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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Quasi-biennial Oscillation and annual variations in tropical ozone fromSHADOZ and HALOE" *by* J. C. Witte et al.

J. C. Witte et al.

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We wish to thank the referee for the helpful comments and suggestions. We have made all grammatical changes recommended by the referee. The following addresses each refereed comment.

Comment: Page 1, line 15: Is it true that the resolution of HALOE becomes coarse around the tropopause region? I thought that because HALOE was a solar occultation instrument that the vertical resolution was fixed and that retrievals in the lower stratosphere and/or upper troposphere become unreliable simply because the optical depth becomes too great.

Response: The referee is correct; HALOE measures vertical profiles from 15 km to 50 km with 2km resolution. From Table 1 of Bruhl et al. (1996), the estimated total error,

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obtained by calculating the root-mean-square of all the random and systematic error inputs is around 30% at 100 hPa (roughly coincides with 15 km which is the lower limit for our study). The data are considered to be within the error range of the correlative measurements used in their study.

Comment: Page 1, line 17: You say that 'Strong annual signals of alternating positive and negative ozone anomalies are observed' but isn't an alternating positive and negative anomaly the very definition of an annual signal? Surely this sentence says nothing more that 'Annual cycles in ozone and temperature are well correlated'?

Response: We agree.

Comment: Page 1, line 23: How does it happen that the ozone QBO precedes the QBO in the winds? I thought the QBO in the winds drives the QBO in ozone? In that case I would have expected that the QBO in the winds would precede the QBO in ozone.

Response: After the manuscript was submitted we discovered that the statistics was interpreted incorrectly. The QBO winds do precede the ozone anomalies. An additional cross-correlation test confirms the interpretation and time lag amount. The error has since been corrected.

Comment: Page 1, line 24: You may need to choose your wording more carefully here. It's not entirely clear to me how an annual cycle can have a horizontal length scale. I can guess, but I shouldn't need to.

Response: We changed 'horizontal' to 'meridional'.

Comment: Page 2, line 12: You say that 'The BDC ... is stronger during northern hemisphere winter' but compared to what? Do you mean it's stronger than in the northern hemisphere summer or stronger than in the southern hemisphere winter, or both? You may need to expand on that statement a bit. Response: We added and slightly modified the sentence to: "Significant topographic differences between the northern and southern hemisphere change the wave activity and hence the strength of the BDC.

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One can see these differences in the distribution of trace gases such as ozone [Fusco and Salby, 1999]. The BDC varies annually and is observed to be stronger during northern hemisphere winter".

Comment: Page 2, line 18: Is it true that radiative heating rates determine the strength of the residual circulation? I thought that the strength of the residual circulation was determined by mid-latitude planetary wave activity and the extra-tropical 'pump'i.e. as proposed by Holtan and Tan.

Response: True. We have revised the paragraph.

Comment: Page 2, line 20: Have the meridional length scales of the QBO and BDC only ever been previously calculated theoretically? The way this sentence is worded it sounds like this is the first study to provide an analysis of these length scales based on observations.

Response: The length scales of the BDC have only been calculated theoretically. As far as we know, this is the first attempt to calculate BDC length scales using in-situ data.

Comment: Page 3, line 3: Doesn't it go without saying that if you have nine years of data that you can capture nine annual cycles?

Response: We removed the redundancy.

Comment: Page 3, line 16: Is the instrument precision 5% throughout the profile? Papers such as: Smit, H. G. J. and Kley, D.: Juelich Ozone Sonde Intercomparison Experiment (JOSIE), 5 February; March 1996, WMO Report No. 130, 108 pp., 1996. and Komhyr, W. D., Oltmans, S. J., Chopra, A. N., and Franchois, P. R.: Performance characteristics of high-altitude ECC ozonsondes, in: Atmospheric Ozone, Proceedings of the Quadrennial Ozone Symposium, Greece, 1985. suggest ozonesonde precision varies with altitude.

Response: It is 5% of the total column compared with TOMS. This is in reference to

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Thompson et al. (2003), which we added to the sentence. The referee is correct that ozone precision does vary with altitude, among other things.

Comment: Page 3, line 22: It is not true that the ozone QBO signal is narrowly confined to the same equatorial region. It is quite easy to detect the influence of the QBO in mid-latitude ozone.

Response: We modified the sentence to read: "The ozone QBO signal is strongest and most evident within that equatorial band region".

Comment: Page 4, line 5: It seems to me that this is the wrong way around. I thought you would have subtracted calendar month means from the monthly averaged profiles so that when ozone is anomalously high your anomaly is positive.

Response: You are correct. We have fixed our sentence.

Comment: Page 5, line 11: Do you definitely mean 'monthly zonal mean wind shear' and not 'monthly zonal mean wind' i.e. did you take the first derivative in the vertical of the monthly mean zonal mean wind to derive the wind shear? I am a little confused by what is shown in Figure 1. The solid and dashed contours show the zonal mean zonal wind shear. So if moving vertically you are shifting from westerly winds to easterly winds this would be plotted as negative wind shear and vice versa. That's all well and good. But then you say 'Solid lines indicate positive (westerly) wind regime and dashed lines indicate negative (easterly) wind regime'. Now you can't use the contours to show both the wind shear and the wind. Surely solid lines indicate positive wind shear and not a westerly wind regime. Also the last line of the figure caption should say 'data are' and not 'data is'.

Response: Yes, we took the derivative of the zonal mean wind to calculate the wind shear. We added that description the "Data" section. Also, as requested, we updated the caption of Figure 1.

Comment: Figure 2: I am surprised that the change of resolution in the SHADOZ

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profiles from 0.25 to 2 km causes a uniform reduction in the profile. Why is that?

Response: We believe that the slight curvature in the ozone reference profile above 23 km plays a role in reducing the profile when we calculate a 2 km coarser vertical resolution.

Comment: Page 4, line 21: Is this an altitude registration issue for HALOE? How much would the derived altitude for the HALOE profiles need to be changed to bring HALOE into agreement with the ozonesondes? The caption for Figure 3 needs to say that these are ozone anomalies and not ozone mixing ratios.

Response: We adjusted the HALOE profile by bringing it down 0.25 hPa, 0.50 hPa and 0.75 hPa. This did not improve the overall agreement with the SHADOZ reference profile. The shift created an underestimate of HALOE with SHADOZ in the lower half of the profile and an overestimate above. We also changed the wording on Figure 3.

Comment: Page 5, line 6: You have to be very careful with your wording here. A positive lag indicates that the ozone precedes the wind shear, not the wind. The distinction is very important.

Response: We corrected this error, noted by the referee above.

Comment: Page 5, line 20: You say that 'The zonal averaging of the SHADOZ data apparently diminishes any ENSO signal...'. Can't you test this easily enough by not calculating zonal means and just look at some individual SHADOZ stations? You could then be more certain than 'apparently'.

Response: We did do a comparison with just the monthly Nairobi ozone anomalies and the zonal wind shear and could not find the 1998 ENSO anti-correlation that Logan et al. (2003) found. We prefer not to discount the possibility that this phenomenon was observed by Logan et al. (2003) in the datasets available to them at the time.

Comment: Page 6, line 13: I think that you need to cite a paper here that supports your statement that the annual variations in temperature and ozone are driven entirely by

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the BDC.

Response: The main reference supporting the statement is Randel et al. (2007) and was added.

Comment: Page 7, line 7: When you say 'the QBO easterly wind regime' do you mean 'the QBO easterly wind shear regime'.

Response: We have made the change.

Comment: Page 7, line 18: So that apparent null point at 30km in the power spectrum fields simply results from the fact that the QBO tends to lift or lower the ozone profile as a whole. A lowering of the ozone profile would cause ozone increases below the ozone maximum and ozone decreases above the ozone maximum. That's why you get signals there. At the ozone maximum however, raising or lowering the whole profile by a km or two has almost no effect. You should see opposite phase in your QBO power spectrum at say 33km compared to 26km. Does the ozone profile vertical gradient maximize at 26km?

Response: We calculated the vertical gradient of the reference ozone from Figure 2 and found that gradient profile does indeed peak at 26 km with an amplitude value of 1.15 ppmv/km. We included this calculation in the text.

Comment: Page 7, line 21: How can it be an extension of the QBO signal when they are of totally different periods?

Response: The referee is correct. We removed the erroneous sentence.

Comment: Page 8, line 1: Just to confirm, the length scale here refers to the latitude range over which the QBO and/or BDC is active, right?

Response: Exactly, yes.

Comment: Page 8, line 29: I thought the change in ozone preceded the passage of the shear zone?

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Response: We corrected this error, noted by the referee above.

Comment: Page 9, line 9: When you say 'about the equator', I assume that you mean 'either side of the equator'. I think that you need to use more precise wording here.

Reponse: We changed the wording as requested.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 6355, 2008.