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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative effects of ozone waves on the Northern Hemisphere polar vortex and its modulation by the QBO" *by* Vered Silverman et al.

Anonymous Referee #1

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Atmospheric and Chemistry and Physics Discussion manuscript review of: "Radiative effects of ozone waves on the Northern Hemisphere polar vortex and its modulation by the QBO" By: V. Silverman et al.

Let me begin by saying that I very much like the paper. The approach using wave packets and taking into the account the implications of the seasonal cycle are novel and lead to insightful results. To be honest, I wish that I had more critical and helpful things to say, but for the most part, the conclusions are physically based and sound. Generally speaking, the paper is well-written, but there are some grammatical issues that need fixing (I don't think I commented on all of the grammar/spelling issues, so

please go over the paper carefully and correct any additional misspellings and errors that I missed). If the authors can take into account my relatively short list of minor suggestions below, then I will gladly recommend this paper for publication.

Major comments:

Comment #1 – Page 3 lines 22-23: I'm not sure about the seasonality statement here. You should double check, but if I recall correctly, Watson and Gray (JAS 2014) find that the QBO signal is stronger later in the winter. This may be an important point in light of the fact that your argument hinges on the seasonal cycle of the waves and the mean. If I am correct here, it would be good for you to comment on how Watson and Gray's results apply to your study.

Comment #2 – Page 4 lines 15-20: How does your approach deal with ozone flux convergences in the ZMO3 runs? While I understand that you only pass zonally symmetrized ozone to the radiation code, the zonal mean ozone does still include one effect of ozone waves on the simulations if the zonal mean ozone field includes the flux convergences. You should clarify this one way or the other and make it clear to readers exactly what pieces of wave ozone physics are included in each type of simulation (i.e. 3DO3 versus ZMO3).

Comment #3 – Page 30 line 30: You mention later that your results are robust to the 70th percentile choice, but I am wondering about the 100 hPa level. I say this because the 100 hPa level is a very sensitive region in the stratosphere as far as the "valving" of wave energy either upwards into the core of the vortex where the PV gradient is strong and there is a strong waveguide versus ducting the energy equatorward. I am guessing that your results are robust to this choice, but it would be good for readers to know this information. I say this mostly because I think your approach is novel and it would be good for readers to be able to have all of the information they need to apply the method in other contexts.

Comment #4 – Page 5 lines 14: Sorry to be picky, but I really think that you should include the original source here when discussing the inverse relationship between ozone and temperature, which is Craig and Ohring 1958, see citation below:

http://journals.ametsoc.org/doi/abs/ 10.1175/1520-0469%281958%29015%3C0059%3ATTD

Also, while the Hartmann 1981 paper is nice in a qualitative sense, much more detailed information can be gathered from the following sets of papers that I think you should also cite: Nathan and Cordero JGR 2007, Hartmann and Garcia JAS 1979, and Garcia and Hartmann JAS 1980. I think in particular the Garcia references are important because they are directly relevant to the physical interpretations of your work and have a good amount of physical insight in them that readers should know about.

Comment #5 – Page 5 lines 10-30:Two related issues here. One, there is some seasonality to the ratio of advective to photochemical timescales and the ratio of advective to Newtonian cooling timescales (see Fig. 3 of Nathan and Cordero JGR 2007). Also, there is strong seasonality in regards to many wave properties as outlined carefully in Nathan and Li (JAS 1991) and Nathan and Cordero (JGR 2007). Do your results agree with these theoretical results? While this may not be a simple set of questions to answer, I think that lending some effort towards deciphering if your WACCM results agree with previous theory would be nice. I will leave it up to you on where you want to comment on this (perhaps the results section is not the right place), but it would be helpful if you could comment somewhere in your text.

Comment #6 – Page 8 lines 25-30: Why are you using the beta-plane geometry form instead of the spherical form? I am wondering if your figure would look any different using the full form. I am also wondering a bit about your interpretation of the refractive index (RI) anomalies. In particular, while I do find your point regarding the ducting of wave energy in the middle portion of the domain (i.e. the blue region spanning 15-45 km in height and 70-80 N to 20 N) during west QBO, I am wondering about your interpretation during east QBO. That is, while there is a region of positive RI in the uppermost stratosphere during east QBO, before the wave energy gets there, it would

first encounter the broad region of negative RI anomaly (i.e. the same blue region I just described above). And given that there appears to be a region of positive RI immediately underneath the blue region (i.e. the red region extending from 60 N to 30 N between 10-30 km in height), isn't it possible that a bunch of wave energy is also being ducted equatorward during east QBO (but lower than is being ducted during QBO west)? Indeed it is somewhat hard to tell from Fig. 8c, but it seems like there is additional EP-flux convergence near 30-40 N at 30 km for QBO east. I'm not saying that there is any inconsistency in your argument, but perhaps east QBO is characterized by both increased upper stratospheric convergence and subtropical convergence at 30 km. Just a thought. Would the spherical form of the RI make determining this clearer? What about the individual wavenumber diagnostics (see below)?

Also, just out of curiosity, why are you not diagnosing the individual wavenumbers as per Eqs. (12) and (13) in Harnik and Lindzen (2001)? I'm certainly okay with using the more traditional 'Matsuno-like' RI and so I am not demanding that you use the individual wavenumber method, rather I am actually just curious for the rationale.

Comment #7 – Page 9 lines 19-20: Why exactly is it expected that the nonlinear terms are larger during QBO east? I realize that the QBO east is characterized by more wave driving, but couldn't that appear via the quasi-nonlinear PV flux term (1st term on the RHS of eq. 1) and not via the fully nonlinear terms? I realize that you cite the White et al. (2016) paper in the next sentence, but that just means that your results are consistent. Stating that something is "as expected" seems to imply that there is a physical reason to expect this result.

Comment #8 – Page 9 lines 25-28: If I understand your line of reasoning here, you are stating the ZMO3 run has stronger damping in the lower stratosphere and weaker damping in the upper stratosphere. Or said another way, 3d ozone decreases ozone damping in the lower stratosphere but increases damping in the upper stratosphere. You mention in Section 3.1 some of the ozone physics involved, but then you don't mention any of that here. I would say that something interesting can be said regarding what is happening. My initial take would be the following (though for sure the authors should give their own interpretation of the results because I may be missing something).

(Note that the discussion below also has implications for your results on page 10 lines 29-35 through page 11 lines 1-9).

Based on photochemical and dynamical timescales, the 3d ozone induced decrease in damping in the lower stratosphere must be associated with advection of zonal mean ozone by the wave fields, yes? And in the upper stratosphere, the 3d ozone induced increase in damping is due to photochemistry, yes? Now, the upper stratospheric increase in damping is to be expected based on the ozone-temperature phase relationship dictated by the temperature dependent Chapman chemistry (e.g., Craig and Ohring 1958).

However, the lower stratospheric dynamically-based ozone result is fundamentally dependent on the vertical and horizontal ozone gradients. Previous studies have discussed this bit of physics but only in the context of 1D mechanistic models (e.g., Nathan and Cordero 2007 and Albers and Nathan 2012). However, your results are the first to be able to state something more general and thus it may be worth pointing out that it appears that 3d ozone causes dynamically induced ozone heating anomalies that decrease wave damping. This would mean that if there is any seasonal cycle to the vertical and meridional ozone gradients, then there should be some seasonality to the effect of 3d ozone that is perhaps contributing to the enhancement of the HT effect that you describe in your conclusions. Or perhaps the vertical and meridional ozone gradients are different for the wQBO versus eQBO, which in turn leads to some of the differences you see in the EP-flux divergence for the two QBO phases? To be honest, I don't have this all worked out in my head clearly, but it is perhaps worth thinking about because it would seem you might be able to add some physical insight here in the context of a CCM whereas previous studies with physics discussions where limited because of their model simplicity. I should also mention that you can quite easily see

how all of the ozone physics modulate the EP-flux divergence by considering Eq. (14) in combination with Eq. (15) (for the lower stratosphere) and Eq. (17) (for the upper stratosphere) in Nathan and Cordero (2007).

Comment #9 – Page 9 Equation (1): Please define your notation here and don't just cite Smith (1983). Specifically, do the different primes mean something different? That is, do the primes in the PV flux term (1st term on the RHS) somehow denote something different from than the primes in the nonlinear terms (2nd and 3rd terms on the RHS)?

Minor comments:

Comment #1 – Introduction lines 1-2: "...exist since the early..." should be "...have existed since the early..."

Comment #2 – Page 2 line 1: Multi decadal should be hyphenated as multi-decadal.

Comment #3 – Page 2 line 4: "...(Taylor et al. 2012), does not..." should be "...... (Taylor et al. 2012), do not..."

Comment #4 – Page 2 line 2: While I could be wrong, I believe that you meant to use the word "assess" and not the word "asses" :)

Comment #5 – Page 3 line 3: Using a hyphen here doesn't work grammatically. Please rework this sentence.

Comment #6 – Page 3 line 17: I would suggest also citing the new (ish) paper by Watson and Gray (JAS January 2014) because it provides new insights supporting the original HT-1980 paper, which Garfinkel et al. 2012 (which you cite) call into question.

Comment #7 – Page 3 line 21: "noninear" should be "nonlinear".

Comment #8 – Page 4 line 24: "tendenfcy" should be "tendency"

Comment #9 – Page 3 lines 24-25: Which figures are you referring to? This is a bit vague.

Comment #10 – Page 5 line 15: Capitalize “northern hemisphere” (both words)

Comment #11 – Page 5 line 21: Similar to my Major Comment #4, while the Douglass reference is nice, I really think that the Hartmann/Garcia 1979 and Garcia/Hartmann 1980 references are very relevant here and they pre-date the Douglass reference by half a decade. They should also be included.

Comment #12 – Page 9 lines 3-4: You seem to be stating the same thing twice here (regarding non-acceleration conditions).

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