red box region to show that the regions of enhanced convection during WPEN events (and QBO-E, during WPEN events) are collocated with enhanced Rossby wave activity in the upper troposphere.

*** Could you speculate about the behavior of the models QBO? Why does it not reach the 100 hPa level and stays confined above? What is the role of the resolved and unresolved equatorial waves? What is the role of the vertical resolution in the representation of the QBO? How many vertical levels are there in the 100-10 hPa region? Was the QBO internally generated in the previous model version? What is the role of the convection scheme in the model in the representation of the QBO?

The model's relatively weak QBO westerly phases are likely due to the weak resolved and eastward propagating equatorial wave modes (e.g., Kelvin waves). In fact, the initiation of the QBO westerly phases requires the dissipation of the wave modes with large amplitudes (much larger than what is believed to be the amplitude of gravity waves) in the equatorial upper troposphere and lower stratosphere (Campbell and Shepherd, 2005). Hence, resolved waves are thought to be more important in determining the QBO phase at 100 hPa than are unresolved gravity waves. However, further research is required.

As noted by this reviewer, the internally generated QBO is very sensitive to dynamics, model grid, cumulus parameterization and vertical resolution. However, it is extremely hard to generalize what is required to internally generate the modelled QBO because the internal QBO also depends severely on the model numerics and characteristics

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(i.e., factors that are not physically based).

Campbell, L. J., and Shepherd, T. G.: Constraints on wave drag parameterization schemes for simulating the quasi-biennial oscillation. Part I: Gravity wave forcing, J. Atmos. Sci., 62, 4178-4195, 2005.

*** Could you add a comment on the implications of not having a quasi-biennial signal in the convection in CCM simulations of present and future climate?

In a present-day climate, the persistent QBO-E response to WPEN events biases precipitation in the central South Pacific and reduces interannual variability in the Antarctic stratosphere. If the frequency of WPEN events were to increase in a future climate, the GEOS CCM would simulate too much warming in the Antarctic stratosphere, possibly leading to an unrealistically quick ozone recovery. We have added a condensed version of this response to Section 4 of the manuscript.

We consider the net impact of the modelled QBO on stratospheric climate and variability in further detail in a separate paper:

Hurwitz, M. M., Newman, P. A., and Song, I.-S.: Influence of an internally-generated QBO on modelled stratospheric dynamics and ozone, submitted to J. Geophys. Res., 2011.

*** A curiosity: how do the model behave in the NH stratosphere (CTEN ENSO/QBO relationship)?

The Arctic stratosphere warms in response to CTEN events in the GEOS CCM, as observed. We have not analysed the NH response to ENSO in detail.

*** page 9754, line 24: five days (?) -> this is not shown

We have removed our comment about the impact of WPEN events on the breakup of the Antarctic vortex. We will discuss the relationship between ENSO and the breakup date in more detail in a future paper.

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