1. Author's responses to Reviewer comments

We gratefully thank the three Reviewers for their suggestions and comments. Below we present our response to each Reviewer. General comments are considered first, which are followed by point-to-point answers to specific comments. (The Reviewer comment is shown in *italics* and highlighted with yellow, which is followed by our response in plain text).

1.1 Reviewer #1

General comments

This paper describes an analysis of surface-based remote sensing observations of a stratus boundary layer over few days. The observations are taken at a coastal site (Mace Head, Ire). The analysis features vertical motions from a Doppler lidar plus some ancillary data from a cloud radar (principally used to define cloud top). Conditions vary diurnally and synoptically over the observation period. The authors concentrate on profiles of properties of the vertical velocity variance sprectrum including the variance, skewness, spectral peak, and rate of dissipation of turbulent kinetic energy (TKE). The time series of these variables are interpreted in terms of a coupled vs de-coupled boundary layer.

In general the paper is reasonably well-written. The background and methodology sections are fairly straightforward and comprehensible. These sections are mercifully brief but provide enough information to make the paper self-contained. The discussion of meteorological conditions is lengthy and is almost a blow-by-blow description of the events of the entire period. To some extent this is needed to set the synoptic context of the changes that are observed. These are confusion factors in attempting to relate turbulence dynamical effects to local BL structure, etc. I confess I found this a bit hard-going, mostly because of the poor quality of Fig. 3-5, which are real eye-strainers. In section 4 the authors strive manfully to relate the estimated turbulent variables to important changes in BL structure and forcing. Decoupling is a key aspect. It is apparent that this is a messy business and just two days of data are not going to bring any clean insights.

In my opinion the paper represents a usable description of an amusing data set. The major weakness is that the authors provide little guidance on how their data are unique and if they have found new insights. Assuming they can do a little more homework on this, I recommend publication with minor revisions. I also have a few editorial comments for the author to consider.

We will adjust Figures 3-5 as noted by the Reviewer. For example, we will divide Figure 5 to two parts to be more easily viewed: the vertical velocity statistics will be given in a separate figure, and the averaging time will be extended from 30 min to 1 hour to produce a cleaner and more consistent presentation.

We will emphasize the motivation of this paper more clearly in the introduction, i.e. the paper demonstrates the versatility of continuous Doppler lidar measurements to characterize the boundary layer structure and focuses on providing additional information to support the diagnosis of the mixed layer properties based on vertical velocity statistics from the lidar. One of the goals is also to investigate how the transition between coupled and decoupled mixed layer states affect the inertial subrange scaling at different height inside the boundary layer.

Specific comments

1.

I suggest the authors make it painfully clear that their turbulence observations are essentially sub-cloud only. See Ghate et al. (JAMC, vol 53, p117-135) for an example of combined lidar and radar turbulence observations.

We will note this in the Introduction, it says "Unless otherwise mentioned, our analysis focuses on the properties of the below-cloud portion of the boundary layer only, in contrast to e.g. Ghate et al. (2014), who employed a combination of data from a Doppler lidar and a cloud radar".

2.

*The parameter Lambda0 is actually the wavelength associated with the wavenumber peak of k*S(k). I don't know why you would refer to it as a 'cut-off wavelength'.*

The 'cut-off' refers to the maximum wavelengths that belong to the inertial subrange. We will adjust the wording to "transition wavelength" throughout the manuscript.

3.

Suggest given a value for a (eq. 4). Did you state what value you used for mu?

For *mu* we tried a few different values. *mu* = 1 gives the von Karman spectrum, but we ended up with a value of 1.5 as it better matched most of the observed spectra and provides a sharper curvature of the spectrum at the transition between -5/3 slope and the smaller wavenumbers. For this, *a* is approximately 0.69.

This will be stated in Section 2.2.

4.

*As an alternative to eq. 6, you can use the value of k^(5/3)*S(k) for wavelengths smaller than Lanbda0 to compute epsilon. It would be interested to see how those values compared.*

We tested this by calculating the dissipation rate from the vertical velocity spectra for the period 12-18 UTC on the 24th of Feb in Mace Head at the three heights used for spectral analysis in the original manuscript. We used the fitted spectral model (Section 2) for the integration over the wavenumber space in order to get better representation at the short wavelengths. The results are shown in Figures 1 and 2 at the end of this document. For small dissipation rates the method suggested by the Reviewer compares pretty well with those from Eq (6) in the manuscript (based on O'Connor et al. 2010, Figure 1 below). For larger dissipation rates, the method suggested here tends to underestimate as compared to the values from Eq (6). Figure 2 of this document shows that at low altitudes the agreement between the two dissipation rate samples is relatively ok, while at higher altitudes there is somewhat more variation in the dissipation rates from Eq (6) than in the ones from the method suggested by the Reviewer. The main reason why the method based on Eq6 gives more scatter in the dissipation rates is that they were derived using just a 2-minute window for processing the raw Doppler retrievals, while our spectral analysis uses samples over 30 minutes. Of course, other possible contributing factors range from accuracy of our spectral retrievals to possible shortcomings in the original dataset. A more in-depth investigation into this might be a topic for another study.

This comparison is reported only in this document since they do not impact the analysis or the results in the manuscript.

5.
One well-used index of decoupling is the difference in the lifting condensation level (LCL) and the observed cloud base height. Is it possible to provide that?

Unfortunately, no collocated thermodynamic profile measurements were available with the lidar measurements. However, we have attempted using the operational soundings from Valentia, although it is pretty far away from Mace Head. Nevertheless, the surface based LCL in Valentia is generally much lower than the actual cloud base height in Mace Head on Feb 24th (300-500 m LCL vs. ~900 m cloud base, thus LCL is pretty well in line with the zero-skewness height). Towards the night and on Feb 25th the LCL increases to about 600-650 m which is relatively close to the cloud base height in the morning hours. This stems from the fact that the cloud layer becomes more strongly coupled with the surface due to intensified cloud generated turbulence which encroaches into the weakly turbulent surface layer.

The radar dataset we used in this paper with CloudNet processing includes collocated model data profiles (the Met Office unified model UK4 in this case). Thermodynamic profiles and LCL were analysed from the model data as well. The data shows hints of a decoupled structure early in the afternoon but most of the time this is not visible. However, the lidar profiles of dissipation rate and vertical velocity variance do indicate a decoupled structure during the afternoon of the 24th and thus we suspect that the model has trouble adequately describing the decoupled structure. This is seen by e.g. by comparing the statistics profiles from the 24th and the 25th, the latter showing a quite characteristic cloud driven but well mixed more strongly coupled boundary layer.

We will briefly note the Valentia soundings. Moreover, we will provide additional line plots of the statistical profiles as suggested by Reviewer #3 to provide a more detailed presentation (Figs 6 and 7 in the revised manuscript).

6.
Speaking for myself, I don’t think Fig. 2 adds much to the paper and could be eliminated.

We will eliminate the figure from the manuscript.

7.
I found the cloud mask shown in Fig. 6 to be useful, suggest it be added to Figs. 4-5. Also suggest enlarging Fig. 5 to make it easier to see.

We will add the cloud mask to the plots of vertical velocity statistics. We will divide Figure 5 into two parts for it to be more easily viewed (“raw” data and statistics separately).

1.2 Reviewer #2

General comments

The paper’s background is succinct and straightforward, the methodology is sound, and the synoptics of the intensive observation period is adequately described. I question the validity of the interpretation of BL vertical structure based on turbulence-scale Doppler lidar kinematic data, especially given the lack of thermodynamic data. Below, I make some suggestions that may corroborate or contradict this interpretation. This probably requires more than a major revision, and certainly would fundamentally alter the paper.

While the basis for this conclusion is correct, the paper does not exclude other factors that may explain the change in w skewness. I see this as the main weakness in this paper. I am especially skeptical because the Doppler lidar wind speed profiles (Fig. 4) do not show any shear layer corresponding with the "decoupling height" (the height of the interface between surface-based and cloud-driven mixing), and because the lidar backscatter power (Fig. 5a) does not show an aerosol layer corresponding with the same stable layer.

The main remedy I suggest is to use proximity temperature and humidity profiles (e.g., from radiosondes) to show the decoupling, and the evolution of the decoupling height. It would be very nice to quantify decoupling strength at the interface. This would be the nail that seals the case, but presumably, such data are not available. In that case the paper will be much weaker, but some venues can be explored to seek further evidence. Six possible venues are listed below.

There is indeed a lack of thermodynamic data, which is addressed further in our responses to the specific comments by the Reviewer. We will add discussion about other possible contributing factors to the vertical velocity statistics in the manuscript. However, even though we cannot provide an estimate for the decoupling strength, we do argue that the kinematic data does make a point for the decoupled boundary layer structure, as further discussed in our responses below. However, we will revise these considerations as the turbulence in the surface layer is mostly very weak and we will also add discussion about the reasons behind it to Sections 4.1 and 4.2 as suggested by the Reviewer.

1.
Explore the flow field relative to the terrain near Mace Head, which appears to be close to a cliff overlooking the ocean. As the wind speed decreases around t=18 hrs in Fig. 4, there may be a shallow layer of offshore or drainage flow. Fig. 4 could be reproduced for wind direction. Changes in wind direction can produce changes flow relative to the terrain and changes in stability, and thus in vertical velocity moments.

We will add the suggested figure. As the wind speed and vertical velocity turbulence are quite weak after 18 UTC, especially near the surface, it is possible that the vertical velocity skewness values are not physically significant at this time.

We will consider these comments in Section 4.1.

2.
It is not clear how the decoupling height diagnosed from the profiles of w skewness. The w skewness field based on 30 min intervals is quite noisy (Fig. 5d). It often changes sign over the full depth of the BL from one instance to the next. The velocity uncertainty increases with decreasing SNR or power, which is quite obvious from a comparison between Fig. 5a and d. It would be good to see whether the pattern becomes more crisp (or vanishes) under different velocity QC, processing, and averaging periods.

We attempt to approximate the decoupling height as the level where the vertical velocity skewness changes sign. We have experimented with longer averaging times and the negative skewness structure indeed becomes somewhat more robust with longer averaging. The skewness profiles remain quite consistent with the dissipation rate and variance profiles.

We will revise the figures showing vertical velocity statistics and analysis of the height of the layer interface using 1-hour averaging timesteps instead of the 30 mins in the original manuscript to provide a cleaner presentation. We will also add new figures (6 and 7 in the revised manuscript), which present line plot profiles of the vertical velocity statistics in more detail, with 1-hour averaging time. The velocity estimates used for calculating the statistics are subject to SNR threshold based quality control as well as to methods presented in O'Connor et al. 2004. In addition, the methods presented in O'Connor et al. 2010 were used to correct the dissipation rate for observation based errors.

We will elaborate on the methodology in Section 2.2. In addition, in Section 4.2, it will be more explicitly shown that scaling of L0 supports the skewness-based decoupling height estimate, at least for periods with pronounced separation in the skewness-profiles.

3.
Repeatability is always useful. This is a case study of a 24-36 hr period. Do the same relations apply in other fair-weather Sc-topped BL conditions?

The synoptic situation is quite common in mid-latitude winter marine environments and the boundary layer properties are not influenced by anything extraordinary. Unfortunately, given the instrumentation at our disposal, it has been difficult to find measurements which could be used for a similar spectral analysis. This is essentially because of two things: First, the analysed period from Mace Head provided quite a unique set of data in terms of quality and prerequisites for spectral analysis. The marine air mass provided sufficiently strong signal with our lidar instrumentation, so that the vertical velocity data wasn't too noisy for deriving the power spectra. Related to this, the SNR of the velocity retrievals can be improved by increasing the integration period for individual velocity samples, yet as a downside, too long integration periods make it impossible to derive meaningful spectra, since (with Taylor's hypothesis in mind) it will mask out higher wavenumbers. Second, the wind speed during the analysed period was moderate (below 10 m/s) which also contributes to better sampling of the higher wavenumbers. Similar instruments to those used in the manuscript have been employed in different parts of Finland by the FMI. Generally, in continental sites, the instruments are not able to provide strong enough signal below the cloud base for robust vertical velocity retrievals at sufficient time resolution, at least for spectral analysis. However, statistical profiles can be derived more often and similar structures of skewness and variance profiles can be identified in marine environments, such as that in the Utö island in the Finnish archipelago. In a future study it is thus possible to extend this investigation and quantitatively study the occurrence of the boundary layer structure reported in this paper with data from different sites. However, for the present manuscript, we stick to the Mace Head data.

4.
Much can be learned from the variation of w power spectral density with height across the interface. If the paper's main conclusion is correct, then one can expect a minimum in TKE near the decoupling height, simply because of distance from the TKE generation regions, i.e. the cloud top layer and the surface. This is unlikely to be the case, because TKE and turbulence dissipation rate tend to strongly correlate, and the computed turbulence dissipation rate (Fig. 5e) does not appear to have a minimum near the decoupling height (Fig. 6), although the time axes do not match so it is difficult to compare the two Figs.

Line plot figures presenting profiles of vertical velocity variance, skewness and the dissipation rate will be added to the manuscript. They show in detail that on many occasions on the 24th Feb the dissipation rate (and thus presumably TKE) is clearly strongest in the upper part of the boundary layer near the cloud deck. Similar results are seen for the standard deviation as well. Especially in the afternoon there is often a sharp decrease in the dissipation rate below the height of decoupling, and the turbulence near the surface actually remains quite weak. Thus we expect it would be difficult to resolve a clear minimum in the turbulence intensity near the decoupling height (though that kind of structure can actually be seen at least on one occasion where mixing close to the surface is intensified for a short period). We will add discussion about this point in Section 4.1.

Moreover, the boundary layer on the 25th of Feb, after midnight, appears essentially well mixed, but the statistical profiles provide strong indication that this is cloud driven as well. There is also a clear contrast in the structure of the profiles as compared to the afternoon of the 24th. This will be elaborated on in Section 4.1 as well.

5.
Cloud-top driven mixing (or cloud top entrainment instability) has been shown to be active in various Sc environments (see review by Woods 2012). It is only hypothesized to be active in this case. Profiling Doppler radar data within the drizzle layer should reveal the presence of vertical velocity turbulence. I believe these data are available.

Profiles of the vertical velocity variance and skewness in the drizzle layer are shown in Figures 3 and 4 of this document. There is considerable mixing observed in the drizzle layer as sigma_w is about 0.4 m/s through much of the depth of the layer on 24 Feb and even stronger on 25 Feb. Moreover, especially on 24 Feb, the skewness is predominantly negative or around zero within the cloud layer, which is expected. Further, on 25 Feb, positive skewness values are seen at approximately the base of the cloud, while below-cloud profiles from lidar (Fig 7 in the revised manuscript) show increasingly negative skewness towards the surface, which is expected for cloud-driven well-mixed boundary layer.

We will comment on these results in Section 4.1 but prefer not to include more additional figures. It now says "The collocated Doppler cloud radar observations also indicate considerable turbulent activity within the cloud layer".

6.
Decoupling strength can be estimated from the difference in potential temperature at the surface and that at cloud base. The latter may be available from a zenith infrared thermometer. If not, then the difference between the lidar-determined cloud base height and surface-based LCL is a good measure of decoupling strength, although it will not give the decoupling height.

As stated, we do lack the thermodynamic profiles. Compared to the LCL from operational soundings at Valentia the cloud base height observed at Mace Head is much higher (300-500 m LCL vs. ~900m cloud base). The model data profiles provided with the radar datasets generally seem to miss the decoupled structure (please refer to comment 5 by Reviewer #1). We argue however that the kinematic statistics do show a clear indication of the decoupling, since in many cases the variance, as well as dissipation rate peak near or within the cloud layer and decrease towards the surface. In addition, the scaling of L0 generally supports these results.

We will briefly comment on the Valentia soundings in Section 4.1. Although they are pretty remote from the lidar observation site and thus can not be regarded as very strong evidence, the meteorological conditions are somewhat similar to those in Mace Head at least towards the afternoon.

Minor comments

1.
The theory in Eqns 1-6 is sound but the text does not specify the value chosen for the variable mu.

We ended up using the value 1.5, which generally provided the best fit to the observed spectra, and causes the curvature over the transition from the -5/3 slope to outer scales to be slightly sharper than that in von Karman spectrum with mu = 1. We will elaborate on this in Section 2.2 (See also our response to the 3rd comment of Reviewer #1).

2.
Table 1: add units to range resolution (m)

Will be corrected.

3.
Fig. 1: please use real data to make the point. The power spectral density curve shown is physically impossible.

We will replace this with an actual example.

4.
It is not quite correct to use "time (hours UTC)" in the abscissa title of most figures. One option is to use "time since 00 UTC on 24 Feb 2012 (hours)".

Will be corrected.

5.
Fig. 6: The black region is NOT the cloud layer. Rather, it is the drizzle layer, which often extends below cloud base. A Ka-band radar can only detect drizzle-size drops (e.g., Fox and Illingworth 1997).

In Figure 6 the cloud base is determined from the lidar data, while radar is used to determine only the cloud top, which is generally invisible to the lidar (This was mentioned already in the original manuscript, p. 24132, lines 23-24). Therefore the black region can be expected to provide a rather good estimate of the vertical extent of the cloud layer.

6.
The evaluation of upper wavelength of the inertial subrange (lamda_o) in Figs. 6 and 7 is done at three heights within the BL, whose depth is based on the radar profiles (cloud echo top). These heights cut across the decoupling height. If indeed the surface-driven layer clearly is decoupled from the cloud-driven above, it would be more interesting to characterize lamda_o in this two respective layers.

We performed additional analysis where L0 is analysed at two levels, one inside the surface layer and one in the cloud driven mixed layer (in the middle of the layers). The results show that L0 sampled from the cloud driven layer scales pretty well with the corresponding layer depth, even though the variability is rather high. The surface-based L0 shows quite robust scaling during pronounced separation in the profiles of skewness between the surface and cloud-driven regimes, while for most other instances, similar results are not seen.

We will update the original Figures 6 and 7 accordingly and elaborate on these results in Section 4.2.

1.3 Reviewer #3

General comments

1.
The authors seem to have a conceptual model in mind, which should be made more explicit in the Introduction. This would make the paper more accessible to a wider audience, rather than just measurement specialists. Their model is first referred to in section 4, and seems to be based on the idea of "competition" between surface- and top-driven convection. In fact, both surface and top driving may be present, and cooperate (rather than competing) to produce turbulence. When thinking purely of skewness, the two drivers do have opposing effects. The questions that should be addressed by the analysis are: What are the depths of influence of surface and top driving? When do the two depths merge to create a single turbulent layer, and when do they remain decoupled? What is the degree of coupling, since coupling is a continuum between fully coupled and fully decoupled? The last question can be addressed by emphasizing variance profiles. Another issue is what processes are reducing coupling (or increasing stability), since turbulence always acts to reduce stability?

We will revise the idea of competition, which indeed does refer to the opposing signs of skewness, that also is an indicator of the direction of the transport of turbulent kinetic energy. It is also a fact that during the 24th, the surface layer is mostly quite weakly turbulent, so that the influence of surface-based processes on the state of the entire boundary layer is likely relatively weak and the boundary layer structure depends more on the factors that influence the intensity of the cloud generated mixing. Moreover, the scaling of L0 from the different layers can perhaps be used to infer the depth of influence of the more intense cloud-driven layer.

We will elaborate on our point of view on the impacts of different layers of the boundary layer structure in the Introduction. We will also include additional discussion about the points raised by the Reviewer in Sections 4.1 and 4.2.

2.
In addition to the figures already included, some line plots of vertical profiles of variance and skewness during the different regimes (appropriately time-averaged) should be shown. This will allow the reader to understand better the data presented in the time-height plots.

We will add line plots with vertical velocity statistics averaged over 1 hour segments for the afternoon of the 24th and the morning hours of the 25th which nicely present the key differences between the two cases (Figs 6 and 7 in the revised manuscript).

These will be commented on in Section 4.1.

3.
Throughout the paper, the vertical coordinate is scaled by the cloud-top height, which is defined as the boundary layer height. This is not treated completely consistently, since it is acknowledged later that the expectation is that each sublayer should scale with its own depth.

We will adjust the normalizing issue in the manuscript and change the normalization to account for the sublayer depths. Our results show that L0 sampled from the cloud driven layer scales rather well with its own layer depth. Similarly, during periods when the surface layer deepens (as during 12-15 UTC on the 24th), L0 sampled near the surface also scales well with the corresponding depth. However, the same is not always true for weaker separation between the cloud-driven and surface-based layers as diagnosed from the skewness profiles.

We will revise the methodology in Section 2.2 and the adjust the analysis accordingly in Section 4.2.

Specific comments

1.
In the last paragraph of section 4.1, the attribution of the measured effects on the boundary layer is unclear. Is there really an influence of the land, even though the flow is still onshore? Advection of cooler air aloft would also reduce stability and increase mixing, can this be ruled in or out?

We cannot strictly rule this out and we will consider this possibility in Section 4.1. It says:

"Further, Fig. 3 suggests some height-dependent fluctuation of the horizontal wind during this period. The fluctuations might act to trigger periods where the stability near the surface is reduced, allowing a surface-based TKE production to affect a deeper layer. In particular, we cannot rule out the possibility of advection of cooler air aloft, which could potentially act as a driver for such events. At the same time, this could also act to slightly increase the stability in the upper portion of the boundary layer, further contributing to the relative strengths and extent of the cloud-driven and surface based mixed layers."

2.
In section 4.2, it is indicated that the spectra have various structures. It might be helpful to include some representative spectra in a supplement.

As suggested by Reviewer #2 we will replace Figure 2 with an example from actual data which show the typical retrieval from which L0 is inferred. The different "structures" that sometimes take place are mainly just artifacts due to noisy retrievals.

3.
page 24133, paragraph beginning with line 19: Does L0 scale with the horizontal size of the clouds or breaks, rather than the layer depth?

No robust dependence is found. This kind of behavior might be found for surface-based cumulus convection. Here L0 appears to be mainly controlled by the TKE generation at cloud top and the resulting BL structure: This paragraph is adjusted according to the revised analysis of the inertial subrange scaling (following the general comment no.3 by the Reviewer).

4.
page 24133, line 27: Here is a particular example of the conceptual model issue. In what sense do the surface and top driving compete to prevent formation of a mixed layer? Don't they in fact cooperate, but with possibly differing strengths?

We were focusing on the predominate direction of the flux of kinetic energy that can be inferred by investigating the sign of the skewness parameter, thus indicating the dominant source of energy in each layer which we attempt to use to characterize the boundary layer structure.

In accordance with the Reviewers notion, we will revise this terminology throughout the manuscript and elaborate the idea in the Introduction.

5.
page 24134, line 11: Is the supression of L0 during this time real, or an artifact due to very weak turbulence?

For the surface-based samples, the weakness of turbulence might affect L0 during this time. However, this is not the case for the samples from the cloud driven layer, which also show relatively small L0 even a few hours after the cloud driven mixed layer covers essentially the entire boundary layer.

This discussion will be revised according to the new figure presenting L0 in Section 4.2. It says: “The suppression of the surface-based L 0 can also be due to artifacts introduced by the weakness the turbulent mixing”.

6.
page 24134, line 17: Wind shear at the cloud top influences entrainment, not wind in general.

This will be corrected.

7.
page 24136, line 13: It should be expected that decoupling reduces L0 as it is defined here, since in a decoupled structure scales should go with their own layer depth, not the depth of the whole structure. This is consistent with the statement of previous expectation in lines 25-26.

We will revise this statement. It will be acknowledged that L0 from the cloud driven regime does scale relatively well with the mixed layer depth. However, it is noted that this is not always true for L0 in the surface layer, except when the surface layer extends to more considerable depths into the boundary layer.

8.
Figure 6 and text discussing it: It should be acknowledged that surface-driven layers without cloud have negative skewness near their tops.

It now says is Section 4.1:
“... negative skewness of vertical velocity, which has been shown to indicate cloud-driven mixing (Hogan et al., 2009), is a predominant feature of the below-cloud mixed layer (although it can also be observed near the tops of clear-sky surface-driven layers)”.

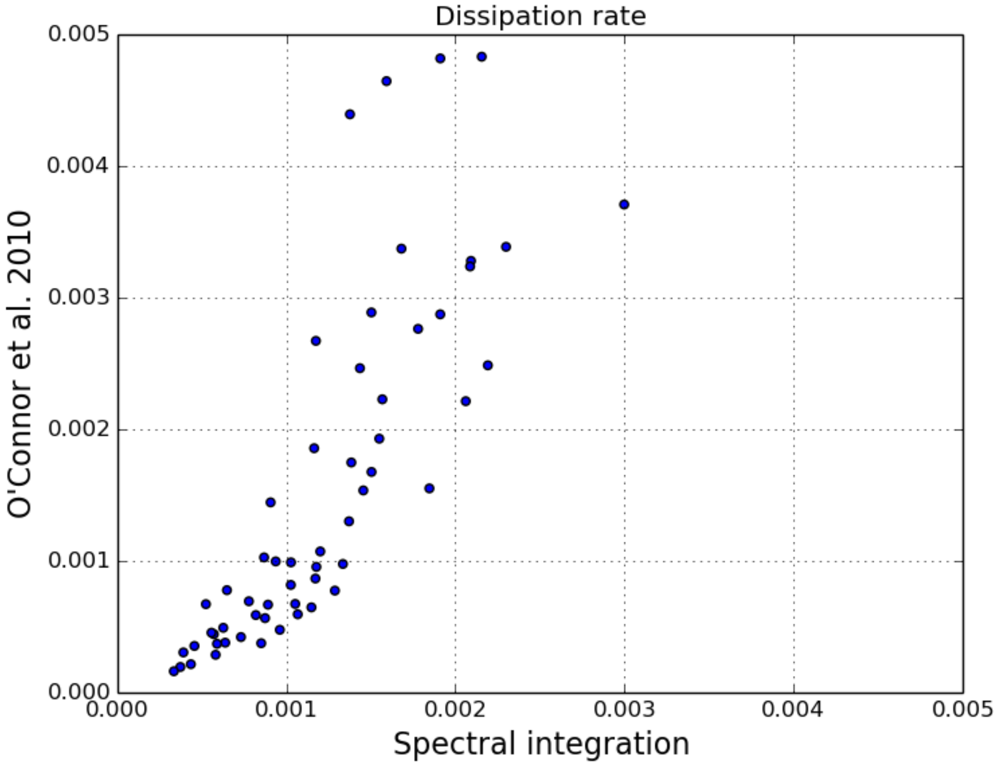

Figure 1: Scatter plot of dissipation rate using Eq. (6) in the original manuscript (O'Connor et al. 2010) vs. that obtained by integrating over the powers pectra as suggested by the Reviewer.

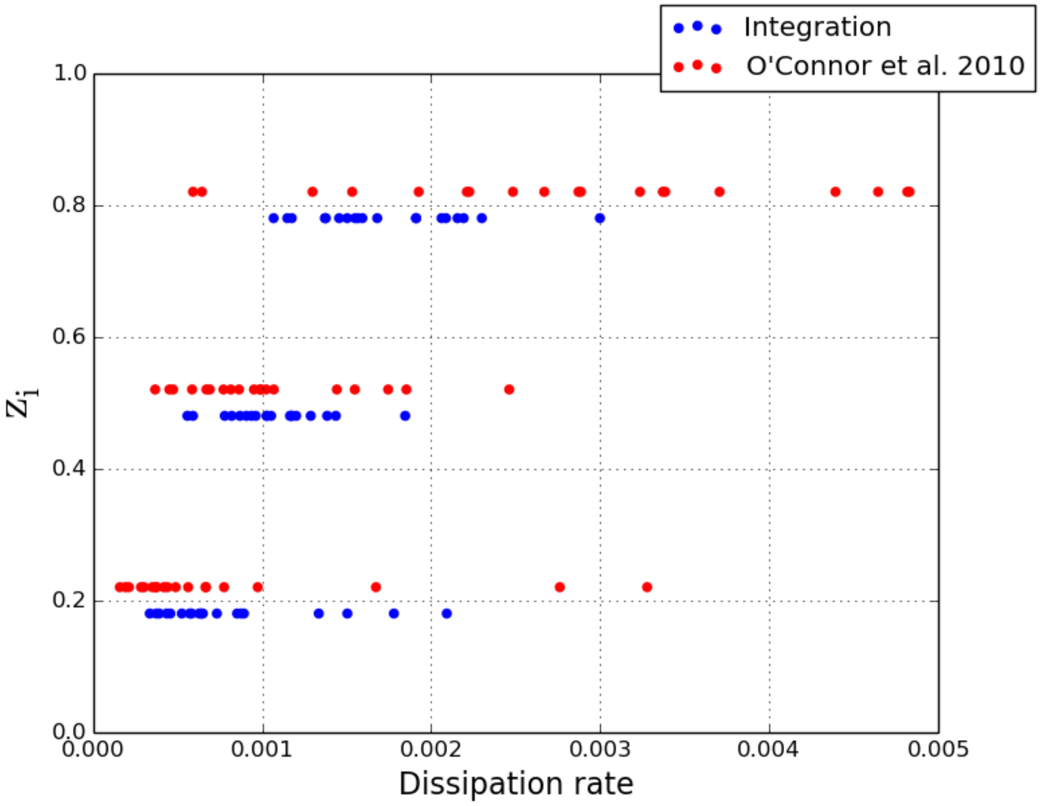

Figure 2: Same as Fig. 1 but analysed separately at three sampling levels (normalized by the boundary layer depth diagnosed from cloud top height). The samples are from the same altitudes but a small offset is applied in the figure for better presentation.

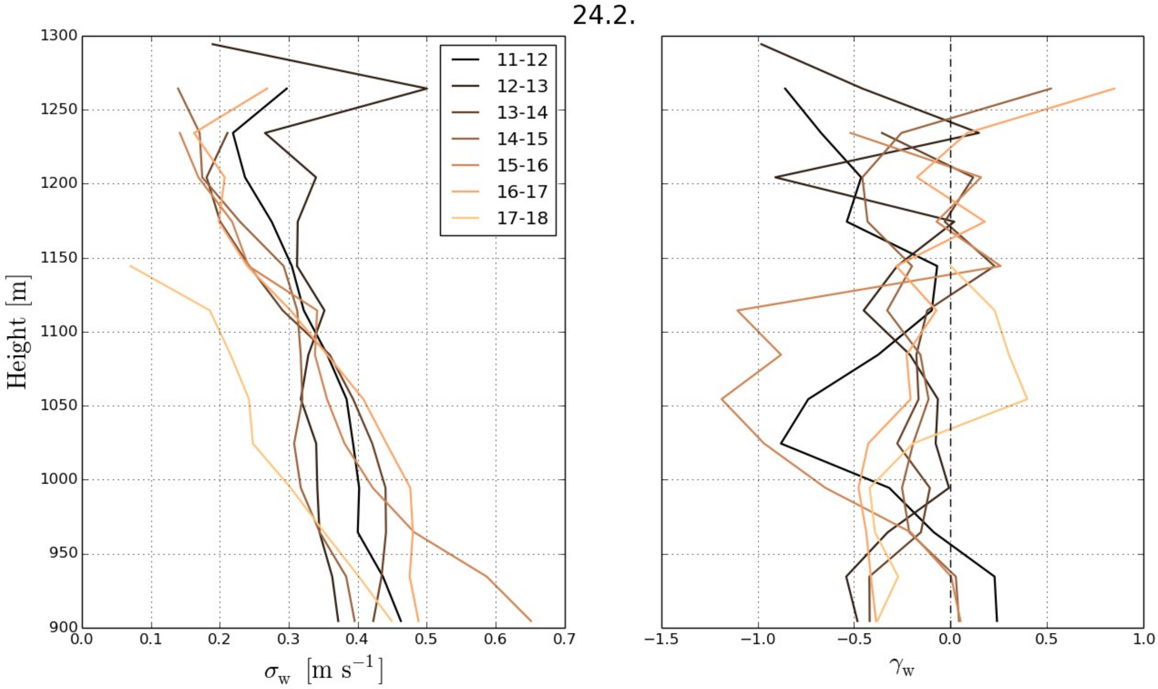

Figure 3: In-cloud standard deviation and skewness of vertical velocity as derived from a Doppler radar on 24 Feb (the height range presented corresponds roughly to the thickness of the cloud).

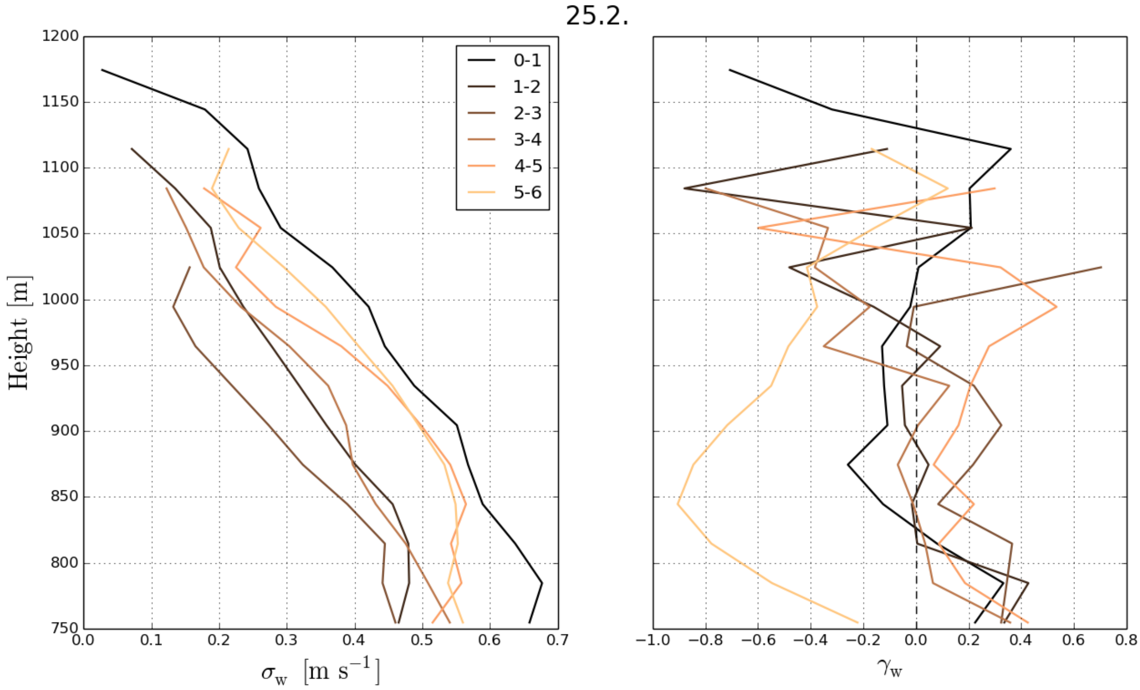

Figure 4: In-cloud standard deviation and skewness of vertical velocity as derived from a Doppler radar on 25 Feb (the height range presented corresponds roughly to the thickness of the cloud).