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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

Anonymous Referee #2

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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.

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.

Minor comments:

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

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.

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

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."?

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

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.

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.

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.

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.

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

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

L 193: 20-days -> 20 days

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?

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

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%?

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

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

L 444: Again, there are others with this idea

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?

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

L 517: occurs, the

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