Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-29-AC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Retrieving characteristics of IGW parameters with least uncertainties using hodograph method" *by* Gopa Dutta et al.

Gopa Dutta et al.

gopadutta@yahoo.com

Received and published: 13 July 2017

Major comments:

Q1. The structure of the manuscript To the best of my understanding, there are four major groups of experiments in this manuscript. They can be listed as below. EXP A: 1) Detrend u and v from the time series; 2) Use a third order Butterworth filter with a band-pass between 36 and 44 h, which is a time-wise filter; 3) Use another bandpass filter between 1.5 and 4 km, which is a height-wise filter. EXP B: The same as EXP A, except that Butterworth filter is replaced by a sixth order FIR1. EXP C: 1) Obtain the fluctuation profiles by removing polynomial of different orders for each individual profiles. 2) Use

a third order Butterworth filter between 1.5 and 4 km, which is a height-wise filter. EXP D: The same as EXP C, except that Butterworth filter is replaced by FIR1. Here, by assuming that the IGW characteristics are relatively stationary within 120 h, EXPs A&B could be considered as the reference for EXPs C&D. Also, in reality, EXPs A&B may not be possible due to the requirement of the continuous high-resolution observations in time. In contrast, EXPs C&D are easier to achieve since they only require individual profiles. The above classification and clarification are summarized by me, and I hope that they are correct. In the current manuscript, it is very hard for the readers to follow the manuscript due to its structure and the lack of the necessary clarification. I would suggest that the methodology part and the list of experiments should be introduced in details in a separate section before the results are shown.

A1. We have taken this valuable comment seriously and attempted to change the matter appropriately, though we have not written the methodology separately. We felt that the results in that case will become somewhat confusing. We request the reviewer to go through this revised portion now and to find out whether clarity is enough.

Q2. The clarification of the details in the methodology Some of the details in the methodology should be clarified and given. Note that the other reviewer also gave similar comments on an earlier version, but I think that there is still room for improvement. Please check my below comments.

2.1 On the method of the filter: In addition to Figure 3, the authors should try to present a brief introduction on Butterworth filter and FIR1 filter. Please give the reference on the mathematical calculation of those two filters. Also, what is the meaning of the "order" for each filter? Why is the third order selected for the Butterworth filter? Why is the sixth order selected for the FIR1 filter? Are the results sensitive to the selection of the order?

2.1 A. A brief introduction of the filters with corresponding references has been incorporated in the matter as per the suggestion of the reviewer. The order of the filter

refers to the number of components that affect the steepness or shape of the filter's frequency response. As the order of the filter increases, the cut-off become sharper, but the length of the data should be at-least 3 times the filter order. The length of our data is 20 (time-wise). So the maximum order of the filter which we could choose is 6. The filter order is normally judiciously chosen by the investigator depending on the efficacy of the filter. A Butterworth filter of order 3 is more efficient than a 6th order FIR1 filter.

2.2 Line 49: It seems to me that the measurement errors for wind and temperature could be very close to the wave-induced perturbation of wind and temperature. Please clarify it.

2.2 A. The fluctuations are almost of the same order of winds ($\pm$10 ms-1) and temperature ($\pm$15 K) so the error (mentioned in the paper) is much less compared to the fluctuations

2.3 Lines 52-54: How many outliers or how many data gaps are there? The authors could try to give the ratio of the reliable data versus the interpolated data, if necessary.

2.3 A. Normally we adopt the method of visual inspection to remove outliers. But in this data we could hardly find 4 small outliers at 4 heights out of 20 profiles with 600 points (heights) each. Only one flight (4th May, 2012; 11:30 LT) data was missing and hence we had to interpolate one point at each height with time. 2.4 Line 58: In this work, the entire temporal duration is 120 h, and the temporal resolution is 6 h. Therefore, one should be careful about the period under 24 h due to the coarse temporal resolution, and one should also be careful about the period over 60 h due to the assumption of periodic boundary condition. Those similar clarifications should be given. Also, in order to capture a wide range of wave spectrum, it would be nice to have a much higher resolution in time. For example, in Wei et al. (2016, JAS), 1 minute is used as the temporal resolution for the analysis of wave period. This is also worth mentioning.

2.4 A. To avoid this problem, the time series data of 120 h with a gap of 6 h has been

filtered between 36 h and 44 h. So waves under 24 h and above 60 h periods are eliminated. The duration of each radiosonde flight is $\sim 1\frac{1}{2}$ h to $2\frac{1}{2}$ h. It is not possible to fly radiosondes with very high time resolution. Wei et al (2016) is a simulation paper on gravity waves generated by baroclinic instability and it could be possible to take very high time resolution. The work is in the mesosphere which might not be relevant for this work. 2.5 Line 84-86: The temperature perturbation profiles are obtained slightly differently from the wind perturbation profiles. Why? Please clarify it.

2.5 A. The velocity and temperature perturbations are normally obtained differently in different papers. We have calculated velocity perturbations by removing different orders of polynomials and we find that removal of 4, 5 and 6 orders yield almost the same results. Temperature fluctuations have been obtained by removing 4th order polynomial in Hu et al (2002), Allen and Vincent (1995) removed 2nd order polynomial. Chane-Ming et al (2010) removed 2nd and 3rd order polynomial from winds and temperature. No reasons are attributed in any of these papers.

2.6 In the current study, the authors apply a height-wise bandpass filter (between 1.5 and 4 km) in many calculations. In contrast, Zhang et al. (2004, GRL) actually don't have a height-wise filter. This may be due to the different vertical resolution between the observational studies in the current work and the numerical studies in Zhang et al. (2004, GRL). The authors should try to clarify those issues related to the above comparison. Is this height-wise filter necessary? What determines the window of the bandpass filter?

2.6 A. The height filter is necessary when we analyze individual altitude profiles of winds or temperatures. The vertical wavelength of IGW is short and generally between 2 – 3 km. The window of the filter is supposed to be selected judiciously by investigator. We have selected it between 1.5 – 4 km which is commonly taken for IGW studies. Hodographs plotted with only time wise filtered fluctuations did not yield good hodographs showing some superposition of other waves and hence height wise filtering was needed.

2.7 Line 119: I am wondering how to determine the statistical significance with a large number of hodographs? What statistical method is used? What is the minimum sample number required for the significance test? Also, in reality, it may not be possible to have a large number of hodographs.

2.7 A. We have not used any statistical significance tests. We have only calculated the percentage of wave propagation in each direction and the maximum number is shown as the final direction of wave propagation. The percentage is mentioned.

2.8 Table 1&2: The direction of the propagation is a fixed number. It is strange to me, since the other parameters have a certain range. Please clarify it.

2.8 A. Parameters like intrinsic period, horizontal and vertical wavelengths etc are obtained from each of, say, 100 plus hodographs. The values obtained from each hodograph will differ but obviously will be within some range. The maximum and minimum of the ranges have been mentioned. But the direction of propagation can be NE, SE, SW and NW. The maximum number showing a particular direction is mentioned and the percentage is written. The same is normally followed by other researchers as well.

Minor comments:

1. Title: Instead of “IGW”, it is better to use “Inertial-Gravity Wave”.

A. The word “IGW” in the title has been changed to “Inertia Gravity Wave”

2. Line 8: When “IGW” is used for the first time in the abstract, please use its full name.

A. The full name of IGW has been introduced in the abstract as per the suggestion of the reviewer.

3. Line 10: When “FIR1” is used for the first time in the abstract (or in the main text), please use its full name.

A. The full name of FIR has been introduced in the abstract.

4. Figure 1: In the subplots, it is better to use “z=24.55 km”, instead of “24.55 km”. Similarly, please apply it to the other places as well.

A. As per the suggestion of the reviewer we have mentioned “z=24.55 km” in the subplots and also applied to other places.

5. Figure 6: Please double check the figure caption of Figure 6. (b) should be FIR1 filter, and (c) should be Butterworth filter. The related information is not consistent between figure subtitles and figure caption.

A. Thanks, figure has been modified accordingly.

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