

Referee's report on the paper

"Case study of wave-breaking with high-resolution turbulence measurements with LITOS and WRF simulations"

by W.K. Hocking

#### **Part A - scientific issues.**

This paper presents some high resolution measurements of turbulence using a special balloon-mounted wind anemometer. The paper concentrates on 4 campaigns, and uses WRF simulations as backup.

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 "slant-wise instability" as far back as 1988.

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. <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

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.

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.

I will summarize this work below. This summary can be used to place the work presented by Schneider et al. in a far more useful context. The current authors have simply justified their paper on the need for measurement of turbulence in the stratosphere. But in fact there are much larger and more important issues at play here which are of great physical significance, unrecognized by a large portion of the community but of great relevance.

The issue is the following. In 1981, Dewan (1981) studied the effects of small layers of fully developed turbulence, separated by regions of laminar flow, on large scale diffusion in the upper troposphere and stratosphere. This work was referenced several times following this (e.g. Hocking, W. K., The effects of middle atmosphere turbulence on coupling between atmospheric regions, *J. Geomag. Geoelectr.*, 43, Suppl., 621-636, 1991; Hocking, W. K., The dynamical parameters of turbulence theory as they apply to middle atmosphere studies, *Earth, Planets and Space*, 51, 525-541, 1999).

In the 1990's, Haynes and co-authors presented a series of papers following a similar theme; citations of these papers can be found in Vanneste, J., Small-scale mixing, large-scale advection, and stratospheric tracer distributions, *J. Atmos. Sci.*, 61, 2749-2761, 2004.

The basic premise was that thin layers of turbulence, separated by essentially laminar regions of flow, are the primary form of turbulence in the stratosphere, and that in determining the large-scale diffusion coefficient (scales of 10 km and more) the effect of these isolated layers is

paramount. Equally importantly, the rate of large scale diffusion is independent of the strength of turbulence within the layers, (as long as mixing is complete), and it is other things like the frequency of occurrence of these layers, their mean depth, and their relative fraction of occurrence, which defines the large-scale diffusion process. This was a major departure from established thinking.

However, issues remained. One such issue was a proper definition of the meaning of "well-mixed". Others were the 1-D nature of the models used, and the impact of partially mixed layers. Vanneste (2004 - see above) attempted inclusion of the impact of partially mixed layers.

Osman et al. (2016) (referenced above) expanded the previous work to a 2D model, developed a proper definition of the meaning of "well-mixed", and went on to study the impact of both small and large layers on the gross 2-D flow. (Another important process, namely that of Stokes Diffusion, was also discussed, though I will not dwell on this here).

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 layers of turbulence, whereas the work of Osman et al. suggests that it is the 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.

Further detailed discussion of these issues can be found in *Hocking et al., "Atmospheric Radar: Application and Science of MST Radars in the Earth's Mesosphere, Stratosphere, Troposphere and weakly Ionized Regions", Cambridge Press, 2016 (see the discussion around Fig. 11.23).*

**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.**

Four other scientific issues should be considered.

**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, 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.

**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 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.

**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 (i) 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 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  $I_0$  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, ln 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.

**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.**

## **Part B - grammatical.**

A variety of grammatical errors occur throughout the text, which are listed below.

P 1, abstract, line 9 - Particularly --> In particular

P 1, ln 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.

P 2, l1 - Measurements are --> Measurements have been??

Section 1 - see earlier notes in pat A - many missing references to other work.

P 2, ln 19 - use of "thereof" seems odd - suggest replacing with "comprising".

P 3, ln 1 - "booms sticking out"--> "booms protruding"

P 3, ln 8 - suggest "windows of 5m" --> "windows with depths of 5m"??

P 3, ln 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.

P 4, ln 3 - suggest "conditions" --> "above conditions"

P 4, ln 4 - suggest "rigorous criteria" --> "rigorous criteria applied"

P 4, ln 8 - "sensor has been ..." --> "sensor has been located ..."

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

P 6, ln 28 - the 30% does not seem meaningful due to the selection criteria used .

P 6, ln 2 - "with respective phase velocities" - with respect to what? the meaning of the sentence is quite unclear.

P 6, ln 6 - "other side" - do you mean "other hand"?

P 6, ln 19 - "It visualizes.." --> "It demonstrates..." ??

P 8, ln 1 - could change "..at 27 Mar 2014 10:10 UT." to "..on 27 Mar 2014 at 10:10 UT."

P 8 ln 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 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.

P 8 ln 31 - "and partly even smaller..." - do you mean "and at times smaller.." ??

P 9, ln 12 - mention is made of a "layer(ed) structure" - since it is a 1D vertical profile, how can you be sure it is really layered?

P 9, ln 26 - maybe change "yields" to "suggests" ??

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.

P 10, last paragraph. Some of these references could be 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.

===== end =====