Reply to Referee 1

This paper combines a nice concise review of the current state of play with measurement of turbulent energy dissipation rates in the atmosphere by in-situ and radar methods. It is backed up by some good experimental data as well, and contains nice side-by-side radar/in-situ comparisons.

I applaud the effort, and will recommend publication - but subject to a few caveats.

We thank the referee for his kind comments and encouragement.

First, there is some lack of clarity in the terms TKE and ∈. The abstract seems to suggest that they are almost the same, but one is a total energy and one is a dissipation rate. But in line 120 and elsewhere they are treated as different entities - maybe I am misinterpreting something, but I think TKE and e need to be more carefully defined.

We do not clearly understand this reviewer comment because TKE (alone) is not defined in the abstract (we introduced its dissipation rate ε only). However, we replaced “TKE dissipation rate” by "dissipation rate of turbulence kinetic energy (TKE)" to eliminate any confusion.

As an introduction to the equations recalled in line 102 for the steady state and in line (120) for the unsteady state, a simplified equation of TKE budget equation is now introduced in the first paragraph of Introduction as follows:

“The dissipation rate ε ($\mathrm{m^2s^{-3}}$ or $\mathrm{mWkg^{-1}}$) of turbulence kinetic energy (TKE) is an important variable for assessing heat deposition by turbulence in the atmosphere. This variable appears in a simplified expression of the ensemble-mean TKE budget equation (see, e.g., Stull (1988) for a complete derivation and for its conditions of validity):

$$\partial TKE/\partial t = P - B - \varepsilon \qquad (1)$$

where P is the shear production term and B the buoyancy flux term.”

A more serious error occurs in lines 136 to 142 - and especially line 142, where it says

"In essence, there is no contribution from an anisotropic buoyancy subrange." The theory of Hocking (1983) does NOT assume this. The factor of 1/2 in Hocking (1983), eq, (2) comes from a crude assumption that the radar receives half of it's v^2_rms from the inertial range and half from the buoyancy range. (In fairness to the readers, this was not fully explained in the original paper.)

I would ask the authors to please look at Hocking et al. (2016) [Atmospheric Radar: Application and Science of MST Radars in the Earth's Mesosphere, Stratosphere, Troposphere, and weakly ionized regions", Cambridge University Press, 2016. ISBN 9781316556115, DOI: https://doi.org/10.1017/9781316556115], pages 407-408.

The more advanced theory presented there allows for a variable contribution to v_rms from BOTH the inertial range and the buoyancy range.

So the theory ascribed to equation (2) of the current paper is not the latest - **in a more complete form, it should include this factor F - see the pages indicated above.**

Indeed it might be possible that this fraction F could be allowed to be dependent on ∈, which could allow better consolidation between the theories presented in the paper under review. Physically, this is not unreasonable - for example, more intense turbulence could more rapidly destroy the stratification, allowing for larger isotropy at larger scales - even into the buoyancy range -- and this would allow a larger contribution to the vertical RMS velocities from the buoyancy range, reducing the fraction F.

Indeed the behaviour of this "buoyancy range" is not fully understood at all - it is clear that turbulence here is anisotropic, with suppressed vertical velocities relative to the horizontal ones, but exactly what this ratio is is quite unclear. It also leads to huge levels of confusion relating to the so-called "buoyancy scale" of Weinstock and the similar but different" Ozmidov scale".

So I ask the authors to at least refer to this later work, and incorporate this fraction F into discussions. They may choose to say that "for a fixed value of F" it does not agree with the eq (1), but it might reconcile better if F varies with ∈, and might allow further insight into why eq. (1) seems to work so well (which is still a bit of a mystery, as discussed by the authors in section 5.1).

And

Page 15 - various limitations of ∈_N are discussed, but remember these all assume a fixed value of F, so it must be made clear that this is the simplest version of this model, not the most complete.

We agree with the above comments of the reviewer. We are very familiar with the seminal treatise on “Atmospheric Radar” by Hocking et al. (2016) and we do reference it. In any case, in response to these comments, we have displaced the sentence *"In essence, there is no contribution from an anisotropic buoyancy subrange"* to a more suitable place in the text which referred to the model for weak or strongly sheared stratification. We also modified the paragraph following Eq. (2) (now Eq. (3)) as follows:

“…where C_N is a constant. This expression is expected to be valid for turbulence in a stable stratification ($N^2 > 0$) whose an outer scale is defined by the buoyancy scale expressed as $L_B = \sqrt{\langle w'^2 \rangle}/N = \sigma/N$. Eq. (3) is thus equivalent to $\varepsilon_N = C_N \sigma^3/L_B$. In a pioneering contribution, Hocking (1983) first derived Eq. (3) from the integration of the transverse 1-D spectrum of vertical velocities over the inertial and buoyancy subranges to relate ε to $\langle w'^2 \rangle$. In its original derivation, the author assumed roughly equal contributions to $\langle w'^2 \rangle$ from the inertial and buoyancy subranges. More recently, Hocking et al (2016) proposed a more general expression by introducing a variable factor F, where F is the ratio of the buoyancy contribution to the inertial subrange contribution. This factor can vary from 0.5 to 1. It affects the value of the constant C_N and Hocking et al. (2016) recommends that a value of (0.5 ± 0.25) be used, which takes into account the variability of F, difficult to determine in practice.”

Indeed (jumping ahead a bit!) I found section (5.1) quite unhelpful in this regard, and I sense that the authors have similar issues, so it might be useful here to consider further the relative roles of vertical velocities in the buoyancy and inertial ranges, which relates in turn to F.

(remembering that most of the issues here arise because the pulse-length and beam widths are right around the buoyancy scale)

And

Section 5.1 - see my earlier comment.

See our reply above.

Another point to bear in mind in regard to section 5.1 relates to Figs. 7.12 and 7.13 of Hocking et al. (2016). While it is easy to believe that the Datahawk is somehow more "perfect" than a radar, as it samples at a single point in space, determination of a spectrum (or alternatively an autocovariance) function requires a finite length of time, and in that time, the datahawk moves, and additionally the mean wind blows different regions of space across the datahawk as well, so the datahawk also has a spatial sampling across many tens of metres, just like the radar. This may also relate to the "70m" scale.

The reviewer is right, and we recognize that considering UAV-derived ε as an "absolute" reference is not fully justified for the advocated reasons: The "70-m" scale may be dependent on the UAV measurement method. For this reason, the empirical model with Lout=70 m must be tested (as well as ε_S and ε_N) independently from UAV measurements, in order to determine the representativeness of this value. This can be made by comparing the results of the models to other data from other instruments (for example, a Doppler lidar) and for other cases of turbulence exhibiting well-developed KH billows (for which Ri values are expected to be much smaller than 0.25). These results will be presented in future papers.

Returning now to section 2, the discussion in section 2.2 is great.

However, I do note some inconsistencies in the text as to the terms Ri and Rf - in places they are written as subscripts (R_f) and sometimes not. Please decide which is best.

We made the necessary changes.

Section 3 is straightforward, though the authors refer to 59-s data sets and 1-min data sets, which I assume are one and the same (??).

Yes, "1-min" is an approximation. The correct value is 59 s.

Section 3.3 -- Ozmidov scales and Thorpe lengths are discussed, but they also relate to Weinstock's "Buoyancy scale" and there is a factor of 10 ($2\pi/0.6$) difference here - may not be relevant in section 3.2 but certainly relevant in a general context.

We did not consider the 2*pi factor in our work as is often the case in fluid mechanics literature.

Line 210 - please more formally define "TKE" and distinguish it from e - maybe it was done earlier(?) - if I missed it, I apologize - but I think the distinction needs to be clear.

See our first response.

The case studies (section 4) seem well documented.

Thank you.

Figs 5 and 6 - note that references to the ∈**_N** formula assumes F=0.5 (see earlier)

It is mentioned in the revised paragraphs

Fig 6 - please add values of R and P, as you have done for Fig. 7

Done. Note that the correlation coefficients are high due to self-correlation (but variables contain σ^2).

Line 392 - Terns like "cst" appear - I am not clear what cst means!? I could not find a definition. Please clarify.

“cst” means “constant”. This abbreviation is not used in English. Sorry for the misunderstanding.

Conclusion - great paper, nice review, nice data, but please recognize that the latest version of the model for Eq. (2) has NOT been used, and the authors have assumed a fixed fraction F (relative contributions of inertial and buoyancy scales). If the authors continue using the current discussions, they are obliged to recognize verbally that they have assumed a simplified model with fixed F, which may not be realistic. Using the 1983 version of the theory pertaining to Eq (2) and not considering the 2016 version is a bit unfair.

The corrections have been made (please see above).

The case of the radar resolution being much less than the buoyancy scale has also been discussed in Hocking et al. (2016) pages 409-411, and of course also in the paper by Kantha et al., (2017),and all parties agree that in such cases ∈ should be proportional to vrms^3 - it should be noted that this is true, and the complications discussed above arise from the cases where the pulse -length exceeds the buoyancy scale (or equivalent) and where data-lengths are not too long.

From our point of view, the statistical σ^3 law cannot be explained by the sole condition of the radar resolution much less than the buoyancy scale. It may be the case on some occasions but not always (as we showed for one case in the manuscript, for example, but there are many other cases as described by Luce et et al. 2018). We speculate that this observation is the result of a multitude of different situations depending on multiple factors (sources and stage of turbulence, stability, shear, volume, etc.) so that a given model (e.g. ε_N or ε_S) can be valid but for specific conditions and may not be applicable to individual cases, but is satisfactory as a statistical average over an ensemble of various conditions..

One positive point from this paper is that it seems we as a community are getting better at measuring ∈ values - contrast this to Fig.1 of Hocking and Mu, in which even the definitions of "light", "moderate" and "heavy" turbulence differed by factors of 10 and more, depending on the author. Despite all the concerns in this paper, Fig. 6 shows that between ∈ = 10^{-4} and 10^{-2}, all models agrees to better than half a decade (factor or 3) nowadays.

Thank you. Once again, we thank this reviewer for a thoughtful review.