Reply on Review Process of acp-2017-472 Version 1

L. Schoon and Ch. Zülicke

October 4, 2017

First of all, we like to thank the two anonymous referees for their time expenses to comment on our manuscript acp-2017-472 published in the discussion part of the special issue of Atmospheric Chemistry and Physics "Sources, propagation, dissipation and impact of gravity waves" on 3 July 2017. In the following we first give an overview of the main changes of the manuscript, adressing both referees and the editor (Sec. 1). This is followed by the respondence to the statements of anonymous Referee #2 (Sec. 2).

1 General Comments of the Authors

- Regarding the suggestion of Referee #2 to "improve the whole text" the authors decided to rewrite the whole manuscript. Therefore, the attached file including the highlighted changes looked very complex and we omitted it.
- Now, we attempt to guide the reader to the impact of our manuscript by highlighting more intensively its novel characters in the introductionary part. We expanded the literature research massively.
- As Referee #2 had concerns regarding the reliability of our data (preprocessed with the WRF Preprocessing System (WPS)) we thoroughly investigated the analysis data of the European Centre for Medium-Range Weather Forecasts (ECMWF) to find the best fitting data set and resolution of data during the last month. All calculations were redone and restricted to altitudes below 45 km to avoid the strong sponge layer in ECMWF data starting at 1 hPa, following the suggestion not just

of Referee #1 but also published findings in literature (Sec. 2.3). We avoid horizontal interpolation by keeping the data on the original latitude-longitude grid, adjusting our algorithm accordingly. The discussion on ECMWF data is shortended appreciably in favour of a brief literature review.

- We provide a step-by-step outline of the methods because Referee #2 doubts that the former explanation was sufficient (Sec. 2.1). We also add some calculations in the Appendix.
- Now, the application of the method is clearer arranged and trimmed to the analysis of three profiles from one time step (Sec.3).
- The concerns of Referee #2 regarding our pictoral schemes of hydromechanics, namely “valves and pumps” are taken care of. We erased this literal description of the analysed mechanisms from the manuscript.

We want to highlight again, that this manuscript focuses on the introduction of our novel method called “Unified Wave Diagnostics” (UWaDi). The application on the minor Sudden Stratospheric Warming on 30 January 2016 acts as a demonstrative application to show the advantage of this method. We plan to join the closer analysis of observations and models with respect to local features of GW generation and propagation. The authors highly recommend, that the introduction and the application of UWaDi should not be seperated and published in different journals as we prefer to join the special issue (SI) “Sources, propagation, dissipation and impact of gravity waves”. All four issues named in the title of this SI are specifically addressed in the discussion part of our manuscript. Furthermore, we hope by belonging to this SI, that other scientists interested in this topic can find simple access to our method and cooperation.

2 Comments to the Referee #2

(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.

We are sorry for this impression. We revised the whole manuscript to better point out the base of our method as well as the result of our application on the minor SSWs on 30 January 2016.

(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.

We thank the reviewer to raise this issue. In response, we added a step-by-step outline of the method. The improvements compared to Zimin et al. (2003) are highlighted in the Introduction as well as in the method part (Sec.2-2.2). We see the necessity of showing that the method works for synthetic data because with that we can point out clearly, that not just the envelope of this wave packet is estimated correctly (like Zimin et al. (2003) showed) but also the wave number calculation at every grid point (which is novel work in UWaDi) works well. This only could be done by an example where the wave number is known in advance. Furthermore, this synthetic wave packet works well as test case for the comparison of several methods (Sec. 2.2).

As described above, we prefer the synthetic test case from Zimin et al. (2003) because with that we can truely show the gain of UWaDi. The discussion of wave quantities in Sec. 3 and 4 should make sure, that we deal with GWs that fulfill the dispersion relation.

With these specifications, we used the advantage of availability of the divergence in the analysis data, which directly made accessible the wavy ageostrophic motion without the need of filtering out the geostrophic modes.

As we point out, this method is developped for gridded data and not primarily for observations. Nevertheless, we added in Sec. 2.1, Step 1 that the method works for every variable on gridded data, if numerical or dynamical filters are approved to provide the fluctuations of the background flow. We choose the horizontal divergence to overcome the use of a numerical filter. Several studies, including the named Khaykin et al. (2015) (Plougonven et al., 2003; Zülicke and Peters, 2006; Limpasuvan et al., 2011; Dörnbrack et al., 2012, 2017) use the horizontal divergence as a dynamical indicator for GWs and so do we.

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

We are again sorry for this impression. We changed the whole analysis and hope that Referee #2 sees the connection of our results to the discussion, now. The differences to Dunkerton and Butchart (1984) are pointed out in the Introduction and discussion part of the paper. Shortly: Dunkerton and Butchart (1984) investigated parameterised GWs of a different range of wave length than we do. We concentrate on resolved GWs in analysis data and its vertical propagation through the middle atmosphere. This was not done in Dunkerton and Butchart (1984). In particular, we provide such a local analysis on every longitude which is not possible with such an accuracy with other methods. This has been demonstrated in Sec. 2.2 with the Zimin test case.

(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.

We take care of this remark and rewrote the whole text. Results (Sec. 3) and Discussion (Sec. 4) are clearly seperated, now. We hope that by reading the whole manuscript, the Referee will see our effort of answering the questions asked in the Introduction, analysed and discussed in Sec. 3 and 4 and summed up in Sec. 5. We carefully took care to keep the golden threat.

We removed the terms "valve and pump" because they seem to take away the attention from our scientific goals which is to point out the longitude-dependent vertical propagation of GWs. The name of the tool is not disputable. The unified character comes from several issues. First, the method is applicable to several different parts of wave types, e.g. GWs or Rossby Waves. Furthermore, any kind of variable can be analysed, as long as it contains wave-like structures. By choosing narrow band bandpass limits one can even analyse different kinds of one wave type. Hence, it can be used for any kind of gridded data. It is applicable on one-dimensional data and up to four dimensional data. We obtain phase-independent wave quantities which makes it easy to calculate wave energy measures locally. Again, our method is based on that one introduced by Zimin et al. (2003) but comes with an extra wave number estimation in all three dimensions

(which is the major novelty) and combines the three dimensional amplitude and wave number estimates on the same grid as the input data.
The Figures are new. The labeling is taken care of.

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!

We extended the list of references to several publications regarding the Winter 2015/16.

As mentioned above, we reformulated the introduction to find scientific questions and tuned the whole text to answer those. Our comment on the seperation of the method and the application into two journals can be found above (Sec. 1).

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.
We hope the new formulation is clearer.
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.

Regarding the last suggestion of the Referee, we rewrote the abstract completely. There, all these comments were taken care of.

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.

We extended the Introduction, including more information on other methods and analyses of the winter 2015/16. We resorted it and took care of rising questions in the individual paragraphs and answering them in the corresponding paragraph of the Conclusion.

First paragraph
PAGE 1
line 12: provide evidence by adding essential references

The authors clearly point out, that an overall overview on the different scales of GWs can be found in the given reference (Fritts and Alexander, 2003).

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??

No, we do not claim that. We reformulated the sentence to make its point clearer.

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.

The new Introduction is clearer.

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.

We are more specific now.

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.

We included more references on SSWs, especially those dealing with GWs. See paragraph 6 of the Introduction.

line 17: What are "winder" conditions?

This typo is removed.

lines 21-23: Very colloquial language! Be specific what the "crucial role in driving ..." means

Rewritten.

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.

Rewritten

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!

We extended this.

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!

We removed the references regarding the momentum flux.

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"
Now, we point out clearly the advantage of our local method in the Introduction.

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??

This is rewritten.

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 .

A discussion on critical layer absorption can be found in Sec. 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.

For above mentioned reasons we removed these terms.

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!

We clearly state our goals in the Introduction, now. More comments on that can be found in Sec. 1 above.

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.*

We are sure that the Introduction is clearer to the reader, now.

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?

A detailed description on the data can be found in Sec. 2.3.

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

The reliability of the data is discussed in the Introduction, as well as in the Sec. 2.3.

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.

We refer to Yamashita et al. (2010) and removed the link to the tropics.

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.

This is rewritten.

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

We are aware of these publications and decided to add also several other studies that highlight local GW features. Especially in the last but one paragraph of the Introduction we deal with the ECMWF data. Furthermore, we made several case studies with respect to resolution and filters to find the best fitting data to our analysis (See Sec. 2.3). There the restrictions of the data are discussed, too. In particular, we found the same results using the 100 km to 1500 km filter for 0.36° and 0.1° grid size data. We interprete this finding with a GW spectrum which is rapidly decaying for horizontal wavelengths above 200 km.
Zhao et al. (2017) used ECMWF model data as background wind information to interprete vertical wavelengths from their lidar observations at McMurdo, Antarctica. For their spectral GW analysis the height range of 30 - 50 km was used. With regard to method, we quote some more complicated approaches, while for the application we focus on the SSW. Hence, for the sake of brevity we do not include this paper.
Ehard et al. (2017) concentrate on the GW behaviour above NZ and traced horizontally refracted GW signals in IFS data. However, in order to better focus the introduction to the considered SSW case, we quote this paper without further details in the introduction as an example for horizontal propagation.

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?

We point out the impact of our analysis at the end of the most paragraphs in the introduction, now.

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

We did that.

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).

This was rewritten, taken care of this comments.

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.

Again, by rewriting we hope to clear up this part.

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.*

The novelty of our method was already mentioned above and is pointed out much clearer in the manuscript now.

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.

These issues are now included in the Introduction and Methodology section.

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)???

Most of the issues stated are removed from the text. The data is explained in detail. Abbreviations are introduced correctly.

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.

We extended the section by a step-by-step explanation of the method.

Section 2.1:
about the name "UWaDi", see above

Discussed above.

line 26: give the range of x-values

Done

lines 25/27: the Hilbert transform does not "provide a new complex series" – the complex values are determined by Eq. (1) by means of the Hilbert transformation

Changed.

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.

As mentioned above, we provide a step-by-step outline, now. As mentioned in the manuscript, the authors may provide the code to interested readers if this is wanted. Again, the agreement to Zimin et al. (2003) is solely restricted to the mathematical background, namely the Hilbert transform.

are the quantities calculated by Eq. (1) and (4) the same?

Yes, they are. However, Eq. (1) shows the idea of the Hilbert transform. Eq. (4) belongs to the stepwise implementation of the Hilbert transform.

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?

This is rewritten for better understanding.

line 23: "First" instead of "Fist"

Changed

Generally, the reference to wave packets and the identification of them is missing!!

We provide the synthetic test case as a simple application of the method. There we indentify wave packets. (Sec.2 to 2.2)

What is the physical meaning of the phase (Eq. 6) with respect to the wave groups?

The real and (Hilbert-derived) imaginary part of the function are used to change to an amplitude-phase representation. While the amplitude takes the maximum elongation of oscillations, the phase describes the changes in between. How often the phase is changing, this is proportional to the frequency (in time) or wave number (in space). Respective differentiation brings it up. When the wave group consists of many freuquencies, our estimate returns the amplitude-weighted mean of all (see appendix A).

In Eqs (8) and (9) indices "d" are used. Later, "d" is used as abbreviation for the vector of Cartesian coordinates.

This is changed.

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

We provide this in the step-by-step manual, now.

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

First advantages are mentioned in the Introduction, now. Further we added a Section where we validate our methods with other methods. This clearly showed the locally precise estimation of amplitude and wave number.

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.

We discussed this above. Only a synthetic test case with a-priori known “truth” can be used to validate a method for itself and to conduct a qualified comparison to other methods.

Section 2.3:
PAGE 6
line 18: “ca.” ???

Removed

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 .

The authors took the explanation of the sponge layers from ECMWF (2016). We shortened the discussion on the sponge layer issue massively. We rather focussed on vertical propagation issues of well-resolved GWs in the troposphere and middle stratosphere during a SSW event. Hence, we decided to not add a discussion on GW parameterization and damping but to quote the Jablonowski and Williamson paper in the introduction.

Orr et al. (2010) discuss the improvements in ECMWF data by changing from Rayleigh Friction to the Scinocca Scheme. Nevertheless, a sponge-specific discussion which would support our statements on resolved GWs in the new manuscript is lacking. Therefore, we dit not include this in our list of references.

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.

The issues have been discussed in-house before. Our findings found agreements, in general, incuding the intercomparison of unpublished data material. We restrict our method application to a region without massive damping up to the mid-stratosphere and therefore follow the advice of Referee #1 and e.g. Yamashita et al. (2010).

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 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?

Regarding this concerns, we changed the data preprocessing as described above in Sec. 1. The horizontal divergence is directly taken from ECMWF. Bandpass filter is needed to restrict the analysis on wavelengths that we are intersted in, e.g. intertia gravity waves. Clarification on the sampled GW spectrum can be found in Sec. 2.3 in the new manuscript.

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.

We made it more clearer, now.

Can you give a reference to the statement in line 21?

The relation between amplitude and standard deviation is general for harmonic functions as can be verified with a sine. We added an explanation to Appendix A to show that.

q. (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?

A derivation of our formulae can be found in Appendix B.

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!

With this items we want to point out the difference between wave energy and wave action and why we prefere the wave action. A discussion on wave action, especially in varying background winds is part of the discussion, Sec. 4.

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 win-

ter 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 159, 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.

We have included the named publications in our Introduction.

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.

We seperated Results and Discussion. The comparison with observations from Lindenberg and Kühlungsborn are removed. It was not our aim to sound foolish, so we removed this part, too.

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

This Figure was erased.

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.

Not relevant any more.

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?

Not relevant any more.

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 (60N latitude band, some place near Greenland) and consider the conditions there without taking into account 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.

In the new manuscript we point out the restrictions on vertical propagation only. We compared our local findings to spatial averages over similiar background wind conditions and found no striking deviations. Therefore, we concentrate on local wave propagation, as it is an advantage of our technique to obtain local wave quantities. Furthermore, we highlight the position of our local GWs.

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!!

The way how we obtain equidistant height levels from the model levels is described in Sec. 2.1, Step 1. As we use new data now, the polarstereographic maps are redone.

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.

This is reformulated.

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.*

The authors are aware of the difficult definition of the edge of the polar vortex. Clearly, this goes beyond the scope of the paper. What is meant is that the bright reader should be capable of combining the wind field (Fig. 2a) with the polar vortex and then sees from the horizontal divergence (Fig. 2b) that anomalies tend to come up at the places where the ring vortex is sharply deformed.

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.

Changed.

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.

Done.

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.

Done

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

Done.

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?

Changed.

I cannot follow the argument, why a 1D model is sufficient. You only consider conditions at 60N! 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.

The vertical column modell for GWs is well approved. We are aware that horizontal alignment to strong winds or horizontal propagation play a role but this did not play a leading role by comparing our local profiles to spatial averaged profiles, see discussions above. We now show GWs not only arise from sources from the troposphere, but also from stratospheric jets. We also demonstrate that UWaDi may detect locally very different GW activities in different wind conditions.

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.

Taken care of.

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?

This Figure was removed.

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 60N 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!*

In the new manuscript we study three different locations at one time step, showing and discussing wind and divergence together with GW parameters. Insofar, we take the relative position with respect to the polar vortex into account. In the Introduction and discussion we mention several more recent publications and their restrictions due to the necessity of spatial or temporal averaging (Yamashita et al., 2010, 2013; Limpasuvan et al., 2011; Ern et al., 2016). We do not claim, that we find results heavily differing from Whiteway (1997) but with this publication we want wo point out the advantages of our method, beneath others we provide snapshots of vertical profiles of local GW propagation without the necessity of e.g. temporal averaging, which was done in Whiteway (1997). We can give local GW properties in faster changing background winds. To keep this manuscript clear, we restricted the list of references to the already listed publications above which support the messsage of our manuscript equally.

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 (50W, 60N) 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??

The issue of critical level filtering is a good case to show the advantages of the method. Only with a precise estimate for any height the critical level can be identified. Other box-like methods smear out the results, as shown in the test case.

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.

We removed the discussion regarding PWs. The improvements regarding Dunkerton and Butchart (1984) are already discussed above.

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

The figure is redone.

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

The figure is removed.

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.

The figure is removed.

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.

The figures are changed according to these comments.

References

Dörnbrack, A., Gisinger, S., Pitts, M. C., Poole, L. R., and Maturilli, M. (2017). Multilevel cloud structures over svalbard. *Monthly Weather Review*, 145(4):1149–1159.

Dörnbrack, A., Pitts, M. C., Poole, L. R., Orsolini, Y. J., Nishii, K., and Nakamura, H. (2012). The 2009-2010 arctic stratospheric winter-general evolution, mountain waves and predictability of an operational weather forecast model. *Atmospheric Chemistry and Physics*, 12(8):3659.

Dunkerton, T. J. and Butchart, N. (1984). Propagation and selective transmission of internal gravity waves in a sudden warming. *Journal of the Atmospheric Sciences*, 41(8):1443–1460.

ECMWF (2016). *Part III: Dynamics and Numerical Procedures.* IFS Documentation. ECMWF.

Ehard, B., Kaifler, B., Dörnbrack, A., Preusse, P., Eckermann, S. D., Bramberger, M., Gisinger, S., Kaifler, N., Liley, B., Wagner, J., et al. (2017). Horizontal propagation of large-amplitude mountain waves into the polar night jet. *Journal of Geophysical Research: Atmospheres*, 122(3):1423–1436.

Ern, M., Thai, Q., John, C., Martin, G., James, M., and Michael, J. (2016). Satellite observations of middle atmosphere gravity wave absolute momentum flux and of its vertical gradient during recent stratospheric warmings. *Atmos. Chem. Phys*, 1680:7324.

Fritts, D. C. and Alexander, M. J. (2003). Gravity wave dynamics and effects in the middle atmosphere. *Reviews of Geophysics*, 41(1). 1003.

Khaykin, S., 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. *Geophysical Research Letters*, 42(4):1251–1258.

Limpasuvan, V., Alexander, M. J., Orsolini, Y. J., Wu, D. L., Xue, M., Richter, J. H., and Yamashita, C. (2011). Mesoscale simulations of gravity waves during the 2008–2009 major stratospheric sudden warming. *Journal of Geophysical Research: Atmospheres*, 116(D17).

Orr, A., Bechtold, P., Scinocca, J., Ern, M., and Janiskova, M. (2010). Improved middle atmosphere climate and forecasts in the ecmwf model through a nonorographic gravity wave drag parameterization. *Journal of Climate*, 23(22):5905–5926.

Plougonven, R., Teitelbaum, H., and Zeitlin, V. (2003). Inertia gravity wave generation by the tropospheric midlatitude jet as given by the fronts and atlantic storm-track experiment radio soundings. *Journal of Geophysical Research: Atmospheres*, 108(D21). 4686.

Yamashita, C., England, S. L., Immel, T. J., and Chang, L. C. (2013). Gravity wave variations during elevated stratopause events using saber observations. *Journal of Geophysical Research: Atmospheres*, 118(11):5287–5303.

Yamashita, C., Liu, H.-L., and Chu, X. (2010). Gravity wave variations during the 2009 stratospheric sudden warming as revealed by ecmwf-t799 and observations. *Geophysical Research Letters*, 37(22). L22806.

Zhao, J., Chu, X., Chen, C., Lu, X., Fong, W., Yu, Z., Michael Jones, R., Roberts, B. R., and Dörnbrack, A. (2017). Lidar observations of stratospheric gravity waves from 2011 to 2015 at mcmurdo (77.84 s, 166.69 e), antarctica: 1. vertical wavelengths, periods, and frequency and vertical wave number spectra. *Journal of Geophysical Research: Atmospheres*, 122(10):5041–5062.

Zimin, A. V., Szunyogh, I., Patil, D., Hunt, B. R., and Ott, E. (2003). Extracting envelopes of Rossby wave packets. *Monthly Weather Review*, 131(5):1011–1017.

Zülicke, C. and Peters, D. (2006). Simulation of inertia-gravity waves in a poleward-breaking Rossby wave. *Journal of the Atmospheric Sciences*, 63(12):3253–3276.