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

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Response to Reviewer 1

We would like to thank the reviewer for carefully reading the manuscript, and for their detailed and constructive comments that will ultimately help improving the original manuscript. Below are our responses to the reviewer, which will be implemented in the revised manuscript in the next stage. Note that we have not provided exact manuscript corrections at this point, but we have provided the intended changes in detail; all figures that were produced in response to the reviewer's comments are at the end of this document; the line numbers/figure references in the reviewer's comments refer to

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the original manuscript. The reviewer's comments are in italics; our responses are in normal text.

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

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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* L^2/ν *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* $Re \ll 1$ *then viscosity dissipates the eddy before non-linear interactions can produce other eddies and the flow is effectively linear. In contrast, if* $Re \gg 1$ *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* τ *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* $Re \ll 1$ *then the friction dissipates fluctuations created by the forcing before they can transfer appreciable energy to other modes by triad interactions. In contrast, if* $Re \gg 1$ *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.

We agree with the reviewer about the limitations of the QG EPFD framework, which is based on quasi-linear theory. However, despite these limitations the QG EPFD framework has been successfully used in the past for studying stratospheric and tropospheric dynamics. For these reasons we use it here, although more so in a qualitative than quantitative sense. We will include remarks on the limitations of the quasi-linear framework at the beginning of section 2. Note that in the model diabatic effects at the tropopause are not present (there is dissipation, but no diabatic source), but in the real atmosphere (though still unlikely) they can be present. This has been mentioned briefly in the methods section, but we will clarify this further in the text. As the reviewer suggests, we will discuss other options for amplified EPFD during upscale cascade, i.e. resonance, non-conservative and diabatic effects, which have also been added to the upscale cascade schematic - Fig. 1b below (revised Fig. 2 from the original manuscript). Therefore, as per reviewer's suggestion we will discuss resonance more carefully throughout the manuscript.

We thank the reviewer for providing detailed comments on turbulence theory and triad interactions. Note that in the present context we think of the upscale cascade mechanism as primarily representing wave-wave interactions of a finite number of large scale waves, which can be highly nonlinear but do not necessarily represent turbulent interactions across a quasi-continuous range of wave numbers. We will clarify this distinction in the text. We will also clarify the difference between the self-tuning resonance that occurs in the stratosphere and the one described here, when introducing potential resonance that follows upscale cascade (in section 2).

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.

We will replace "anomalous" with "extreme" where relevant as suggested by the reviewer.

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.*

We thank the reviewer for mentioning this issue - this allowed us to recognise an error in the code - it only applies to Fig. 8, which will be replaced by Figs. 2,3 below. The discussion around the Fig. 8 (especially in section 5.2.3; but also in conclusions and in the abstract) will be revised accordingly. We have also looked into local responses by plotting maps, which further revealed interesting points (see Figs. 4,5 below). However, since we now see a zonal mean downward impact and as we have map plots, which show weak impact in the Atlantic for surface wave source, we will not use the N. Atlantic zonal mean. The zonal mean picture has been well-established in the literature and as such provides easier comparison to previous work. Maps further explain some of its results. Here are the main points that we will now discuss in section 5.2.3, as shown in Figs. 2-5 below.

(i) Zonal mean figures (Figs. 2,3 below) reveal that the model (Fig. 2) and ERA-20C (Fig. 3) largely agree on the sign of the anomalies following SSWs preceded by the tropopause (Fig. 3e,f, 2g,h,i) and surface (Fig. 3g,h, 2j,k,l) wave source events. Therefore, despite the more robust downward impact for SSWs preceded by surface wave source events in ERA-20C, and a more robust response for SSWs preceded by tropopause wave source events in the model, there is a general agreement between the two datasets on the tropospheric zonal mean zonal wind response to SSWs preceded by different wave source events. The response to SSWs in both datasets reveals opposite signed anomalies of the zonal mean zonal wind following SSWs preceded by the tropopause and surface wave source events, with the latter having a negative anomaly further poleward (60-90N) than the former (40-60N). This is consistent with the original manuscript, where we already suggested that this could be a result of different impacts of planetary and synoptic waves on driving the tropospheric mean flow (and in the tropopause wave source case no extreme planetary wave source is found in the troposphere prior to the SSW event, thus a different response makes sense). Also, the robust 'response' of the tropospheric winds to the surface wave source could be partly a result of the wave-mean flow dynamics in the troposphere, regardless of the SSW events (see text about map results below). This would mean that when there is a surface wave source present, the tropospheric dynamics behave in a similar way, whether there is an SSW or not (but only in the Pacific; SSWs seem to obscure the behaviour in the Atlantic - see below). We can now also recover downward impact in zonal mean zonal winds following all SSWs in both datasets as well (e.g. Fig. 3b, 2a,c).

(ii) Figs. 4,5 show map-composites over all SSW events (top row), SSWs without wave source event (second row), SSWs preceded by tropopause wave source (third row), SSWs preceded by surface wave source (fourth row), all tropopause wave source events regardless of SSWs (fifth row), and all surface wave source events regardless of SSWs (bottom row). The composites are shown for zonal wind at 950 hPa (U950) and geopotential height at 500 hPa (Z500), respectively, for 30 days prior to SSW or wave source event and for 30 days following the event in ERA20C. While the case for all SSWs picks up the strongest signal from all the cases studied here, it now clearly shows a strengthening of the Pacific jet stream at negative lags, and equatorward shift of the Atlantic jet at positive lags (Fig. 4), consistent with reviewer's remarks. Consistent with this, the Z500 (Fig. 5) shows stronger Aleutian low as well as Scandianvian blocking-like signal over Europe at negative lags, and negative North Atlantic Oscillation (NAO-)-like signal at positive lags. However, we find that it is the SSWs preceded by the tropopause wave source that give a robust response in the Atlantic following SSWs (third rows in Figs. 4,5), i.e. NAO- signal at positive lags in both U950 and Z500, where the latter also revealed Greenland-blocking-like signal (not shown). Also, at negative lags there is clear Scandinavian-blocking-like signal, which has been shown in previous studies to have preceded SSWs and NAO- events (e.g. Kautz et al. 2020). This case also shows positive U950 anomalies in the subtropical jet region, suggesting shifts in Hadley cell following SSWs as well. On the other hand, the SSWs preceded by surface wave source show no clear signal in the Atlantic, but instead show a very robust signal in the Pacific, consistent with strengthening of the jet stream there both prior and after the SSW event, which is likely a consequence of the wave-mean flow interaction within the troposphere there (fourth and bottom row in Fig. 4 have similar signals in the Pacific). This is likely the case for the tropopause wave source in the Pacific as well (third and fifth rows in Fig. 4). In the Atlantic the surface wave source suggests an NAO- signal (bottom right panel in Figs. 4, 5), which is likely disturbed by the SSWs (fourth row right panel in Figs. 4,5). Here note that at positive lags the SSWs preceded by tropopause and surface wave sources have opposite signs in the Pacific (similar for all wave source events), which could be responsible for the opposing downward impact in a zonal mean as well. The results for SSWs that are not preceded by any wave source events are not clear. Note that while the results for the local surface response are interesting, we will keep their discussion to a minimum (e.g. in a supplement) as studying local impacts is beyond the scope of this study.

Also note that the downward impact in ERA-20C (average over all SSW events) may be different (slightly less robust) to downward impact in satellite-era reanalyses for two reasons: (i) ERA-20C is constrained by surface observations only; and (ii) ERA-20C has 50+ years of data more than satellite-era reanalyses (more events that can obscure the signal found in satellite-era). For further discussions on different reanalyses see, e.g., Gerber and Martineau (2018), Hitchcock (2019). We will mention these points in the text.

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 insta-

bility 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.

We will add the references as suggested. Generally, baroclinic instability and land-sea contrasts play a role closer to the surface; we will clarify that these mechanisms are not excluded further up in the troposphere, though their direct impacts are unlikely at the tropopause.

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

We will rephrase this as: "Therefore, even if the EP flux divergence exceeds a set threshold and appears as though there is a wave source, this is merely representing a decay of a wave, and thus we will refer to it as an apparent wave source."

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.

Fig. 6 below shows the composite over 10-day smoothed data (thick lines) and unsmoothed data (thin lines) as the reviewer suggested. The figure shows that unsmoothed EP flux divergence shows less persistence but there is still a peak of $\sim 2\sigma$ and $\sim 8+$ days persistence (i.e., EP flux divergence exceeding 0.75σ for over 8 days), as well as anomalously positive EP flux divergence persistence for an even longer period. This means that while the original Fig. (i.e. from smoothed data) does not necessarily show the persistence, persistent events still exist. We will rephrase the sentence "This shows that in both datasets there are long-lived (exceeding the 0.75-σ threshold for over 10 days) wave source events at the tropopause and in the lower troposphere (at surface), and that they have a similar evolution and standardised strength (peaking at 2σ) when they occur." as (or along the lines of) "The figure shows wave source

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events at the tropopause and at the surface, which are similar in strength (peaking at 2σ) and persistence. These events appear long-lived (exceeding the 0.75-σ threshold for over 10 days) due to a 10-day smoothing applied before compositing over all events. Thus, care must be taken in interpreting this persistence, even though there are individual long lived events in the dataset. Note that it is the 10-day mean wave forcing exceeding the threshold (e.g., 0.75σ) that matters for the SSDs."

Note that we will keep the figure as it is in the manuscript as the above comment addresses the issue raised by the reviewer.

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

We will indicate in caption that [u] is computed at 10 hPa and averaged between 45 and 75N.

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)

The time smoothing is applied before plotting everywhere (no smoothing yields similar but slightly noiser results though). We will clarify this in the methods section (within paragraph on l. 244-247). We agree with the reviewer about the l. 258/9 - we will clarify that the events can occur simultaneously or synoptic waves slightly precede the planetary waves (case by case study reveals both options - not shown).

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

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We have now made ticks symmetric about zero (figures not shown here; see Figs. 7-8 for an example).

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.

While the meridional wave propagation is not emphasised in the manuscript, it generally still matters as the differences in wave propagation may lead to different responses of the atmosphere. We will clarify in the text and in the figure captions that arrows denote "*meridional* wave propagation, not propagation in time".

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.

The reviewer is correct - we will change the text accordingly.

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

The reviewer is correct in that the winds are positive prior to SSDs in both datasets. However, in the model the wind anomaly remains below 0.5σ before an SSD, whereas in ERA-20C it is much stronger and exhibits a slight increase before an SSD. We will clarify these preconditioning differences in the text.

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.

We have plotted the difference between panels b and e (surface wave source cases minus tropopause wave source cases) - see Figs. 7,8 below. The difference is significant in ERA-20C (Fig. 8), but less so in the model (Fig. 7). We will discuss it this way and (as mentioned above) more carefully discuss the preconditioning differences where relevant. We will not include these figures in the manuscript.

Technical comments

*Line 121 in *the* lower*

We will add "the".

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List of Figures with full captions

Note that the template had a limit to the length of the caption, thus we provide full figure descriptions here, and figures are provided below with incomplete captions.

Fig. 1: Revised upscale cascade schematic from Fig. 2 of original manuscript.

Fig. 2: Composite analysis of downward impact in zonal mean zonal wind (latitude-

pressure vertical cross section) averaged between the lags 15 and 25 days (top row), 30 and 40 days (middle row), and 40 to 50 (bottom row) following (a,b,c) all SSW events, (d,e,f) SSWs not preceded by any wave source events, (g,h,i) SSWs preceded by tropopause wave source, and (j,k,l) SSWs preceded by surface wave source. The figure shows standardised zonal mean zonal wind anomalies (shading) and zonal mean zonal wind climatology (contours; contour interval is 5 m s^{-1} with 0^{th} contour omitted for clarity, i.e. ...,-10, -5, 5, 10,...). Grey shading masks out data that are not significant at 95% level. Numbers in brackets denote number of events in each composite. Data are from the model.

Fig. 3: As in Fig. 2, but for ERA-20C data. Here composites are shown for zonal mean zonal wind (latitude-pressure vertical cross section) averaged between the lags 20 and 30 days (top row), 35 and 45 days (bottom row) following (a,b) all SSW events, (c,d) SSWs not preceded by any wave source events, (e,f) SSWs preceded by tropopause wave source, and (g,h) SSWs preceded by surface wave source.

Fig. 4: Map-composite analysis of downward impact in zonal wind at 950 hPa (U950) averaged between the lags -30 and 0 days (left column), and 0 and +30 days (right column) around SSW events: (top row) all SSWs, (second row) SSW events not preceded by a wave source, (third row) SSWs preceded by tropopause wave source, (fourth row) SSWs preceded by surface wave source. The bottom two rows show the same but for all wave source events around the central wave source date: (fifth row) all tropopause wave source events, (bottom row) all surface wave source events. The figure shows standardised U950 anomalies (shading) and U950 climatology (contours; contour interval is 4 m s^{-1} with 0^{th} contour omitted for clarity, i.e. ...,-8, -4, 4, 8,...). Stippling denotes values that are significant at 95% level. Numbers in brackets denote number of events in each composite. Data are from the ERA-20C.

Fig. 5: As in Fig. 4, but for geopotential height at 500 hPa (Z500). Here the

figure shows standardised Z500 anomalies (shading) and Z500 climatology (contours; contour interval is 200 m, i.e. ...,4800, 5000, 5200,...).

Fig. 6: Similar to Fig. 3 from the original manuscript. Thick lines are as before, thin lines represent unsmoothed quantities.

Fig. 7: Difference between panels 6e and 6b (of the original manuscript). The plot shows standardised zonal mean zonal wind averaged between 40 and 60N for various lags. Data are from the model.

Fig. 8: Difference between panels 7e and 7b (of the original manuscript). The plot shows standardised zonal mean zonal wind averaged between 45 and 75N for various lags. Data are from ERA-20C.

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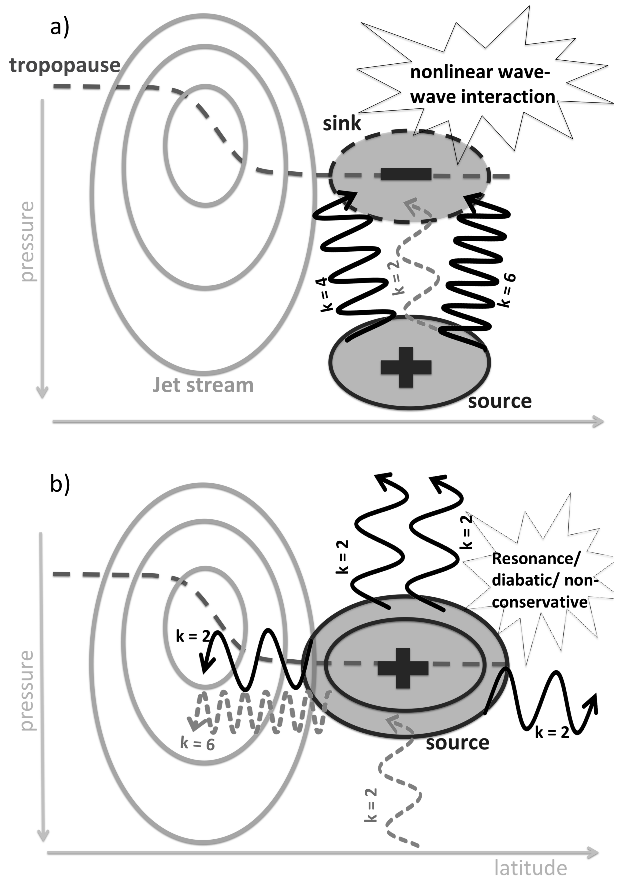

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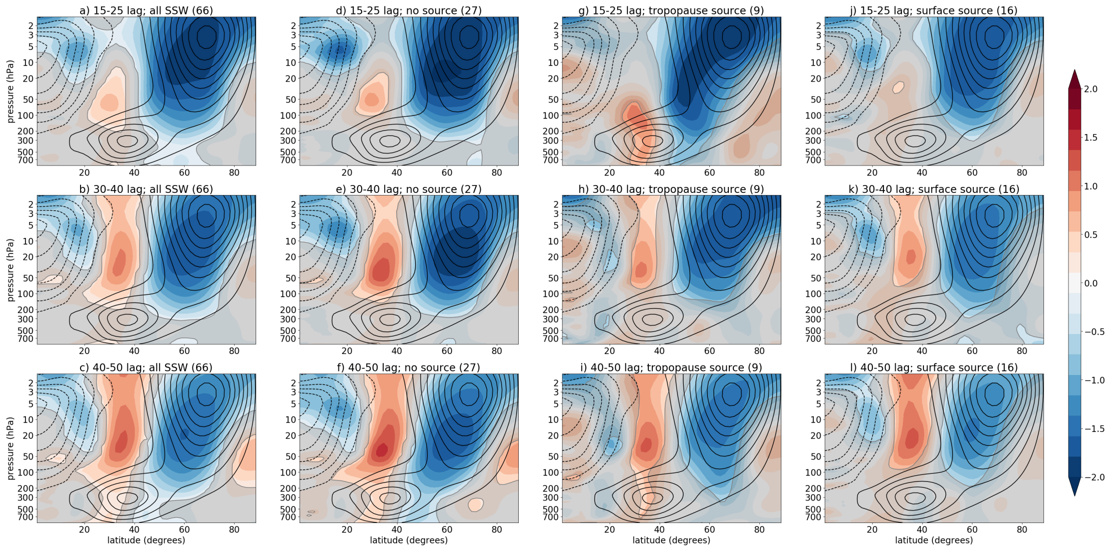

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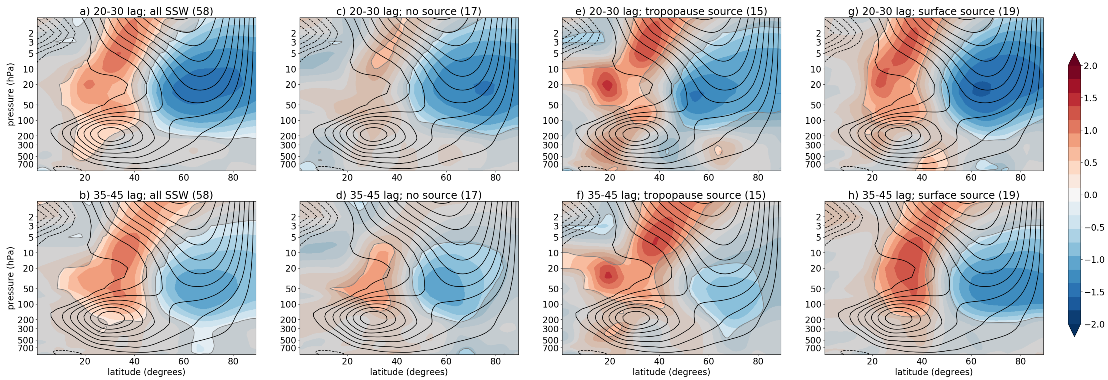

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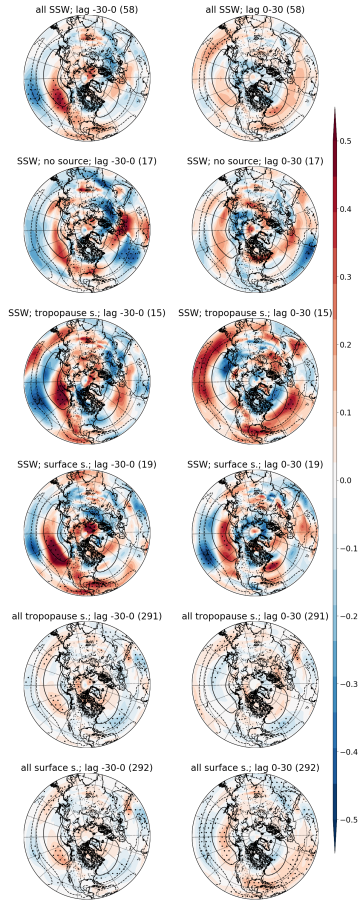

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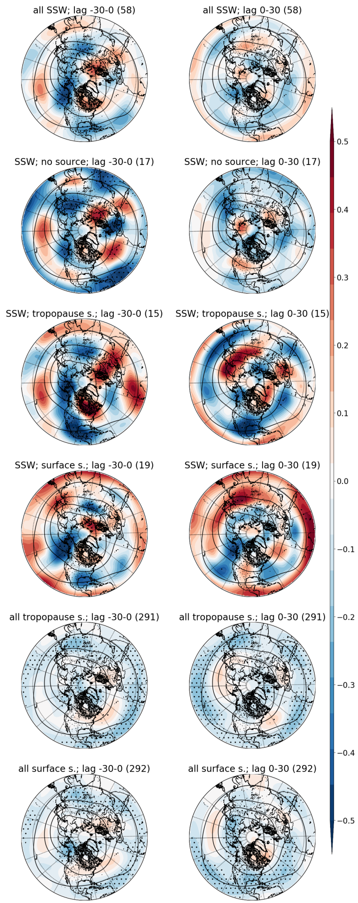

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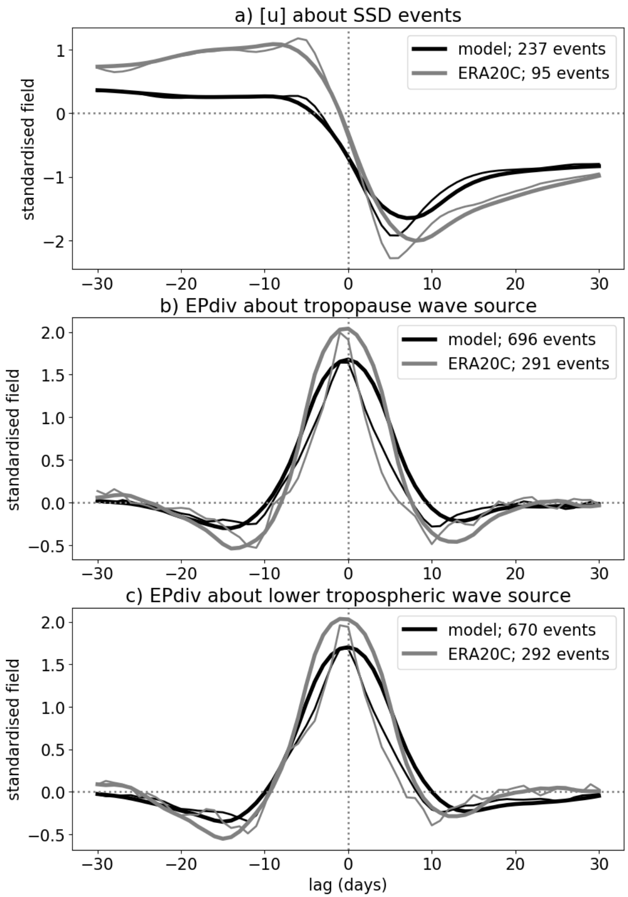

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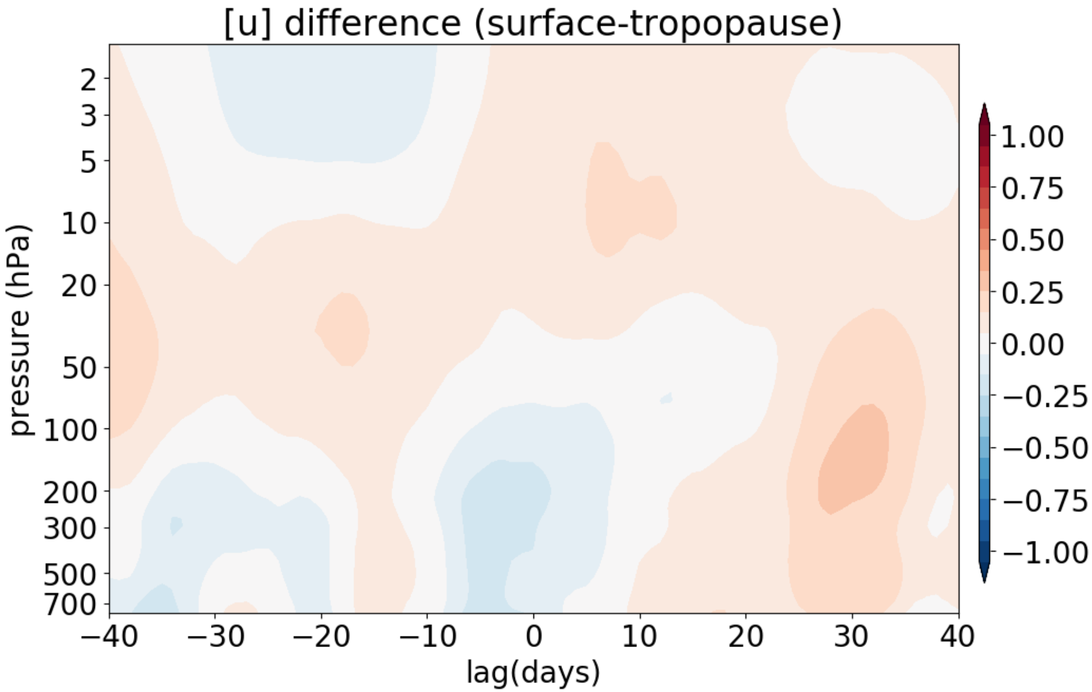

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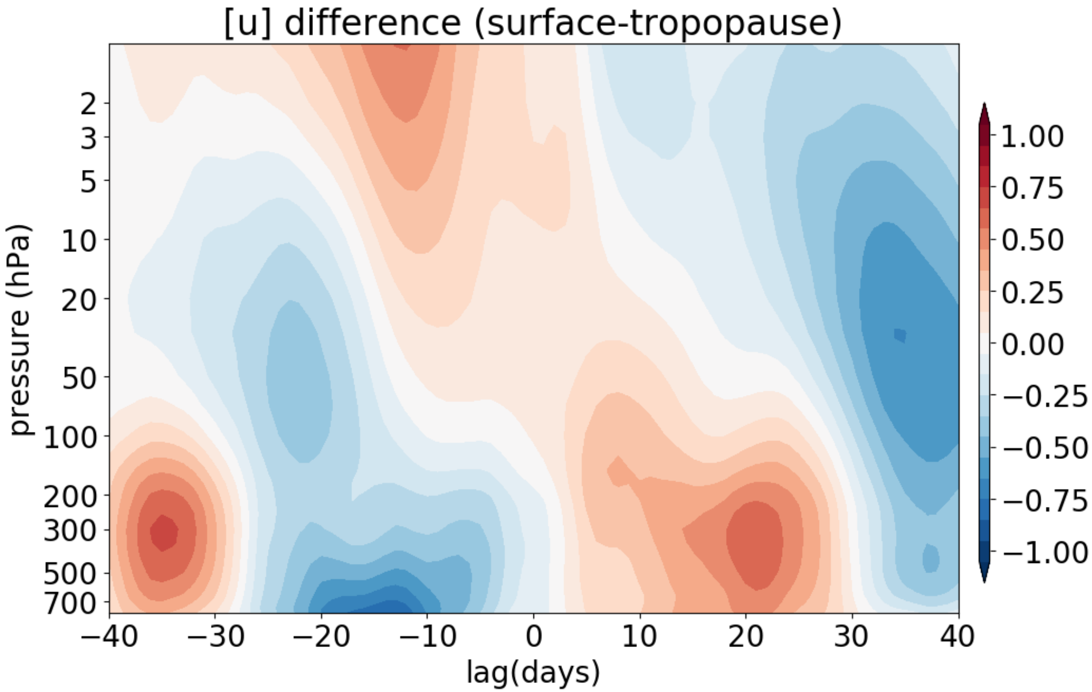

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