Response to reviewer 1:

The paper is part 2 of a description of basic characteristics of the Schneefernerhaus Research Station (UFS); part 1 Risius et al., AMTD 5, 541-568, 2015. Such studies are important for further research work on this site.

Absolutely unusual is the definition of large scale turbulence (part 1 of the paper) and small scale turbulence. Turbulence in meteorology is classified into macroscale turbulence (synoptical scale) and microscale turbulence (Etling, 2008). In between is mesoscale turbulence (spectral gap), e.g. local circulation systems in the mountains. The large and small scale turbulence are both in the range of microscale turbulence. Probably the authors want to separate the microscale turbulence into that which has frequencies smaller than the frequencies of the inertial subrange (large scale), and that with frequencies of the inertial subrange and dissipation range (isotropic turbulence, small scale). The classification should be made clear in accordance with the textbooks of atmospheric turbulence and atmospheric boundary layer physics.

This misunderstanding about the terms „large-scale turbulence" and „small-scale turbulence" is apparently due to the fact that these terms are used differently in different communities. In the laboratory turbulence community (although see also the recent textbook „Atmospheric turbulence" by J. Wyngaard) the term „small-scale" is frequently used for scales where intermittency effects become important. We use that sense of the word, and to ensure that the meaning is clear we have edited the manuscript accordingly. Specifically, we have made the following two changes:

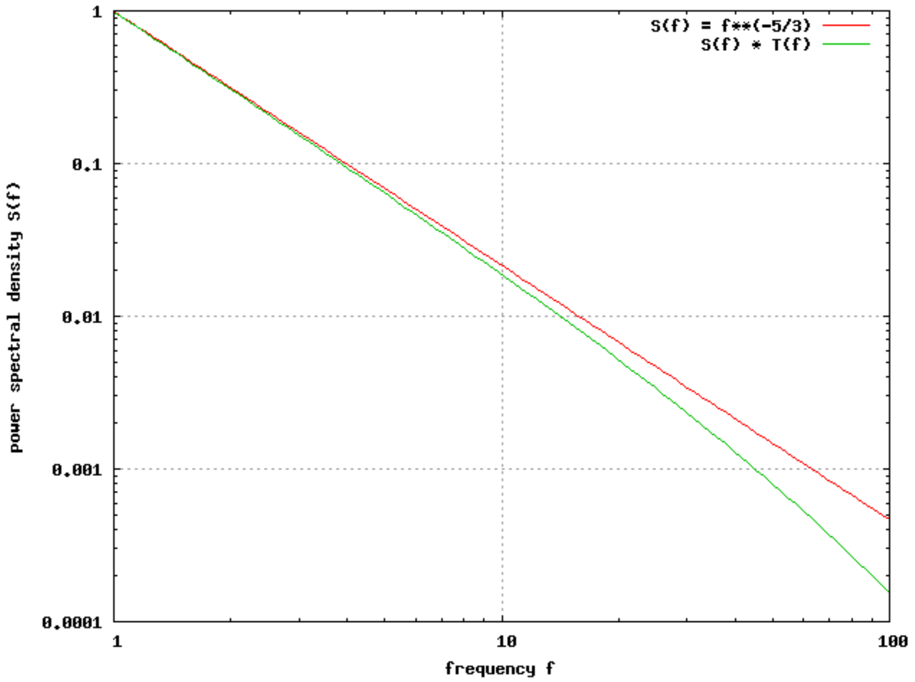
- ***i)*** ***We changed the title to „High resolution measurement of cloud microphysics and turbulence at a mountain-top station"; In this way, the term fine-scale no longer appears in the title, and cannot lead to misunderstanding before the definition appears in the paper.***
- ***ii)*** ***In the introduction (2nd paragraph) we now specify the scales of interest and avoid the term „small-scale turbulence". Instead we mention that this paper focuses on scales well within the inertial subrange down to about 10 time the Kolmogorov scale.***

Sonic anemometers sample with a high frequency (e.g. 100 Hz) and make an oversampling to exclude aliasing effects. A data analysis only makes sense with a time resolution of about 0.05 s because of the long measuring path (relation of time and space scales of atmospheric turbulence). The time resolution of hot wire anemometers or cold wire thermometers is much higher. Please make clear which sensor was used for which investigation.

We agree with this comment but it should be mentiond that „useful" resolution of the sonic depends on the mean flow velocity U. A simple spectral transfer function is given by $T = (1+\beta L k)^{-7/3}$ where $\beta = 0.027$ is an empirical factor, $L = 0.15$ m is the distance between two transducers and $k = 2 \pi f / U$ is the wavenumber (see Wamser et al. BLM, 1997 and references given therein). We plotted this transfer function in the figure below and the effect of linear averaging on the ideal spectrum in the inertial subrange on high frequencies is quite obvious for high frequencies. The 100Hz resolution of the Solent HS ultrasonic anemometer was only used for the PDF of velocity fluctuations presented in Fig 4. of the original manuscript. For the revised version of Fig 4 we checked the influence of the line averaging effect and it was not visible. In fact the line averaging acts like a low-pass filter.

In the revised version we included in every figure lable the sensor which was used for the

observations to avoid any misunderstandings.



Please give for Fig. 2 and 6 the date and the time, otherwise it is not clear if this is (partly) the same data set or not. Probably the data set of Fig. 6 contains the data of the red block in Fig. 2, but this information is missing in the legend.

This information is given in the text at the beginning of Sec 3.2. All velocity and turbulence data (Fig 2 to Fig 7) are based on the same sampling time; Fig 4 are Fig. 7 is based on the data marked with the red box in Fig 2. We have clarified this in the revised manuscript in each Figure caption.

Why have you used a linear detrending? This is unusual (Finnigan et al., 2003) and generates an additional transfer function (Rannik and Vesala, 1999).

Because „linear detrending" does not fulfill the rules of Reynolds averaging we have re-calculated the PDFs in Fig. 3 by subtracting the mean value of each sub-record and we also now show the mean values in Fig 2 (orange lines). The PDFs did not change significantly from what was shown before so the conclusions are unchanged.

Intermittencies are usually not periodic (Fig. 6). Could this be a locally generated eddy (Whiteman, 2000)?

We agree that a few bursts of local energy dissipation rate may be quasi-periodic with a time period of 50 s. This is somewhat evident for the time between t = 2350 and 2550 s. A locally generated eddy is a plausible explanation and we have included a short paragraph to mention this issue. A longer, stationary time series would be necessary to conclude unambiguously that a periodicity actually exists. A short comment about this issue is included in the revised version.

Fig. 2 and 3 are typical for turbulence near the ground. Fig. 4 only shows that the hot wire anemometer give reproducible values.

The aim of this data analysis is to show that even in the presence of the mountains the turbulence on small scales (below one meter or so) show classical (K-42 and K-62) behaviour. This is the motivation for Fig 4 to Fig 7. Fig 2 and Fig 3 simply introduce the data set.

The reviewer is not familiar with cloud physics. This part should be reviewed by a specialist in cloud physics.

Contrary to part 1 of the paper the results are partly compared to laboratory and airborne measurements. Possible differences to results for the peak of the Zugspitze may be interesting.

A comparison to airborne data is discussed for the combination of turbulence and cloud data. The peak of the Zugspitze has not been considered because we want to show that the UFS with its comprehensive infrastructure can be used to study cloud-turbulence interaction.

Because part 1 of the paper can probably not be published, this paper should also include, after some modifications, some basic data of the Schneefernerhaus, and the dissipation analysis of part 1 can probably be partly included.

Please see the responses to the reviews of the companion paper by Risius et al.