Weather Clim. Dynam. Discuss.,
https://doi.org/10.5194/wcd-2020-23-RC1, 2020

Weather and Climate Dynamics Discussions

Open Access

EGU

Interactive comment on "Tropopause-level planetary wave source and its role in two-way troposphere–stratosphere coupling" *by* Lina Boljka and Thomas Birner

Anonymous Referee #1

Received and published: 21 June 2020

Review of " Tropopause-level planetary wave source and its role in two-way" by Boljka and Birner

The authors analyze the possible forcing mechanisms for a planetary wave source near the tropopause that subsequently propagates upwards. The authors identify two different mechanisms: nonlinear wave-wave interactions and subsequent resonance, and also transient wave decay. They find a more robust downward impact for SSW preceded by a tropopause wave source event. This paper has many interesting results, and while there are some points the authors need to clarify, it is very likely that the paper will be suitable for publication after revisions which are best classified as somewhere between major and minor.

Major comments: 1. The authors argue that the upscale cascade is then followed by resonance, but little evidence is provided for resonance actually occurring, nor is resonance in the present context defined (the relevant section in Vallis doesn't help). There is a brief statement that e.g. the magnitude of EP flux divergence of wave 2 exceeds the convergence for synoptic wavenumbers (line 284-285), however this does not necessarily mean resonance is occurring. While a sudden change of EPFD would imply a change in wave activity if non-conservative processes are not present (equation 7.23a of Vallis 2006), this relationship is derived under specific limitations. However more generally (finite amplitude and non-QG), EPFD is not related directly to wave activity and thus not there is no expectation that it should be conserved. That is, a negative EPF in a given wavenumber range does not need to be balanced by positive EPFD in a different wavenumber range in a turbulent cascade with dry dynamics only. Furthermore, even if EPF was directly related to conserved wave activity, diabatic and other such non-conservative processes can create wave activity, and hence it is impossible given the diagnostics shown to determine whether the increase in planetary waves is indeed due exclusively to triad interactions as the authors suggest, or other processes. Overall there is no need for EPFD to be conserved in general, let alone locally.

Second the terminology is a bit confusing, as the authors refer to resonance here not in the context used most often in stratospheric dynamics (the way e.g. Plumb 2010 described it, or the way Matthewman and Esler 2011 have in mind). This distinction could be made somewhat clearer.

Finally, there is an entire literature in turbulence community of physics on a concept called wave turbulence. I am certainly not an expert on this topic, but I have had interactions with people from the turbulence community, and there is an entire book on the wave turbulence regime where it is meaningful to focus on specific triad interactions but to ignore the entire spectrum of possible interactions (Zakharov et al 2012). In short, the regime of any turbulent system can be characterized by the Reynolds number

Re. Re is defined as the ratio of the dissipation time Lˆ2/\nu due to viscosity and the inertial time L/V. The inertial time characterizes the generation by triad interactions of other Fourier harmonics out of velocity fluctuations with characteristic scale L and characteristic velocity V, often referred as “eddies", that are injected by the force. If Re«1 then viscosity dissipates the eddy before non-linear interactions can produce other eddies and the flow is effectively linear. In contrast, if Re»1 then the initial eddy injected by the forcing generates eddies of comparable, yet different, scale before any dissipation occurs. These eddies in turn generate by non-linear interaction other eddies with scale which is already comparable with theirs.

For the present study, Re can be defined as Re=V\tau/L where \tau is the characteristic time of decay of fluctuations due to frictional processes, V is some metric of velocity, and L is a metric of time. If Re«1 then the friction dissipates fluctuations created by the forcing before they can transfer appreciable energy to other modes by triad interactions. In contrast, if Re»1 the energy cascade would occur. Wave turbulence of the kind the authors have in mind (where a specific triad interaction can be studied ignoring all other possibilities) is only relevant when Re is less than “around” 1 (with the definition “around” very specific to the problem at hand). Regardless of the relevant value of Re an upscale cascade occurs, but a focus on the EPFD budget for individual triad interactions seems misplaced since there is no reason for EPFD to be conserved.

In short, unless the authors are able to bolster their results, I recommend deleting the claim that resonance is occurring. (I think this paper would still be a useful contribution without this claim.) At most, the possibility of resonance could be broached in the discussion section.

2. This is more a pet peeve than a major comment, but the authors cite several papers in the introduction claiming that “anomalous wave forcing is not always necessary for producing SSD events”. However these previous papers define wave forcing events by 2 std deviations. A 1.95 std deviation wave forcing from the long-term mean is clearly “anomalous” to me, but would be classified by some of these papers as not particularly

noteworthy.

In this paper the authors use a lower threshold, and claim that there are still many SSD events without an anomalous wave forcing (line 443). However Figure 6d clearly shows an anomalous wave forcing both at the surface and also at the tropopause. Presumably the 0.75 std dev threshold used here just barely misses some events that end up being included in Figure 6d. While these events certainly don't have "extreme" wave flux, wave fluxes are anomalous, and the use of "anomalous" on line 443 is incorrect.

Here is a list of lines where "anomalous" should be replaced with "extreme": line 42, 443, 541 "extreme" could also be added to line 36 .

3. I found figure 8 and its accompanying discussion a bit under-developed. First of all, it is strange that the authors find no significant tropospheric impact when all SSW are composited (line 504-505). This seems contrary to dozens of published studies finding a downward surface impact from SSWs. Second, the tropospheric impact is much clearer when focusing on the Atlantic sector (i.e. the NAO) and not the zonal mean (dozens of studies on this too), and the analysis here would be much more convincing if in addition to (or instead of) the zonal mean figures the authors only composited winds in the Atlantic sector. In other words, instead of showing zonal mean zonal winds, please only show zonal winds averaged between, say, 300E and 20E. A map view figure might also help in interpreting the results, especially in discriminating among the three options listed on lines 478 to 481.

minor comments: line 99:The Held et al 2002 review paper and the recent study Garfinkel et al 2020 should be added here. More generally, it isn't clear to me that these forcings should lead to a lower tropospheric wave source per se, and not a wave source higher in the troposphere. For example, upper tropospheric diabatic heating due to baroclinic instability or land-sea contrast should directly affect upper tropospheric stationary waves. That being said, I agree that these factors are likely not directly forcing waves at the tropopause.

Line 113-114 I found this sentence difficult to parse. Please rephrase.

line 231: It is impossible to tell from this figure that the events are long-lived due to the 10 day smoothing filter applied. If it is important to emphasize the long-lived nature of the EP flux divergence, then please perhaps add a thin line for the (raw) non-filtered data for the model and quasi-reanalysis.

Figure 3a: please indicate the pressure level for [U] either in the caption or on the figure itself

Is time smoothing applied for figure 4 and 5 and subsequent figures, or is figure 3 the only figure with time-smoothing? Please clarify either way. The reason I ask is that the text near line 258/259 seems to imply a time separation in the “synoptic” vs. planetary EPFD, however no such time separation appears in figure 4 and 5 (rather the planetary and “synoptic” waves change essentially simultaneously)

Colorbar tick labels on figure 4,5, 6, etc.– please label the ticks symmetrically about zero.

Figure 4 and 6: I found the equatorward/poleward tilt of EPF arrows to be somewhat confusing. I first thought this reflected some sort of propagation backwards in time, which of course makes no sense, but then I reread and understood. I don’t think the question of equatorward vs. poleward propagation is particularly important to this study.

Line 386- there is a positive stratospheric wind anomaly before the SSD in figure 6b, it is just weaker than in 6a,6e or 6e.

Lime 388 I don’t understand this comment about a lack of preconditioning in figure 3a. Winds are clearly stronger than average before the SSD on figure 3a

Line 392- is this difference in [U] before the SSD between the “tropopause source” composite and the “surface source” composite actually statistically significant? I.e. a difference plot between panels b and e.

Technical comments Line 121 in *the* lower

Plumb, R. Alan. "Planetary waves and the extratropical winter stratosphere." The Stratosphere, Dynamics, Transport and Chemistry, Geophys. Monogr 190 (2010): 23-41.

Matthewman, N.J. and Esler, J.G., 2011. Stratospheric sudden warmings as self-tuning resonances. Part I: Vortex splitting events. Journal of the atmospheric sciences, 68(11), pp.2481-2504.

Held, I.M., Ting, M. and Wang, H., 2002. Northern winter stationary waves: Theory and modeling. Journal of climate, 15(16), pp.2125-2144.

Garfinkel, C.I., White, I., Gerber, E.P., Jucker, M. and Erez, M., 2020. The building blocks of Northern Hemisphere wintertime stationary waves. Journal of Climate, 33(13), pp.5611-5633.

Zakharov, V.E., L'vov, V.S. and Falkovich, G., 2012. Kolmogorov spectra of turbulence I: Wave turbulence. Springer Science & Business Media.

Interactive comment on Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2020-23, 2020.