Review

Diagnosis of Local Gravity Wave Properties during a Sudden Stratospheric Warming

by

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The paper's intention is to diagnose local properties of gravity waves (essentially, amplitude and wavenumber vector) from 3D gridded data by a tool named "UWaDi". The paper essentially consists of two portions. One part is the presentation of the wave diagnosis tool and the second part describes an application to 3D IFS analyses for one selected day in January 2016. Unfortunately, both parts are half-baked. And both parts would gain immensely if they were written such as they were individual scientific contributions to this journal.

Part 1: The presentation of a new software tool (line 13, page 1: "Here, we want to introduce a new method named ”Unified Wave Diagnosis“ (UWaDi).") which is coded with open source software should be happily resolved by offering it also as open source code. Currently, the standard is to publish the open source software under a certain license and to allow access to the code via a repository. Requests to the authors (line 24, page 15) are simply not up-to-date as authors might leave institutions, for example. There are other remarks concerning the outline and scope of the "UWaDi"-tool which are listed below.

Part 2: At some places in the text, the authors try to state their scientific goals which shall be tackled with the "UWaDI"-tool: line 1, page 1 ("The selective transmission of gravity waves through an inhomogeneous mean flow is investigated.", line 7, page 3: "We are interested in the longitude-dependent transmission of GWs during a SSW."). This is a relevant topic; however, the results presented shed only some spotlight on the whole dynamical processes during the selected period. Only one selected time is considered and both the temporal process of propagating waves (transmission) and the specification of the wave sources remain rather speculative. As above: the paper would greatly benefit if this part is separated from the paper and investigated in an own, full-length scientific contribution.

Specific Remarks

Abstract:

- it should be mentioned right at the beginning that the method is only applicable for gridded 3D data

- line 3: "wave properties": they should be listed here for completeness

- line 3: "wave-containing data" should be specified; later, this turns out to be a crucial point of the analysis where a preselection of wave modes is made by the choice of the horizontal divergence as "wave containing data"; furthermore, the retrieval of this field in a specified spectral resolution certainly impacts the results which is not discussed in the paper at all

- line 7: scientific result: "confirm locally different transmission"; rather weak statement: as no propagation is investigated but only still images are presented you can only refer to "appearance" of gravity waves under the given background conditions (see your own statement in line 14, page 3)

- line 8: I thought, the local wavenumbers are the output of the tool; why "additionally ..."?

- line 9: very speculative statement

Introduction:

- it is certainly an advantage to discuss and compare the different methods for the retrieval of wave properties; there are two points which should be added. First: make clear that "UWaDi" needs regularly gridded data from the very first beginning to point out the difference to other methods. Second, I miss the state-of-the-art review of the application of Hilbert transforms to atmospheric data in 3D. For example, the sentences in the paragraph about the Hilbert transform (page 2, 3rd para) can lead to a misunderstanding: "Kinoshita and Sato (2013) provide a three-dimensional application on Rossby and GWs. Our method comes up with an enhancement for three dimensions and the additionally provision of the wave number in every dimension which was not presented before." as they give the impression that K&S2013 use the Hilbert transform. Unfortunately, the word "Hilbert transform" does not appear in that paper. Maybe, I didn't read the paper carefully enough but I suggest to check this statement and to add appropriate applications of Hilbert transforms in atmospheric 3D data discussing their advantages and disadvantages (Glatt and Wirth, you know these papers).

- the scientific focus on the characterization of gravity waves appearing in high-resolution IFS data is good; the authors have all means at hand to provide a substantial contribution; however, this would be a full paper and not only an addition to the presentation of "UWaDI". See part 2 above!

- line 16, page 3: the full set of output of "UWaDi" is unclear to me: Is " ...we will use “UWaDi” with a GW-specific diagnostic." an addition or the standard set?

- the intention to study a period which is " ... very well sampled with observations of GW properties" (line 29, page 3) is very good; unfortunately, the authors do not use them; furthermore, they use a quantity to identify waves which is hardly measurable ("Our approach concentrates on fields of horizontal divergence of ECMWF IFS data", line 2, page 4); for atmospheric physicists, temperature fluctuations would be a much better choice!

- line 32/33, page 3: I would recommend to avoid such strong statements as "Even the T799 resolution gives proof of correct GW appearance in the stratosphere."

lines 5 -9, page 4: As mentioned above, I have problems to see a discussion about propagation (vertical or horizontal) when you only provide one time snapshots. This must be speculative.

Method and Data

(a) The description of the first step (page 4 and 5) is totally incomprehensible:

"Horizontally, the grids are equidistant if they are provided on a regular latitude-longitude grid." I don't think, this statement can be true if distance is measured in meters. Please, provide the correct formulas how you compute the distances for the regular lat-lon grid. This should be specified in a step-by-step outline of the method!

"Vertical interpolation from model levels to equidistant height levels is performed by associating constant heights with pressure levels."

I don’t understand this. First of all: are you using IFS data on pressure or model levels? How do you determine the height on these levels? And how do you interpolate the data on an equidistant altitude grid?

"Consider to first separate the fluctuations from the background with appropriate numerical or dynamical filters."

I have no idea what the sentence means and what you are referring to.

(b) You outline the method for 1D fields. Could you specify what f_x means?! Is this f(x,y_f,z_f,t_f) ?? Subscripts "_f" mean at fixed positions or time?

(c) Is Eq (10) correct? delta denotes horizontal divergence, right?! Shouldn't be the "delta" in the formulae a "sigma" as f_x is the divergence in your applications, correct??

(d) Figure 1: Why do you plot negative amplitudes when Eq. (11) takes the positive square-root? What are the thin black lines? The choice of colors could be changed to improve readability.

(e) The subsection about the IFS data must be improved. Please, provide the following information and avoid discussions about other cycles here:

- IFS cycle 41r1 is used; (if you really want to refer to differences of 41r1 to other cycles, see Ehard et al, 2018: Comparing ECMWF high resolution analyses to lidar temperature measurements in the middle atmosphere. Q.J.R. Meteorol. Soc. doi:10.1002/qj.3206;)

For which spectral resolution you retrieve the data from the archive?

Maybe, I have to mention that the retrieval of data goes along in a two-step procedure. You can retrieve data using the full set of spectral coefficients (in MARS retrieval RESOL=AV) or you can specify a zonal wavenumber (e.g., RESOL=309) you want. If you do not specify anything, default values are chosen, see: https://software.ecmwf.int/wiki). After this first step, the data are interpolated on a given grid (specification of the GRID parameter in the retrieval).

- on which regular lat/lon grid are the data interpolated to?

- do you use data on pressure or model levels?

- how do you compute the altitude on these levels?

(f) Section 2.4 and Appendix B

Equation (14) cannot be derived by the relations provided in appendix B.

Appendix B: The Appendix is about the "Derivation of the TOTAL wave energy" not only about the kinetic one. The presented equations cannot lead to the final result (B4). There is a mistake (I think, omega is missing) in the provided formula for vorticity tendency (line 23, page 16). Actually, zeta is the default symbol for relative vorticity not xi. Referring to two text books (Vallis, Eq 4.69, Gill, Eq. 7.10.7), the formula for the absolute vorticity in a rotating frame of reference is D(zeta+f)/Dt = -(f+zeta)*delta. Why do you obviously use d zeta/dt=-f*delta only? Discuss this approximation! Furthermore, I obtain different signs in (B3) when I use the provided relations between vorticity and divergence and between divergence and vertical velocity. Please, check!!

line 8, page 9: What is Omega?

Results

- for which time the analysis is performed?

- line 14, page 9: Change formulation "The jet streak above northern Europe is decelerating" if you just refer to the wind inside the jet. If you refer to the propagation of the jet streak itself and its deceleration, you should provide evidence.

- line 16: "Equal patterns appear above eastern Siberia .." I cannot see EQUAL patterns.

- Fig 2b is not mentioned in the text, but reference to it fits in line 16.

- The details in Figure 2 are hardly visible. Maybe, different line increments might increase the readability.

- line 20: If it is "more convenient in terms of wavelengths" you should plot them instead of wavenumbers in the respective Figures 3 and 5

- line 21: I found an upper limit at 196 km (2 pi/3.2 10^{-5} m^{-1}) not 165 km.

- line 20 and line 21: the horizontal wavenumber changes by about 66%, the vertical only by about 45% over the height region. Thus, the statement: "In the zonal mean the horizontal wave number remains nearly constant with increasing altitude" cannot be supported by the data provided in Fig. 3.

- very speculative and not provided by evidence: "independent from the overall synoptic situation and is therefore expected to be an artefact of artificial wave damping from the IFS sponge layer." (line 24, page 9)

- I do not get the meaning of "We did not find significant differences between spatial averaging over areas of some longitudes extension and the local profiles (not shown)." What mean areas? Do you also average zonally?

- line 29, page 9: "The low-pass filter applied in Step 8 helps to overcome massive grid-point to grid-point fluctuations." The documentation of this step could be a nice addition to a more substantial documentation of Part 1 (above).

- line 1, page 10: do you really mean "descented"?

- line 9, page 10: "The not trustworthy areas are excluded." See above for Part 1.

- line 12, page 12: "Altogether, GW emissions seems to take place in the .." and line 16, same page "..Mountains, hence, mountain waves are most likely." are very speculative and not fully supported by the data provided. This issue would belong to the Part 2 of the a possible separated paper.

Discussion

first paragraph: "These findings were obtained with the box-based S-3D algorithm. We add some spatially more refined analysis with UWaDi." What about to really apply the other methods used for the comparision with the synthetic data to the real case considered here? This would add substance!

line 27, page 12: " ... and agree with findings of Limpasuvan et al. (2011)." and line 29: " ... This is close to the findings of Krisch et al. (2017), who .." If you really intent a quantitative comparision, I would recommend to avoid statements like those. The Limpasuvan case lacks comparability in the background wind field, I guess. And, it is not clear what exactly you compare and refer to. The Krisch case is even more dangerous as they point at the dominant horizontal propagation which is omitted here.

line 30, page 12: there is no evidence of data to provide a proof of the statement " ... from the 25 January to 30 January the overall approaching flow direction did not change above northern Europe and comparable GW characteristics can be expected."

line 4, page 14: again, the hypothesis of a wave source at the stratospheric jet remains hypothetical unless more facts of evidence are provided. The geographical association of large wave action with the nose of the jet does not mean undoubtedly that the jet is the source.

line 15..., page 14: the identification of a critical level by detecting a local maximum in k_z is a possible choice which alone is not sufficient to proof the critical level absorption. As you have all tools at hand, why you do not derive the group and phase velocities of wave packets and identify their sources? This would give much stronger evidence.

line 19/20, page 14: I don't understand "The near-inertial GWs are not subject of absorption." Why?? There exists so-called Jones critical levels.

Summary and Conclusions

I would have expected here NEW insights into the scientific topics which were mentioned in the Introduction and in the Abstract: "The selective transmission of gravity waves through an inhomogeneous mean flow is investigated." One finds a summary of "UWaDI" and potential future developments "UWaDi may also provide local estimates for more complex tools such as the combined Rossby wave and gravity wave diagnostics of Kinoshita and Sato (2013)." (line 8, page 16). Scientific conclusions are essentially absent. Again, it must be stressed that the results of the paper do not allow any conclusions with respect to propagation as only one time is considered and very simplifying assumptions are made.

Typos:

inhomogenous -> inhomogeneous (line 1 in Abstract)

artifical -> artificial (page 9, line 24)

“. wave action ..” -> “. Wave action …” (page 8, line 28)

imhogeneous -> inhomogeneous (page 8, line 27)