Review of

Diagnosis of Local Gravity Wave Properties during a Sudden Stratospheric Warming

by

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This paper diagnoses local gravity wave properties during a series on minor stratospheric sudden warmings (SSWs) which occurred in the Northern hemispheric spring 2016. The authors focus on only a few wave properties as amplitude and wavenumber which are retrieved from gridded 3D horizontal divergence data by applying a well-established methodology to gravity waves. From these quantities, wave action is estimated which is the key quantity of the SSW analysis. Altogether, the paper suffers from too many weaknesses, and I recommend rewriting the paper, maybe even split it in two separate contributions. The main critical points are listed in the following and will be explained in more detail afterwards.

(1) As a first impression, the paper reads as an attempt to combine the presentation of an analysis tool (called "UWaDi") for estimating kinematic gravity wave properties with the discussion of the gravity wave propagation during a prolonged period of minor SSWs. Unfortunately, I've to admit: This attempt totally fails as neither the analysis tool part nor the gravity wave analysis are substantial enough to allow for a combined scientific publication.

(2) The methodology to retrieve gravity wave parameters is not convincingly introduced and clearly outlined for global 3D gridded data. Compared to solid and mathematically exact descriptions, e.g. provided by Zimin et al. (2003), the mathematical part is poor, see comments below. Especially, it is not necessary to repeat that the method is working for synthetic data as this was documented by others already.

It would be much more interesting to see the application of the method to gravity wave packets using 3D IFS analysis fields of horizontal divergence step-by-step. Essential parts are missing in the description: extraction of wave packets (not all regions of non-zero divergence belong to gravity waves) and proof that the extracted wave packets really satisfy the dispersion relationship.

Another point: The horizontal divergence is a quantity which can hardly be observed in the atmosphere. I miss a clear link to observable quantities like temperature fluctuations. There are published attempts, e.g. by Khaykin et al. (2015)[1] to do so. Without such a link, the whole analysis tool is probably handy for gridded data but gives no quantitative relation to observations in the real atmosphere.

(3) The analysis of the minor SSW is totally incomprehensive. It is not clear what the relation between time/space is and which mean values, which locations are considered. There are several hypotheses formulated and statements given in the text which are not proven by results presented in the paper. Is there any progress in knowledge, new

[1] Khaykin, S. M., Hauchecorne, A., Mzé, N. and Keckhut, P. (2015), Seasonal variation of gravity wave activity at midlatitudes from 7 years of COSMIC GPS and Rayleigh lidar temperature observations. Geophys. Res. Lett., 42: 1251–1258. doi: 10.1002/2014GL062891

understanding compared to the results on selective wave transmission during SSWs published by Dunkerton and Butchart (1984)[2]?

(4) The writing is often very sloppy and not precise. Definitions are modified without discussing the implications, see remarks about wave action. The whole style of the paper is essentially not acceptable for a scientific publication. There is a frequent mix between presenting of results and discussions which blurs the paper and makes reading more than difficult. Below, I give several examples without attempting to edit through the whole text. This would take too much time and effort I cannot spend. I actually stopped reading and commenting after Sec. 3.2. This does not mean, afterwards is all fine. It just means, I see the action by the authors to improve the whole text.

Generally, I noticed a tendency to name, denote facts and processes with new, partly fancy terms (mostly taken from hydromechanics for what reason ever) which are not exactly defined or explained in the text and which leave room for associations. I just want to remind the authors on one principle, scientific publications should follow. It is known as Occam's razor and says "Entities must not be multiplied beyond necessity". It would be great, if the author could follow this principle in future publications. Take as an example the naming of the analysis tool. Why a new name is created for a well-documented methodology which has been obviously used several times before? Well, maybe for other scales and maybe also because an approximated form of wave action is calculated here, but it is absolutely not clear why this minor modification should be named with "Unified Wave Analysis". What does “unified” mean?

The quality and labeling of some of the figures is poor. Examples are given below.

(5) Essential references are missing in the text. The authors focus on the winter 2015/16. They totally ignore papers which are even published from authors of their own institution! Examples are given below.

Last but not least, clear-cut formulated scientific questions are missing for both parts of the paper. So, the suitability of the paper to fit within the scope of ACP cannot be evaluated so far. And maybe, to formulate scientific questions might be a suitable starting point for a new attempt to publish results of the presented study. Thus, at the end, I recommend to proceeds on two routes. First, outline the new facets of the wave analysis clearly and publish these as an independent methodological contribution, e.g. to the GMD. Secondly, conduct a thorough study of the sequence of minor SSWs which occurred in January/February 2016. If the increment of knowledge gain is measurable and constitutes a significant contribution to the understanding, such a paper would fit perfectly to ACP!

[2] Dunkerton, T.J. and N. Butchart, 1984: Propagation and Selective Transmission of Internal Gravity Waves in a Sudden Warming. *J. Atmos. Sci.,* **41**, 1443–1460, https://doi.org/10.1175/1520-0469(1984)041<1443:PASTOI>2.0.CO;2

Specific Remarks:

Abstract

line 1: These two sentences are incomprehensible. What do they mean? Furthermore, Abstract is not a place to argue.

line 2: Reads like a technical task which is the topic of the paper. Formulation and grammar is unclear: What is a "diagnostic tool for studies of wave packets locally"? Do you mean: "retrieve localized wave packets from 3D gridded data"? The following sentence with "UWaDi" confirms the impression of a technical study.

line 4: Be more specific: you use 6 hourly operational analyses of the IFS? Why do you use such a general formulation as " ...is used to perform the analysis"? Write exactly what you do with the data: they are interpolated on a spatially equidistant grid to apply the Hilbert transformation to extract amplitudes and wave numbers at specific times ....

line 5: The first result appears (about the effect of the sponge layer). Is this an essential result of the applied method to be mentioned first? Does it undoubtedly relate to the assumed numerical damping or is there a possibility that the atmospheric state simply didn’t supported gravity waves? See remarks to Sec.2.3.

line 7: Second result, however, incomprehensible again. What means " zonal mean wind quantities cannot reveal local 'valves' ...". The usage of not generally accepted terminology or terminology which is not yet introduced in the previous text is dangerous and does not explain anything. What are "zonal mean wind quantities"?

Line 8: third result: obviously, one event of the mentioned three cases (line 6) is picked randomly which states high gravity wave activity without any relation to location and height. And again a term "local pump" is used which does not explain anything. Why these relations to hydro-machines?

line 9: Why "Accordingly"? What shall the reader re-connect in order to conclude about the advantages which are stated?

At the end: The Abstract is incomprehensive and incomprehensible, and it leaves more questions than answers! It needs a thorough re-write and focus either on methodology or SSW dynamics.

1 Introduction

Generally, an Introduction should contain the state-of-the-art knowledge of the topic which is going to be addressed in the paper. It should formulate the challenges and the methods which are applied to answer the scientific questions resulting from the challenges. At the end, the answers are given in the Conclusions where you should clearly state what kind of new knowledge has been generated by the research conducted for the paper. Unfortunately, this Section 1 only partly serves this purpose.

First paragraph

PAGE 1

line 12: provide evidence by adding essential references

line 12/13: The logic of the sentence goes wrong: Do "the scales of GWs ... create a broad field of interest .."?? I don't think so. Furthermore, do you really claim that atmospheric gravity waves exist at 10 m scale??

line 14/15: What do you mean with "huge changes in GW appearance"? Where? When? Increase? Decrease? Provide evidence by references. Be more specific. For example, mention that you consider the Northern hemisphere only and specify the physical variables you are referring to.

line 15/16: This classification relies on the definition of "normal winter conditions" and "summer-like conditions". Specify what is meant! Which months are you referring to? Early winter, late winter? The use of these terms is an example where the application of the principle of Occam's razor would be beneficial.

Essential references about SSWs are missing, also at lines 18-20. Start with

Butler, A.H., D.J. Seidel, S.C. Hardiman, N. Butchart, T. Birner, and A. Match, 2015: Defining Sudden Stratospheric Warmings. Bull. Amer. Meteor. Soc., 96, 1913–1928, https://doi.org/10.1175/BAMS-D-13-00173.1

and find relevant references therein.

line 17: What are "winder" conditions?

lines 21-23: Very colloquial language! Be specific what the “crucial role in driving …” means

lines 23-25: Be more specific, not so general. Attention by using the term "wave guide": in the cited paper (Dunkerton and Butchart, 1984) this term never appears and, mostly, it refers to horizontal propagation. I think you might refer to the concept of selective wave transmission instead which was introduced by Dunkerton and Butchart (1984). Again: very colloquial language.

line 25: This is a rather general statement. Ask yourself what specific facts, information do we need from the cited papers for introducing your research topic! Just the statement that their data can be analyzed seems to weak!

PAGE 2

line 2: Do De Wit et al and the other cited papers really "verify" the momentum fluxes analyzed by the mentioned modeling papers? Be more specific and keep an eye what is needed in your text. As far as I see, momentum flux does not play any role in the paper!

line 4:

- The statements of the Ern et al. (2016) seem to be essential: Describe what is exactly meant with the "zonal average view of GW parameters". Then, get the way to your point of local wave quantities.

- provide evidence of your statements using "mainly extracted" and "misleading" line 5: the fact that "local GW activity can vary locally" is known and best expressed in the intermittency which was derived from various observations - why such a long chain of arguments before??

line 6: colloquial: "gravity waves slow down" – be more physically exact and refer to vanishing vertical group velocity. Not all gravity waves interact with the critical level, only those whose phase speed is equal to the background wind. Good references are text books on gravity waves as Nappo (2012), Sutherland (2010), Gill (1982), Gossard and Hooke (1975), ... or the papers of Bretherton (1966, 1969)[3] and Booker and Bretherton (1967)[4].

line 7 and 8: Introduce and explain physically what is meant by the used terms ("valve" and "bottleneck" and “pump”) as you are now making the step from background conditions to local flow regimes.

line 8 and 9: statement of the goal of this study, I suppose. Why test case? What is the emphasis of this study? Is it the methodology or the analysis of the minor SSWs? Focus on one or the other. To keep both alive does not work!

Altogether, the **whole first paragraph** contains too many aspects which do not logically lead to a clear goal formulated in terms of scientific questions. Even the last sentence leaves it open what the paper is focusing on. It does not become evident what the scientific problem is nor why it is timely to conduct such an analysis being presented in the paper. There are vague associations that some kind of previous wave analysis is giving results which will be contrasted (improved, complemented??) with the results of this study. But, at the end, the paragraph is not saying this explicitly and remains incomprehensive.

Second paragraph

line 10/11: a very general statement that combines too many aspects: Specifiy the data you are going to analyse! What is meant by "local phenomena and their coupling"? Give evidence for the statement ".. resolve essential parts of GW dynamics .." - in which sense essential?

line 11/12: provide reference, why already? What is meant with "correct GW appearance"??

line 13/14: why the link to the tropics is necessary? Refer specifically to the results of Yamashita et al. if they are relevant for the present study.

lines 14-20: provide evidence for the ".... bigger portion of resolved GWs ....", this is just a statement, are there references? The collected arguments and statements do not convincingly lead to the concluding sentence starting with "Hence, ...". First of all, the requirements were never specified before. Secondly, the term "local valves" is not defined yet.

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[3] Bretherton, F. P. 1966 ‘The propagation of groups of internal gravity waves in shear flow,’ Quart. J. R. Met. Soc., 92, *pp.* 466–480
Bretherton, F. P. (1969), Momentum transport by gravity waves. Q.J.R. Meteorol. Soc., 95: 213–243. doi:10.1002/qj.49709540402

[4] Booker, J., & Bretherton, F. (1967). The critical layer for internal gravity waves in a shear flow. *Journal of Fluid Mechanics, 27*(3), 513-539. doi:10.1017/S0022112067000515

I’m trying to guess: you claim that the IFS data provide the locations of wave-induced critical levels?? This might be true if one would know of which part of the GW spectrum you are talking about. Essentially, this aspect of resolution dependence should be discussed in detail to provide fair ground for further arguments. The presented arguments are too general. Moreover, there are quite a few case studies of the recent years using high-resolution analyses and forecasts of the IFS to derive local wave parameters, just to name a few:

Zhao, J., et al., 2017: Lidar observations of stratospheric gravity waves from 2011 to 2015 at McMurdo (77.84° S, 166.69° E), Antarctica: Part I. Vertical wavelengths, periods, and frequency and vertical wavenumber spectra. J. Geophys. Res., DOI: 10.1002/2016JD026368

Ehard, B.,et al, 2017: Horizontal propagation of large-amplitude mountain waves in the vicinity of the polar night jet, J. Geophys. Res., Atmos., 122, doi:10.1002/2016JD025621

lines 19-21: It is not convincingly explained why such an analysis is necessary. And what does such an analysis add to the understanding of internal gravity waves? What are the challenges? Why is such an analysis necessary?

Again: also the second paragraph should be much better structured and focused on the needs which lead to the presentation of the presented approach to analyze gravity waves.

Third paragraph:

lines 22-line 9(**PAGE 3**):

This paragraph starts with sentences about sources (why not name them as non-orographic sources) and at line 24 it jumps to methods to extract wave properties: I would recommend to separate these both issues.

- what means “varying” in "search for varying GW sources": different, variable, transient, ...? Regarding the logics in the first sentence: Why is there “Another issue … because there is some likeliness of ..."? No idea what this means and implies.
- I don't like the formulation " … which may 'pump' them into the middle atmosphere .." Why "pumping"? Why this analogy to hydro-machines? Waves are excited and they propagate in response to the ambient properties (wind, stability) of the medium. Physically, there exists an established terminology: vertical flux of wave energy or wave action (see again: Occam’s razor).

line 25: provide evidence by proper references (" .. found in the literature."); the 2nd sentence in this line, and the following one too, remain incomprehensible as nobody knows what are you referring to. Also, the concluding sentence starting with "Hence, .." (line 26) cannot be verified based on the information you provided.

line 27 - 35: Explain why the mentioned methods are relevant for the present study. From reading this part and scanning through the mentioned papers, I've got the impression that essentially all methodology to derive " .. wave amplitudes and wave numbers .." is available. What is the challenge and the need to present another method? I might be misled, but: you as the authors are responsible to make clear what the community is missing in terms of knowledge and/or methodology. And: what are you going to add with your paper to close this identified gap! This is not obvious from the present text.

PAGE 3

lines 1 -9: Again, it would be beneficial if the reader would be provided with more accurate information. For me, it is rather nebulous what is taken from the published methodology and what is missing and will be added here.

lines 10-18:

The two goals are reformulated: (1) a new method is introduced here "to obtain phase-independent wave properties locally"? What specifically is meant? Amplitudes only? and (2) "local valves" are going to be detected by considering the vertical GW propagation through the varying background conditions during a mSSW (abbreviation not introduced yet).

"valve detection" – explain exactly what you mean.

- Here, you state you use “reanalyses” (line 12) but later I learnt, these are the operational analyses. Consistency in naming required! This also refers to the new terms “prewarming, midwarming, and postwarming” phases (line 17). Are these the same periods as the stages mentioned earlier on page 1, lines 16,17)???

2 Method and Data

Line 19-23: In a potential methodological paper, the very short technical description could be expanded by a code description. Otherwise, the hints to “autonomous” processing and plotting and user-defined namelist as elements of the actual code do not make sense here.

Section 2.1:

- about the name “UWaDi”, see above
- line 26: give the range of x-values
- lines 25/27: the Hilbert transform does not “provide a new complex series” – the complex values are determined by Eq. (1) <u>by means of</u> the Hilbert transformation
- the mathematical description is poor as the definitions of DFT and F are not given; are these the same formulae as in Zimin etal (2003)? As a matter of fact, the interested reader should be able to code your algorithm solely based on the equations you provide and on references which exactly point to ingredients you used – this is not possible with the provided information.
- are the quantities calculated by Eq. (1) and (4) the same?
- **PAGE 4**, line 9: I don’t think “maintain” is the appropriate verb here, the amplitude or magnitude of a complex number is simply defined as written in Eq. (5); I think, the formulation “ .. gives an estimate of the local envelope …” is not correct. Shouldn’t it be the amplitude of the wave packet?
- line 23: “First” instead of “Fist”
- Generally, the reference to wave packets and the identification of them is missing!!
- What is the physical meaning of the phase (Eq. 6) with respect to the wave groups?
- In Eqs (8) and (9) indices “d” are used. Later, “d” is used as abbreviation for the vector of Cartesian coordinates.

- The filtering and smoothing, and the quality checks are not explained in a transparent way!

A concluding paragraph about the advantages of the new method would facilitate the understanding and judgment of the presented algorithm.

Section 2.2:

To conduct the presented tests was certainly necessary to code the algorithm properly. However, as the results are neither surprising nor new, I would recommend skipping this part. Instead, the application of algorithm to a 1D series of horizontal divergence along a constant latitude circle at some selected altitude (taken from the IFS data) would be a convincing test if the algorithm really retrieves wave packets and leads to a realistic estimate of amplitude and wavenumber.

Section 2.3:

PAGE 6

line 18: “ca.” ???

PAGE 7

It appears that the authors only have limited information and knowledge about the physical parametrizations and the additional filtering and damping in numerical weather prediction models, especially, the IFS cycle they have chosen for their analysis. The main part of the damping in the stratosphere is due to the non-orographic wave drag formulation introduced several years ago (Orr et al., 2010)[5]. Terms as “stratospheric sponge” and “mesospheric sponge” do not describe properly what is done in the model integrations. Essential references are missing which describe the older status of filtering and damping (Jablonowski and Williamson, 2010)[6].

As mentioned above, it is simply assumed that the fading of the waves in the upper stratosphere is due to numerical damping alone. However, physical effects and ceasing wind above the polar night jet might be another reason for wave attenuation. Here, wind lidar measurements or the meteor radar winds (see Fig. 2 in Stober et al, 2017) during the SSWs of spring 2016 conducted by colleagues of the home institution of the authors could clarify at least part of the situation during the minor SSWs.

lines 38-42: As far as I know, the pre-processing step of WRF not only interpolates the data on a regular Cartesian grid it also applies some sort of balancing the field to satisfy the WRF equations. There were also scale factors introduced: u and v are multiplied with them to account for the projection used later on. Did this impact the results? Specify

[5] Orr, A., P. Bechtold, J. Scinocca, M. Ern, and M. Janiskova, 2010: Improved Middle Atmosphere Climate and Forecasts in the ECMWF Model through a Nonorographic Gravity Wave Drag Parameterization. *J. Climate,* **23**, 5905–5926, https://doi.org/10.1175/2010JCLI3490.1

[6] Jablonowski, C. and D. L. Williamson (2011): "The Pros and Cons of Diffusion, Filters and Fixers in Atmospheric General Circulation Models", In: Lauritzen, P. H., C. Jablonowski, M. A. Taylor, R. D. Nair (Eds.), Numerical Techniques for Global Atmospheric Models', Lecture Notes in Computational Science and Engineering, Springer, Vol. 80, 381-493.

exactly which part you have applied to pre-process your data. How was the horizontal divergence calculated? Did you take the ECMWF values or are they calculated by means of WRF-pre-processing? Why was band-pass filtering necessary?

Section 2.4:

- Eq. (13): How is s_delta defined? How is Eq (13) derived? Which assumption went into the derivation? Unfortunately, also the mentioned reference is not very helpful either.
- Can you give a reference to the statement in line 21?
- Eq. (14): I learned that wave action is the mean wave energy (E_KIN+E_POT) divided by the intrinsic frequency, for example Sutherland (2010) Eq. 3.94 or Gill (1982) Eqs. 8.12.33 and 8.6.1. Obviously, Eq. (14) and using “e” as the E_KIN is an approximation. Can you comment why you neglect E_POT?
- Line 28-31 and

PAGE 8

Lines 1-3: you should discuss properties of the wave action and how wave action is changing in s sheared environment!

3 Results

Section 3.1 The stratospheric conditions in winter 2016

Reading such a headline (I would modify the last part to Arctic winter 2015/16), one would expect that the authors have undertaken a literature research what has already been published about the winter 2015/2016. And there are indeed some articles. Just to mention a few:

Matthias, V.*,* A. Dörnbrack*, and* G. Stober *(*2016*),* The extraordinarily strong and cold polar vortex in the early northern winter 2015/2016*,* Geophys. Res. Lett.*,* 43*,* 12,287–12,294*, doi:*10.1002/2016GL071676*.*

Manney, G. L. and Lawrence, Z. D.: The major stratospheric final warming in 2016: dispersal of vortex air and termination of Arctic chemical ozone loss, Atmos. Chem. Phys., 16, 15371-15396, https://doi.org/10.5194/acp-16-15371-2016, 2016.

Stober, G., Matthias, V., Jacobi, C., Wilhelm, S., Höffner, J., and Chau, J. L.: Exceptionally strong summer-like zonal wind reversal in the upper mesosphere during winter 2015/16, Ann. Geophys., 35, 711-720, https://doi.org/10.5194/angeo-35-711-2017, 2017.

Dörnbrack, A., S. Gisinger, M.C. Pitts, L.R. Poole, and M. Maturilli, 2017: Multilevel Cloud Structures over Svalbard. *Mon. Wea. Rev.,* **145**, 1149–1159, https://doi.org/10.1175/MWR-D-16-0214.1

All of them deal inter alia with meteorological conditions in the stratosphere, with planetary wave activity, with SSWs, and, eventually, with gravity wave activity in the Arctic. So, they are highly relevant and totally ignored here. As mentioned above, this is not understandable as two of these publications come from the same institutions as the authors themselves.

The section 3.1 is not very focused as it mixes the presentation of meteorological results (mean state in terms of U, Z, gravity waves in terms of DIV, and results from the wave analysis) from the Jan/Feb 2016 period with the discussion. So, a strict separation of presenting results and the discussion is highly recommended to enhance the readability of the text. Furthermore, the comparison to so-called long-term observations in Lindenberg and campaigns in Kühlungsborn is not convincing as the link to SSWs is not obvious. The question stated at the end of line 14, **PAGE 9** is either foolish or not necessary as everybody knows that SSWs are large-amplitude PW events deviating the flow from long-term averages.

line 8: Are these zonal mean zonal winds plotted in Fig. 3? Clarify this in the text!

line 9: Specify the exact criteria which are used to determine the dates of the minor SSWs? From Fig. 3, there is only information about U.

line 15: What are you referring to? Which “diagnosed GW properties” do you mean? Do you refer to the mean values presented some lines above?

line 17: The first sentence manifests the dilemma of the approach which is followed in the whole Section 3: The authors assume a (I assume local) relation between zonal wind and gravity wave activity without explicitly considering the conditions for excitation and propagation. They selected special geographical locations (60°N latitude band, some place near Greenland) and consider the conditions there <u>without taking into account</u> the generation of gravity waves at remote places and their horizontal propagation. At the end, this cumulates in the 1D mechanical analog applying “pumps” and “valves” presented in the final Fig. 9.

line 20: there is inconsistency: here and in the Fig. 4 you say: U, Z at 30 km altitude. But how can you plot Z at a fixed altitude? Maybe, the caption is right saying that the plots are at the 10 hPa pressure surface?! Clarify!!

line 21: What “uniformily distributed wind” mean? As the wind consists of a magnitude and direction, a ring vortex can hardly ever have such property.

line 22: How do you define the edge of the polar vortex? Which quantitative measure you are using? There is a huge volume of literature devoted to this topic and I’m not sure what are you referring to.

line 23: A sentence like “They are supposed to ..” is ridiculous in a scientific paper! There is no proof, no evidence of “typical orographic features”, just a statement. Please, go ahead and show that this statement is true. I guess, it will be another full paper. And most probably, you will be forced to modify or revise your statement.

lines 23-28, also 32-35: the links to published results should be separated into a discussion chapter and not mixed with the presentation of your results here in this Section 3.

Generally: the quantification of wave activity is very sloppy although the authors applied a tool to quantify them. Therefore sentences like those in lines 31 (“In this area increased GW activity can be observed in the horizontal divergence field …”) or on **PAGE 10**,line 2 (“The horizontal divergence field shows much more fluctuations ..” should be avoided.

line 4: Avoid statement like this in the presentation of results. They belong to the discussion.

Section 3.2

PAGE 10, line 7: The logic of the sentence is strange: Why is the focus on "vertical wave propagation since… " the horizontal wavenumber is assumed to be constant?

I cannot follow the argument, why a 1D model is sufficient. You only consider conditions at 60°N! And from them you conclude later on the mechanisms which are involved. I don't think, this pure mechanistic picture is in any way related to processes in the real atmosphere. There, gravity waves are excited over widespread areas due to a number of sources at different levels from the surface to the mesosphere and they contain a broad spectrum of frequencies and wavelengths. The whole section and the following ones are based on this very strong restriction to assume a wave source near the surface and a pure vertical propagation. I think, this type of argumentation and reasoning is a big step backward from the results on selective wave transmission during SSWs published by Dunkerton and Butchart 33 years ago.

PAGE 11

line 4: "westerly orientation": first zonal wind are always east-west winds, so the orientation is clear; second, "westerly" is enough to name wind from the west.

line 8: in my understanding "wind reversal" means change of sign in U; so, in Fig. 6c I see no reversal at all; the wind must be zero by definition at the surface. Why do you mention this?

Line 10: the comparison of this statement with well-defined wave packets visible at 10 hPa (~30 km) in Fig. 4a (divergence) south of the considered band at 60°N evidently show the limited conclusiveness of the analysis. The limited stratospheric wave activity is certainly related to the respective positions with respect to the polar night jet. By the way, this finding is known since years, see the publication of Whiteway et al. (1997)[7] and papers citing his work!

On the other hand, such experimental studies could guide you to adapt your analysis strategy to available knowledge.

PAGE 13

Last two paragraphs of Section 3.2: Here, again, you pick a arbitrary location (50°W, 60°N) and build a 1D model out of it which leads to the left schematic in Fig. 9. This is not to accept as you assume that waves are exited near the surface. First of all, you should show that this is really the case. Second, what frequencies, wavelengths, phase velocities do they have? Third, even assuming that all works out fine for our reasoning: What is so different, so new in your conclusions and in the schematic from the common knowledge about critical level filtering??

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[7] Whiteway, J. A.,T. J. Duck, D. P. Donovan, J. C. Bird, S. R. Pal, A. I. Carswell, Measurements of gravity wave activity within and around the Arctic stratospheric vortex, Geophys. Res. Lett., 24, 1387–1390, 1997

You mention the link to PW activity. Nothing (!!) is shown this respect which gives evidence that the statement is true. Again: what is the progress to the paper of Dunkerton and Butchart (1984)??

I stop here.

FIGURES

Fig 1: Units are missing at the axes. The mentioned crosses are not visible. Or are these the elements of the bold lines?

Fig 2: Numbers and units are missing at both of the axes in all panels.

Fig 3: It is not clear what exactly is plotted. Zonal mean quantities? Specify! Are the graphs really at 30 km altitude? See Remark to Figure 4 in the text above.

Fig 4: Remove the irritating “30 km” label from the figures. It would be helpful not to show the horizontal divergence field alone but also the retrieved wave packets from the algorithm. The scaling of the divergence is too detailed; select a lower absolute value (e.g. 2 10^{-4} s^{-1}) for plotting.