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Earth System Dynamics
Discussions

Interactive comment on "Stratospheric ozone and QBO interaction with the tropical troposphere on intraseasonal and interanual time-scales: a wave interaction perspective" *by* Breno Raphaldini et al.

Breno Raphaldini et al.

brenorfs@gmail.com

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(1)I think the statistics of the method need to be much more carefully described. At the moment, we aren't really given any indication of how the significance is determined other than a reference to another article. One thing I am particularly concerned about is that by doing this frequency decomposition as well as using multiple variables, it means that effectively a very large number of tests has been performed. Is this accounted for when performing the significance tests. For example, if you test 100 different frequencies and use a 95% level, you'd expect 5 different frequencies to show a significant signal. Furthermore, how is autocorrelation in the time series for the low frequencies

accounted for in the significance testing. For the decadal timescales there will be very few degrees of freedom in the observational record and I would hope that this is being accounted for in the statistical testing but it's not clear. So, I strongly recommend an improved discussion of the statistical testing and the significance of results in light of these complicating factors.

R: We apologize that we did not describe the statistics with sufficient details. PDC is a function of the coefficients of vector autoregressive model. Given that the coeffcients are asymptotically jointly normally distributed, we can use the delta method (Serfling, 1980) to analytically calculate the asymptotic statistics for PDC. After a straighforward but tedious algebraic computation, we can show that PDC at frequency lambda is distributed asymptotically (under the null hypothesis of zero PDC) as the weighted sum of two chi-square with one degree of freedom (Takahashi et al., 2007). Therefore, we can use this asymptotic distribution to calculate the p-values. For details of the derivation, we refer to Takahashi et al. (2007). Significance levels for frequency domain quantities are controlled only point-wise as this is the standard everywhere. The reason for this is that the point estimates for neighboring frequencies are highly correlated. Therefore, standard correction like bonferroni or even FDR that assume independence or weak dependence give the wrong significance level. Every single article that we found where PDC, coherence or bi-coherence were used and the significance level is reported use the frequency-wise significance level (for representative examples see Huybers and Curry, 2006 and Came et al., 2007). For PDC it is easy to see that the use of frequency-wise significance level is reasonable given that the PDC values for different frequencies are the Fourier transform of the same coefficients of the autoregressive process. The fact that lower frequency have fewer samples are taken care by higher threshold values for PDC at lower frequencies. We added the following brief description of the statistics for PDC in the main text. "PDC is a function of the coefficients of vector autoregressive model. Given that the coefficients are asymptotically jointly normally distributed, we can use the delta method (Serfling, 1980) to obtain analytically the asymptotic statistics for PDC. After an algebraic computation we can show

that PDC at frequency lambda is distributed asymptotically (under the null hypothesis of zero PDC) as the weighted sum of two chi-square with one degree of freedom (Takahashi et al., 2007). Therefore, we can use the asymptotic distribution to calculate the p-value. For details of the derivation, we refer to Takahashi et al. (2007). The significance level used in the article for PDC is the frequency-wise value as it is the standard for frequency domain analysis given the high correlation between the point estimates for neighboring frequencies (see e.g. Huybers and Curry, 2006; Came et al., 2007)."

(2) I question whether showing the interaction between the gravity waves and the MJO is really an explanation. At pg 2, l3, it is stated that this connection represents a partial explanation, but it's not really a mechanistic understanding. It certainly hints at something that should be investigated, but I wouldn't even call it a partial explanation. One aspect I'm concerned about with this inference is whether the stratospheric zonal winds are accounted for when assessing the connection between the gravity waves and the MJO or not. It's not entirely clear to me. Is the connection between the gravity waves and the MJO just a simple assessment of the connection between the gravity waves and the MJO or is it an assessment of whether the gravity waves provide you more information beyond what you'd already get given the connection between the stratospheric zonal wind and the MJO. If it is not the latter, then isn't it possible that this connection between the gravity waves and the MJO simply represent the connection between the QBO and the MJO where the gravity wave variability is a signal of the QBO and not necessarily connected to the MJO in a causal sense.

R:The idea to investigate the effect of QBO related normal modes with MJO related normal modes was inspired by the works on nonlinear resonance as a driver for MJO through the interaction of tropics-extra tropics,see : Raupp, C. F., & Dias, P. L. S. (2010). Interaction of equatorial waves through resonance with the diurnal cycle of tropical heating. Tellus A: Dynamic Meteorology and Oceanography, 62(5), 706-718/ -Majda, A. J., & Biello, J. A. (2003). The nonlinear interaction of barotropic and equatorial baroclinic Rossby waves. Journal of the atmospheric sciences, 60(15), 1809-

1821.). The idea then is to search for evidence for mode interaction that may lead to stratosphere-troposphere interaction similar to the aforementioned theories for the interaction tropics-extratropics. In this sense our work may be regarded and a evidence for such a mechanism, although we do not develop the theory itself. Regarding the information of the interaction of gravity waves on MJO. The normal modes that contribute to the QBO are determined by a linear regression procedure, gravity waves being some of the main contributors. To say that gravity waves associated with the QBO also interact with the QBO gives more information on the MJO-QBO interaction since it restricts the type of mode responsible for the interaction, in this particular case gravity modes rather than balanced (Rossby) modes.

3) Conclusions are drawn about what factors influence the MJO on what frequencies. I wonder if, having performed this causality analysis, which I expect will seem like a bit of a black box to many readers, whether the results could then be related back to something a bit more physical e.g., could you present the time series and lagged correlations between the fields at the relevant frequencies to convince readers of the actual correlation between these time series.

R:In the present version of the manuscript we have included a composite analysis based on Reviwer #2 suggestion showing the differences on each normal mode component of the the MJO depending on the phase of the QBO.

4) I'm not entirely sure what is shown in Fig 12, but it looks kind of strange. It is described by "We recompose the zonal wind fields of WIG waves associated with the QBO". Is this showing where the amplitude of the gravity waves fluctuate along with the QBO? So it's really showing where orographically generated gravity waves are active? If so, it makes sense that there should be such a close correspondence between orography and this metric. But is it really the case that gravity waves over Greenland and Antarctica are varying with the QBO? Furthermore, I don't think it's really the orographic gravity waves that interact with the QBO, it's more the convectively generated gravity waves, which we don't really see in this figure. I think this all needs a bit more

explanation and a bit more discussion of the physical linkages to complement the Partial Directed Coherence analysis.

R: After discussion with the co-authors we decided to remove this section on the spatial structure of the gravity waves, since we came to the conclusion that it was not bringing insight into the main problem of the article. Instead we followed the Reviewer #2 suggestion to present composites of the MJO related normal modes for each MJO phase, comparing them as a function of the phase of the QBO (positive or negative).

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