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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 2

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; the line numbers/figure references in the reviewer's comments refer to the original manuscript. The reviewer's comments are in italics; our responses are in normal text.

This paper addresses an important subject in stratosphere-troposphere coupling, which is how weak vortex events are ultimately forced. In particular, the authors concentrate on the appearance of a wave source around the tropopause in contrast to most work considering surface or mid-tropospheric wave sources. It is good to see new ideas explored, and I am looking forward to seeing this published. However, there are a few issues as described below which need to be addressed.

Major comments

1) My first major comment is about the model selection. a) The Held-Suarez/Polvani-Kushner model has been known to exhibit somewhat unrealistic dynamical behavior. Adding topography has mostly fixed the jet position issue, but what is probably more important in this particular study is the generally too low tropopause. I do not know what starting the stratospheric setup at 200hPa instead of 100hPa as described does to this bias, and there is no mention of any model validation in the manuscript. For instance, it is not true that dry dynamical cores do not produce SSWs with k=1 topography. The authors cite Sheshadri et al (2015), which is the model setup they use, but there are various other, improved versions of the dynamical core which do produce SSWs with wave-one (or realistic) topography. b) Throughout the manuscript, there are important differences between the model and ERA-20C, but those are not critically discussed. For instance, Fig. 4c) shows synchronous wave sources at both the surface and the tropopause, whereas 5c) indicates a possible propagation from the surface (at -10d) to the tropopause (around 0) and then back to the surface (at 5-10d and later). This is similar for Figs. 6a) and 7a). Fig. 6b) shows a surface source in the tropopause composite, whereas 7b) does not. Also, while shortly discussed in the manuscript, the preconditioning of the stratosphere in Figs 6 and 7 has a very different structure in the model vs ERA-20C, and so does the zonal mean zonal wind anomaly in Fig. 8. In Table

1, some percentages are close to ERA-20C, some are not, but there is no discussion of the confidence in the model results.

I think these differences are qualitative and require some discussion. Or the use of a different model with more realistic behavior.

(a) The Polvani-Kushner setup has been extensively and successfully used for studying stratospheric dynamics and thus we feel comfortable using this particular setup - also because it offers comparisons to various previous studies. Also, as the main interest of this paper is the role of the tropopause wave source in coupling between troposphere and stratosphere (presently not well understood), a simplified mechanistic GCM is used, which allows further insight into the dynamical processes involved. The low tropopause issue is common in the tropics, but not in the midlatitudes - in fact, in our setup the tropopause is slightly higher up (thus the tropopause wave source is at higher altitude compared with ERA-20C). The 200 hPa transition was proposed as the tropopause layer in the Polvani-Kushner setup is too deep (i.e. unrealistic) and the 200 hPa transition somewhat helps with this. The displacement events are much harder to produce in the Polvani-Kushner setup than splits - even with k=1 topography as was clearly demonstrated by, e.g., Sheshadri et al (2015). If a more realistic setup or topography is used then also displacements become more common, but this is not the point of our study. We will further clarify these points in the manuscript.

(b) While we agree with the reviewer that these differences require more discussion, we would also like to point out that there are many similarities between the ERA-20C and the model (see also the response to Reviewer 1, comment 3). We will add further discussion about those differences in sections 4 and 5. Note that some similarities of the model with ERA-20C are remarkable and as such we are confident that the model results are representative of the zonal mean response, but less so for any local responses (e.g. Atlantic vs Pacific - again, see also the response to Reviewer 1, comment 3).

2) Another important comment is a missing clear acknowledgment that most SSDs do not have any of the two wave sources. I understand the subjective selection to be able to study "clean" examples, and they do reveal interesting physics. But I feel that the manuscript is missing an estimate of how important the studied mechanisms really are. Another point is that only 10% of the tropopause source events are also SSDs (4% in the model), so what is happening with the other 90%? Same is true for surface source events.

Note that in ERA-20C (an approximation to the real atmosphere) $\sim$30% of the SSDs are preceded by a tropopause wave source, thus the tropopause wave source is clearly important for the dynamics of SSDs in the real atmosphere. Moreover, this is a comparable percentage of SSDs that are preceded by the surface wave source, which have been studied extensively in the past. In the model the purpose is not to reproduce the same percentage of SSDs preceded by tropopause wave source, instead our aim is to study the events in a simplified setup. What is important here is that the model indeed exhibits tropopause wave source events and that these events can precede SSDs, allowing us to study the dynamics of wave source events and SSDs. The bias of the model towards smaller frequency of SSDs preceded by the tropopause wave source events may be a consequence of the model dynamics. Thus studying the reasons behind this bias could reveal some important aspects of the model dynamics, however this is beyond the scope of this study (left for future work).

Note also that the goal of our study is to advance our understanding of the role of the tropopause wave source in driving the SSDs, which are notoriously hard to predict. Thus, a better understanding of any precursor signal that can improve their predictability is useful, even if only a fraction of, e.g., wave source events result in SSDs (true for both surface and tropopause wave source events). That only 10% of the wave source events precede SSDs is a consequence of the stratospheric dynamics - i.e. some preconditioning in the stratosphere should be present for SSDs to occur (see, e.g., Hitchcock and Haynes 2016).

We will clarify these points in the revised manuscript.

Minor comments

L 28-29: "Here note ..." should probably be moved to line 24, where Charney and Drazin (1961) is already discussed.

We will move it to l. 25 after the sentence, instead of within it.

L 31-32: This list of references is too long. It does not concentrate on the most important works but is not exhaustive either, and seems to mainly serve self-citation.

We will cut some of the references.

L 42: "suggest that an anomalous ..."

We will change this phrase as the reviewer suggested.

L 43: Same as L 31-32: too long but not exhaustive list of references. Also, shouldn't "Camara et al." read "de la Camara et al."?

We thank the reviewer for pointing out the "de la Camara" issue (official bibtex files had it wrong). As for the reference list - we will remove some of them.

L 102: "Note that the wave decay..." is not necessary as this is discussed immediately afterward.

We will remove the sentence.

L 113-14: "Therefore, even...": you just concluded that div(F) = 0, so where is the "significant increase"? I am sure you are trying to say something different, but this *is confusing as written.*

If there is a significant decrease and then a significant increase (exceeding 0.75σ), and they equal each other, integral over time gives us div(F)=0. We will rephrase the sentence 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."

L 124: "(sink)": this is a bit confusing, as at first I thought this was meant to mean that the waves are sinking downward. Maybe move this into the parenthesis "(negative EP flux divergence, i.e. a sink)" or similar.

We will move "sink" after "negative EP flux divergence" within parantheses as suggested by the reviewer.

L 131: Note that the newly created k=2 wave can also cancel an existing k=2 wave if the phase is opposite. Try and be more careful when describing the triad interactions.

We agree with the reviewer and will therefore mention the cancellation as well.

Paragraph 2.2: Note that while valid, the triad interactions can (and probably will) only convert waves partially, i.e. there is going to be partial dissipation and resonance, plus other effects, so a comment on the testability of this process would be welcome.

We agree with the reviewer. Reviewer 1 has also pointed out some caveats about the 'resonance' we were mentioning. Resonance was never meant as the only possible mechanism here, thus we will add further options for enhanced EPFD, and more carefully discuss the resonance part in the majority of the manuscript (see also the response to Reviewer 1). We will add a comment about the dissipation in the revised manuscript.

Paragraph 3.1: Maybe change title to "Model & Data"

We will change the title as the reviewer suggests.

L 164: 0 hPa is at infinity, so surely the model top is somewhere else?

The top model interface is at 0 Pa (which is at the top of the atmosphere in pressure coordinates, not necessarily in the infinity), but the top half-level is at ~ 7 Pa. See also Held and Suarez (1994) - section 3a, where they say that the model top is formally at 0 Pa.

L 193: 20-days $->$ 20 days

We will change it accordingly.

Paragraph 3.3: It is ok to subjectively select a few "clean" events, but there should be an estimate of the relative importance of what you are filtering out, i.e. you select a few events which show your mechanism, but in the grand scheme of things, how important is that mechanism?

We mentioned that generally speaking the two are hard to separate as they tend to occur simultaneously, likely amplifying each other. Thus, the two mechanisms are important - we can also see them in an average over all events (as mentioned in the text). However, other accompanying effects cannot be excluded, i.e. rare events may have a different origin. We will clarify this further.

L 332: remove "(section 5)" as that's the very next thing.

We will remove this remark.

L 363-365: This relates to some of the comments above: Do you have any interpretation as to why only 10/4% result in SSDs and what happens with the remaining 90/96%?

I have mentioned this above as well - this is likely because the stratosphere has to be in the right state (e.g. Hitchcock and Haynes 2016). We will add this comment in.

L 424: "into deep stratosphere": maybe change to "deep into the stratosphere"

We will change it.

L 439-440: Do the authors have any suggestion about how one could check for selftuning or downscale cascade? Not necessary, but would be very helpful for future work.

For downscale cascade we could use a similar approach as used here for upscale cascade, however the self-tuning resonance is a more complex issue; both are well beyond the scope of this study. This comment was merely meant as a remark - we will mention that this could be looked into in the future.

L 444: Again, there are others with this idea

We will change the list to include other authors as well.

L 491-492: The difference between model and ERA-20C seems much larger than the difference between tropopause and source wave source events. Can you then really assert that there is a difference between tropopause and surface wave source in terms of downward impact?

Note that we made a mistake when producing Fig. 8, which will now be updated. The revised figures (see Figs. 2-3 in response to reviewer 1) that will replace Fig. 8 of the original manuscript show more similarities between the model and ERA-20C

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than the original Fig. 8. While some differences remain, the differences between the tropopause and surface wave source are generally larger than differences between the model and ERA-20C. The difference between the surface and tropopause wave source is especially pronounced in ERA-20C (see also response to reviewer 1, comment 3).

L 515-516: "While cases..." there is something missing in this sentence.

This will be changed to: "While there are cases..."

L 517: occurs, the

We will add the comma as the reviewer suggested.

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