

# ***Interactive comment on “Case study of wave breaking with high-resolution turbulence measurements with LITOS and WRF simulations” by Andreas Schneider et al.***

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## **Author response to Review by Wayne K. Hocking**

We thank the reviewer for his detailed review and for alerting us to literature that previously escaped our notice.

In the following, the review is quoted in italics part by part, and our response given below.

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*My first impression is that the paper is trying too hard to justify the idea of “being the first” on a number of fronts. It is not necessary for a paper to always “be the first”, and a good paper can make meaningful contributions even if such status is not valid. In this case, I feel that the paper over-reaches in this area. It claims that, to the knowledge of the authors, “currently the only instrument for the direct in situ observation of turbulent wind fluctuations in the middle stratosphere is the balloon-borne instrument Leibniz Institute Turbulence Observations in the Stratosphere (LITOS)”. Other statement pertaining to the uniqueness of the paper occur elsewhere in the text, where the authors discuss the Richardson number, and the essentially repeat the studies of Hines (1988), who introduced the idea of the “slantwise instability” as far back as 1988.*

We are sorry that our phrasing was mistakable, leading to the impression of claiming to be better than we actually are. As described below, we have rewritten the introduction. The sentence under discussion regarding LITOS has been deleted. The introduction now puts our method and instrument in a better context of existing data sets from airplanes etc. Regarding the discussion about the validity of the Richardson criterion, we have now put it in a “historical” context. In fact we did not claim to be the first here, but wanted to keep the description short as this has already been described in a previous paper from our group (Haack et al, 2014). In the revised version we provide a broader discussion and proper reference of this topic inclusive the slantwise instability described by Hines.

*The first measurements of velocity fluctuations in the stratosphere using balloon-borne instruments was due to Barat (1982), which the authors do refer to later, but fail to give it due recognition. More recently, extensive measurements (including velocity, temperature and humidity) have been presented by Cho et al., (Cho, J.Y.N., Newell, R.E., Anderson, B.E., Barrick, J.D.W., Thornhill, K.L., 2003, Characterizations of tropospheric turbulence and stability layers from aircraft observations. J. Geophys. Res. 108(D20), 8784.*

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<http://dx.doi.org/10.1029/2002JD0082820>) and Cornman ((1) Cornman, L.B., Corrine, S.M., Cuning, G., 1995, *Real-time estimation of atmospheric turbulence severity from in-situ aircraft measurements*, *J. Aircraft* 32, 171–177.; and (2) Cornman, L.B., Meymaris, G., Limber, M., 2004, *An update on the FAA Aviation Weather Research Program's in situ turbulence measurement and report system*. Preprints.In:11th Conference on Aviation, Range, and Aerospace Meteorology, Hyannis, MA, Amer. Meteor. Soc. CD-ROM, P4.3.f.

None of these latter works are referenced. It also should be noted that Dehghan et al., (“Comparisons between multiple in-situ aircraft turbulence measurements and radar in the troposphere”, *J. Atmos. Solar-Terr. Phys.*, 118(A), 64-77, <http://dx.doi.org/10.1016/j.jastp.2013.10.009>, 2014) have found errors in the calibration of the papers by Cornman et al.

It is true that the procedures used in the paper under review are probably the most detailed I have seen, but they are not the only ones. The procedure used by Cornman and colleagues, for example, places accelerometers on commercial aircraft and measures turbulent fluctuations. The results are of course heavily filtered because only scales comparable to and larger than the size of the aircraft are measured, which partly accounts for the corrections introduced by Dehghan et al (2014). However, the sheer magnitude of measurements by this technique is staggering, and vastly outweighs the measurements by LITOS - an important aspect for studies of large-scale diffusion, as will be discussed below. The works by Cho et al. are much more thorough.

Nevertheless, the fact remains that LITOS is not the only instrument used for these studies, nor is it the only instrument which measures velocity fluctuations.

As mentioned above, our phrasing seems to have been mistakable, thus we have revised it. However, we did not claim that LITOS is the only instrument measuring velocities in general, but that LITOS is currently the only instrument for the in-situ measurement of wind fluctuations *in the middle stratosphere*, i. e. above airplane flight altitudes.

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Since Barat's (1982) instrument seems to be no longer in operation, and all other in situ instruments for small-scale wind measurement known to us cannot measure in the middle stratosphere, we still think our statement is correct.

We agree that the database used by Cornman et al. (1995) is much larger, but it is from commercial aircraft flying in the upper troposphere or lowermost stratosphere, not the middle stratosphere. Similarly, Cho et al. (2003) used data from aircraft with a ceiling of 8 km, i. e. tropospheric heights. In the revised introduction, we have cited Cornman et al. (1995) to point out the contrast in available data for the different heights.

*There are a variety of instances when the authors do not give due recognition. The work of Barat, Sidi, Wilson etc., who have spent over 30 years studying stratospheric turbulence - mainly with temperature probes - have not been mentioned in the introduction. (see the references in Osman et al., 2016, discussed below).*

*So these references need to be added.*

We admit that the introduction was overly short in some aspects. We have rephrased it and in this context have added some references, especially technical papers. Besides, we want to focus on wave saturation and breaking, thus we have limited the cited scientific works to those related to these topics.

*However, rather than simply being critical, I would like to offer an alternative approach to the introduction. I first invite the authors to look at the introduction to Osman et al., (2016) viz. Osman, M.K., W. K. Hocking and D. W. Tarasick, "Parameterization of Large-Scale Turbulent Diffusion in the presence of both well-mixed and weakly mixed patchy layers", J. Atmos. Solar-Terr. Phys., 143-144 14-36, 2016.*

[...]

*The importance of this work to the current paper is as follows: earlier works suggested that large-scale stratospheric diffusion rates were determined by the many thousands of small layer of turbulence, whereas the work of Osman et al. suggests that it is the*

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*small number of large layers which dominate the diffusion process. The work presented in the paper under review can help resolve this critical question of how large scale diffusion relates to small-scale turbulent layering, and in so doing will have major impact on the parameterization of large-scale 2D stratospheric models - including WRF. I am not asking that the authors resolve the issue - but simply that they link their measurements to this critical debate.*

*A discussion along the lines given above in the introduction will strengthen the paper enormously and highlight this critical issue of the relation between localized turbulence and large-scale diffusion. Unfortunately, despite the incredible importance of this issue, it is not as widely understood as it should be, and this is an excellent chance to emphasize this issue. Bringing the issues to the forefront can of course also allow justification for more detailed research and hence raise the profile of the issue within granting agencies.*

We agree that the issues of intermittency and turbulent mixing are important. LITOS data are very suitable for such a study. Yet this is outside the scope of this paper, which is about wave breaking and turbulence. We want to address these issues in future work.

[...]

***I therefore ask that the authors substantially revise their introduction, add the references cited, discuss in more detail the work of Sidi, Wilson etc., (see references in Osman et al, 2016) and demonstrate the nature of their work within the context of this important discussion.***

We have revised the introduction, setting our measurements in a better historical context. Furthermore, we have strengthened the description of the geophysical scope.

***Richardson number;*** *First, in several places the authors discuss the relevance of the Richardson number. They conclude that this only applies in purely horizontal flows,*

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whereas gravity waves are 3D e.g. page 6, para 2, lines 4-5 and elsewhere. They then discuss their own resolution of the problem using Achatz (2005). However, this problem was raised and discussed by Hines in 1988 (Hines, C. O., *Generation of Turbulence by Atmospheric Gravity Waves*, *J. Atmos. Sci.*, 45, 1269–1278, 1988) and a citation of his work is very much deserved in this context.

We thank the referee for pointing us to Hines' (1988) work which has inspired many later works. We have added a few sentences mentioning his ideas:

“Already Hines (1988) discussed slantwise static instabilities created by gravity waves. He developed a wave period criterion for turbulence by comparing the e-folding time of the (slantwise) instability with the period of the wave. Turbulence is more likely to occur for slantwise static instability than for vertical static instability.”

**Shedding:** *The authors discuss the idea that wave breaking is not due to single waves, but due to multiple waves adding together, and also the idea that the waves do not break catastrophically, but at times simply throw off just enough energy to allow them to become stable. This is a process called “shedding”, or alternatively “convective adjustment”, which is very well documented in the literature. The authors refer to it as “continuous fractional wave breaking” (page 11, paragraph 3). It is very important that the authors (again) give credit to those who have gone before them. References, and extensive discussion, can be found in Hocking, W.K., A review of Mesosphere–Stratosphere–Troposphere(MST) radar developments and studies, circa 1997–2008, *Journal of Atmospheric and Solar-Terrestrial Physics* (2010), doi:10.1016/j.jastp.2010.12.009, section 8. An even more extended discussion can be found in “Atmospheric Radar: Application and Science of MST Radars in the Earth’s Mesosphere, Stratosphere, Troposphere and weakly Ionized Regions”, Cambridge Press, 2016, chapter 11, section 11.2.12. (Note that Hocking was not the main person who proposed this method, but has summarized the many different papers on the technique- the authors are asked to use these to references simply as a starting point for their own clarification, and to properly cite those who have discussed the method*

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in more detail). Another paper on a similar topic, but “tuned” to the upper troposphere and stratosphere, is Fairall, C. W., A. B. White, and D. W. Thomson, *A stochastic model of gravity-wave-induced clear-air turbulence*, *J. Atmos. Sci.*, 48 , 1771–1790, 1991.

We thank the reviewer for pointing us to the correct terminology. A literature search starting from the articles given above has yielded that “saturation” seems to be the most commonly used term for the phenomenon. Thus we have changed our manuscript accordingly.

**Equation (1) and appendix A:** *Equation (1) and appendix A take an interesting approach to determination of the energy dissipation rate (epsilon). The traditional method for determining epsilon is to determine structure functions, or spectra, and fit relevant Kolmogoroff functions to the inertial range portion. The introduction of the method given in equation (1) began in the early 1990’s, when Luebken (a co-author on this paper), introduced it as an alternative procedure in order to help resolve an argument that developed in the literature when applications of the more traditional approach produced differences of almost an order of magnitude when applied by different authors. For details of the debate, the reader is referred to Hocking, W. K., *The dynamical parameters of turbulence theory as they apply to middle atmosphere studies, Earth, Planets and Space*, 51 , 525-541, 1999, but the argument was resolved by introduction of Luebken’s approach. However, the paper just mentioned (Hocking) then showed that the inner-scale-approach and the traditional approach produced similar values as long as the correct constants were used. The problem arose because a group of workers incorrectly used a constant pertaining to the integrated 3-D energy spectrum, whereas the measurements were made using a probe passing through the turbulence in a straight line, dictating the need for a different constant (the integrated 3-D spectrum and the 1D spectrum both have a  $k^{-5/3}$  spectral form, giving rise to confusion). The constant differed by a factor of 3, so the epsilon thus deduced was in error by  $3^{5/3}$  times, or about 6x. Similar problems have arisen in other areas of the literature, even quite recently. The Appendix of Hocking , *EPS*, 1999 (given above) and appendix A of Hocking*

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*et al.*, (book published by Cambridge Press and discussed above) show how to use the correct constants. Further verification of these constants has been given by aircraft/radar comparisons in Dehghan et al., (2014) (reference given above). Given that the issue of the correct constants is now resolved, there is no reason why the more traditional approach should not be used - and indeed it has been used for many years by a variety of authors who HAVE used the correct constants - it is simply unfortunate that from time to time papers are published by authors who apply the wrong criteria, for reasons outlined above. The use of equation (1) in the paper under review is OK, but places significant constraints on the analysis. As the author have shown, many spectra cannot be used since they do not show a “knee” in the spectrum. It would be of interest to see how the approach using  $l_0$  and the more traditional structure function/spectral approach (with correct constants) compare. The issue is important in view of the first item discussed in this review concerning the relation between small-scale and large-scale diffusion rates. The ability to measure epsilon using only the inertial part of the spectrum allows access to a larger data set, and the most important parameters could be argued to be the frequency of occurrence of turbulent layers, and their fraction of occupancy, while the actual strength of the turbulence within the layers might be less important for determination of large-scale diffusion. Hence the availability of more useable data allows a better contribution to studies of these fractions and statistics (see P 4, In 4 - seems the study presented here is not ideal for determining percentages). If there is insufficient room to discuss it here, it at least seems good topic for future study, and I recommend it to the authors. If the author have already done such a study, they should cite it.

For our measurement the “traditional” method to fit the inertial range of the spectrum is not possible, because that method crucially depends on the absolute value of the periodogram, which is not available due to missing calibration. A calibration to infer wind velocities from the anemometer voltage of the constant temperature anemometer would be difficult because it has to be performed in a laboratory for known velocities under the same ambient conditions for pressure and temperature as the measurement.

Conditions of a balloon flight, where pressure varies within several orders of magnitude and temperature changes by  $\sim 80$  K, are very difficult to simulate in a wind tunnel. We do not know a facility where such a calibration would be possible.

We agree that a comparison of dissipation rates from both retrievals, i. e. the traditional inertial range method and Lübken's method, would be very interesting. We have planned to do such a comparison for a measurement on the ground where the calibration problem can be solved with relative ease.

***Use of WRF:** The authors include substantial discussions of the wave-field inferred from the WRF model. I do find myself wondering about the validity of this approach. Are modern models really good enough to reproduce the detailed small-scale structure in real-life situations? It seems somewhat unlikely to me, but perhaps I have not kept pace with current computer developments. But treating the model output as a true representation of the wind and temperature field seems a stretch. Even if the waves are generated reasonably accurately in amplitude, variations of phase estimates can significantly impact the likelihood that they break (as per the author's on comments about "continuous fractional wave breaking" and also the concepts presented by Fairall et al. discussed above). I feel the approach is interesting, but am concerned it is a bit premature. I am happy to see the process introduced, **but would ask for more commentary about its likely validity.***

We agree that WRF is an idealised representation and does not reproduce reality in a perfect way. In our paper it is used to get an overview of the respective meteorological situation during the LITOS flights and to demonstrate that gravity waves occurred in the vicinity of the flight tracks. Our interpretation of the model results is not based on the small-scale structures, but on the general dynamics. Obviously, in some cases (e. g. the BEXUS 12 flight) small-scale dynamics in WRF is at least qualitatively correct and produces turbulent layers that were also found prominent in our observations. There was a good agreement between observed increase in dissipation rates and intensified TKE in the model. On the other hand, we intentionally do not investigate and interpret

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the many cases where LITOS observes turbulence and WRF not. All our statements derived from WRF are based on well-resolved events.

We agree that a general validation of model results was missing and have added plots of winds and temperatures from WRF interpolated along the trajectory to the plots of the radiosonde measurements. These compare very well, the only difference is that the radiosonde data contain signatures from small-scale gravity waves which WRF cannot resolve.

Part B – grammatical

We thank the referee for the detailed grammatical corrections. We appreciate the effort.

*P 1, abstract, line 9 - Particularly → In particular*

Changed.

*P 1, In 17 - “This typically happens in the mesosphere”. What typically happen there? Are the authors talking about catastrophic wave breakdown, or shedding? As discussed in part A, wave breaking is expected in some form everywhere, either by full breakdown or multiple-wave interference effects, so I am not sure this sentence is especially useful.*

We have removed this sentence in our revised version of the introduction. Instead, we have written: “This mechanism has been suggested by Hodges (1967) to explain turbulence in the mesosphere.”

*P 2, l1 - Measurements are → Measurements have been??*

This sentence has been removed in the revision process.

*P 2, In 19 - use of “thereof” seems odd - suggest replacing with “comprising”.*

Changed.

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*P 3, In 1 - “booms sticking out” → “booms protruding”*

Changed.

*P 3, In 8 - suggest “windows of 5m” → “windows with depths of 5m”??*

Changed.

*P 3, In 25 - rejection of spectra which are “not meaningful” - seems presumptive to assume that the only acceptable spectra are ones consistent with their proposed theory, though its understandable that no useful epsilon can be achieved in such circumstances I guess.*

We state that if the bend in the spectrum is not resolved, the *fit* is not meaningful (not the spectrum). That means no  $\varepsilon$  can be retrieved using Heisenberg’s model. We have added a phrase to clarify that:

“This means that the bend in the spectrum is not within the fit range and thus the fit is not meaningful, allowing no useful retrieval of  $\varepsilon$ .”

Generally, we only consider spectra that follow the turbulence model, which may exclude turbulence that is not fully developed. The criteria sort out cases where  $\varepsilon$  cannot be retrieved. In our manuscript we have added two sentences discussing this issue:

“Requiring the spectrum to follow Heisenberg’s turbulence model may exclude turbulence that is not fully developed. However, it is not feasible to retrieve  $\varepsilon$  in cases where the periodogram does not follow the turbulence model.”

*P 4, In 3 - suggest “conditions” → “above conditions”*

Changed.

*P 4, In 4 - suggest “rigorous criteria” → “rigorous criteria applied”*

Changed.

*P 4, In 8 - “sensor has been ...” → “sensor has been located ...”*

Changed.

*P 4, In 26 - reference to Hines' work on slantwise instability is needed.*

Done, see response above under Part A.

*P 5, In 28 - the 30% does not seem meaningful due to the selection criteria used.*

We have rephrased the sentence to make clear the 30 % is according to the criteria presented in Section 2.1:

“Overall, ~30 % of the atmosphere was turbulent according to the criteria presented in Section 2.1.”

*P 6, In 2 - “with respective phase velocities” - with respect to what? the meaning of the sentence is quite unclear.*

We have rephrased the sentence as “caused filtering of gravity waves with phase velocities equal to the background winds (if present).”

*P 6, In 6 - “other side” - do you mean “other hand”?*

Yes, changed.

*P 6, In 19 - “It visualizes..” → “It demonstrates...” ??*

Changed.

*P 8, In 1 - could change “..at 27 Mar 2014 10:10 UT.” to “..on 27 Mar 2014 at 10:10 UT.”*

Changed, also for the other flights.

*P 8 In 7... “.. were easterly and turned northwards ...” This is a confusing mixture of directional conventions use either “.. were westward and turned northwards..” or “were easterly and turned southerly.” Similar problems exist elsewhere in the text - try to standardize directional information (meteorological directions end in “ly” and indicate the direction from which the wind comes. whereas middle-atmosphere convention*

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*more commonly ends directions in “..ward” and indicate the direction in which the wind is blowing towards. Whichever convention is used is fine, but please try to standardize.*

This was an error. Northwards has been corrected to northerly.

*P 8 In 31 - “and partly even smaller...” - do you mean “and at times smaller...” ??*

Yes, changed.

*P 9, In 12 - mention is made of a “layer(ed) structure” - since it is a 1D vertical profile, how ca you be sure it is really layered?*

We cannot be sure about the horizontal extension of the layers. Our use of the term “layer” stemmed from the general belief that turbulence occurs in pancake-shaped layers of a few 10 m vertical and several km horizontal extent, which is supported by radar and aircraft measurements. To avoid misunderstandings, we have changed our phrasing to “patchy structure”.

*P 9, In 26 - maybe change “yields” to “suggests” ?*

Changed.

*P 10, paragraph 2 - while a useful summary of these data, there were only 4 flights, and these results can really only be considered as anecdotal.*

A phrase was added to clarify that averages over altitude for single flights are meant.

*P 10, last paragraph. Some of these references could be cited in the introduction.*

All of these references except Wilson et al. (2014) are already cited in the introduction.

*P 11, lines 21-22 - link to pre-existing papers regarding shedding and convective adjustment rather than introducing yet another name.*

As mentioned above, we have changed the terminology to wave saturation, and have cited papers discussing it.

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