Reply to the comments by Referee #1

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We thank Referee 1 for his/her review of our work. We respectfully disagree with the referee's findings. Our sense is that the referee's criticism originates from misunderstanding of the main purpose of our paper and/or the confusion in the usage of technical terms in different communities. In the revised version of the manuscript we have made a concerted effort to better explain these issues. In particular, we changed the titles of our papers to describe more clearly the scope of our work. In the revised version, this manuscript (previous Part 1 of the two) is titled "Schneefernerhaus as a mountain research station for clouds and turbulence" and the previous Part 2 paper is now titled "High resolution measurement of cloud microphysics and turbulence at a mountain-top station".

Our work is not concerned with the study of the climatology or meteorology at Zugspitze. We agree that these are important problems by themselves, as stressed by the referee. We intend to show in our manuscript that both the turbulence and the cloud properties at Zugspitze carry similar characteristics as in other well-studied turbulent flows and in airborne clouds. This is of great relevance for a large community of scientists studying inertial particle dynamics and also for scientist interested in cloud microphysics.

In this manuscript, we are concerned mostly with the turbulent flow in the scales ranging from the dissipation range up to the local peak of the energy spectrum at the other end of the inertial subrange (the "large scales", see below). In the accompanying paper (Siebert et al.), we focus on the cloud properties at dissipation scales (or the "small scales", see below) since the sizes of the cloud droplets are typically much smaller than the dissipation scales of the turbulence and hence their dynamics is directly influenced by turbulence at such small scales.

In both papers, we presented data from on-site observation using various type of sensors that cover the entire range of time and length scales that are important for the turbulence and cloud properties. We compared those data with other data from both laboratory measurements and airborne observations to show that, at least for cloud-turbulence interaction study, the observation station at Schneefernerhaus is well-suited. Our results presented here can also serve as a benchmark characterization of the turbulence and cloud physics conditions at Schneefernerhaus, which can be used for other researchers who are interested in carrying out related studies at Schneefernerhaus to evaluate the usefulness of the research station for their own investigation.

Moreover, we realized that the referee prefers to refer to the range of scales that we study in these two-part paper simply as "microscale turbulence". Within the fluid mechanics community it is customary to divide this range of scales into the "large scale" or the "forcing scale", which corresponds to the range of the local peak in the energy spectrum and is related to the scale at which the kinetic energy is supplied into the turbulent motion; the "small scale" or the "dissipation scale", at which the viscous dissipation converts the kinetic energy into heat; and in-between the "inertial (sub)range", which is the range of scales where the kinetic energy is cascaded down to smaller and smaller scales without significant loss. This nomenclature is conventional in the turbulence community (see, also the textbooks (Frisch 1995, Monin and Yaglom 1971, 1975, Pope 2000, Tennekes and Lumley 1972)) and is also widely adopted in other related communities (see, e.g., (Mac Low and Klessen 2004, Wyngaard 2010)). We therefore followed the same terminology in our manuscript and assumed its general acceptance. The reaction from Referee 1 motivated us to make this more explicit in our revised manuscript.

Below are the point-by-point responses to the comments raised by Referee 1.

1. **The referee writes:** *The paper is part 1 of a description of basic characteristics of the Schneefernerhaus research Station (UFS); part 2 Siebert et al., AMTD 5, 569-597, 2015. Such studies are important for further research work on this site. Unfortunately, this study does not fulfil my expectations for such a type of paper:*

   Climatological information for the site is very important. If no data for a 30-year climatological period is available (only for 2000 2012 is given here), it would be possible to generate such a period using the data of the peak of the Zugspitze. It is trivial that the temperature at Schneefernerhaus is higher than at the peak of the Zugspitze, but of special interest would be the vertical temperature gradient between Schneefernerhaus and Zugspitze and perhaps the occasional existence of an inversion layer.

   The authors reply: As we explained at the beginning of this reply, we did not intend to study climatology or mountain meteorology at Zugspitze. Regarding the referee's comment on the difference in temperatures between Schneefernerhaus and the peak of Zugspitze, we merely reported the meteorological data we obtained, which is part of the general description of the environment at Schneefernerhaus. We certainly did not say anything on whether this difference is trivial or not, which is not the purpose of our manuscript.

   This comment and similar comments later by Referee 1 caused us to more clearly state the purpose of our manuscript to distinguish it better from other works on climatology and/or mountain meteorology.

2. **The referee writes:** *The authors are surprised that the pdf-functions of the wind speed and wind direction do not differ between cloudy and non-cloudy conditions. Why should they differ?*

   The authors reply: Our exact formulation related to this comment is "the flow conditions (wind direction and wind speed) are almost independent of whether the wind is carrying clouds or not" (page 5, lines 14-15 of our manuscript). We merely reported this fact.

   Our intention was not to indicate surprise by the similar PDFs of wind speed and wind direction under conditions with or without clouds. Rather it was to report the results from our observation; it may or may not be surprising to readers, but we think in either case it is important to confirm the wisdom or experience with actual observational data.

3. **The referee writes:** *I cannot understand why topography is the reason for different wind distributions for wind from east or west. Easterly winds are often related to anticyclonal conditions and westerly winds to cyclonal conditions. The authors have to first normalize the wind velocity with the geostrophical wind (available from the re-analysis; perhaps the wind velocity at the peak of the Zugspitze does not differ very much). If after the normalization you see differences, a discussion of topographic effects may be possible.*

   The authors reply: East and west are the two predominant wind directions, so we simply wish to determine whether the turbulence properties are different for the two directions. The large scale geostrophic wind, or other mountain meteorological factors that contribute to east vs west wind, while fascinating in and of themselves, are not our primary concern in a paper dealing with turbulence.

4. **The referee writes:** *A paper about flow conditions at a mountain site, especially when the site is on a slope and in a valley, should discuss typical situations of mountain meteorology like luv blocking, lee waves, lee rotators, lee cavity, and turbulence wake. All these are a function of the Froude number (Stull, 1988;Whiteman, 2000). Because the wind direction is channelized into easterly and westerly winds it would be interesting to know what happens in the case of foehn and Alpine pumping (Winkler et al., 2006).*

   The authors reply: The referee's comments on mountain meteorology are of course valid and we thank him/her for suggesting interesting research problems in that direction. Our focus in the manuscript is on the "microscale turbulence" (in the terminology used by Referee 1), which is not expected to be directly affected by the "macro-" and "meso-scale" flows. The flow at Schneefernerhaus at those scales are part of our measurement and we think it is interesting to report them, which can at least provide more complete information of the environment and may trigger further research along the line suggested by the referee.

5. **The referee writes:** *Also the part concerning clouds is very weak: Do banner clouds affect the Schneefernerhaus? Why should you measure cloud physics at Schneefernerhaus and not at the peak of the Zugspitze? It may be easier to study cloud physics at a mountain station. Is the cloud physics comparable to that of airborne measurements?*

   The authors reply: The reference Wirth *et al.* (2012) in our manuscript is exactly on studying banner clouds at Zugspitze. As the referee is very likely aware, banner clouds appear near the peak of the mountain. In Wirth *et al.*, the observation was performed along the ridge that is above 2800 m, while Schneefernerhaus is at 2650 m.

   As we show clearly in the accompanying paper (Siebert et al.), the clouds properties measured at Zugspitze can be nicely compared with that of airborne measurements. This is the central point of our study.

   Of course it would be great if we can study clouds at both Schneefernerhaus and at the peak of Zugspitze. On the other hand, the purpose of our work is to show that Schneefernerhaus is a site well-suited for cloud-turbulence interaction study. This conclusion suggests that other stationary sites could also be suitable for the same study. Therefore, we have applied to the State of Bavaria to develop Schneefernerkopf, which is at a location above UFS, as another station for cloud-turbulence research.

6. **The referee writes:** *Absolutely unusual is the definition of large scale turbulence and small scale turbulence (part 2 of the paper). 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 scale and small scale turbulence are both in the range of microscale turbulence. Probably the authors want to separate the microscale turbulence into frequencies smaller than the frequencies of the inertial subrange (large scale) and into frequencies of the inertial subrange and dissipation range (small scale). On page 546, line 27 the authors call both ranges local turbulence (?).*

The authors reply: As we explained at the beginning of this reply, we realized that there is a significant difference in nomenclature used in different communities. In the revised version, we carefully explained the terms and the different scales we refer to.

7. **The referee writes:** *From the spectral analysis I cannot see results which differ from the typical conditions. The anisotropy is typical in the investigated spectral range (Lumley and Panofsky, 1964). If there are some effects which differ from typical conditions near the ground, the authors should present these results. Furthermore, it may be interesting if the conditions found near the ground differ from the turbulence conditions for airborne cloud physics investigations.*

   The authors reply: We thank the referee for his/her agreement regarding the value of reporting this observational data. The turbulence (and the clouds) at Schneefernerhaus is the same as other turbulence and the research station can well serve as a site for cloud-turbulence interaction research.

8. **The referee writes:** *The investigation of the structure function and the energy dissipation is a typical issue of the investigations of the inertial sub range. This is probably a task of part 2 of the paper.*

   The authors reply: We think this comment is related to the difference in terminology as we explained above. To make this point clear, we explained more carefully the range of scales when discussing Figure 8.

9. **The referee writes:** *How can you measure the wind field and the turbulence when the distance to the mountains (rock?) is only 20 m (p. 546, line 13)?*

   The authors reply: This comment is not clear to us. There seems to be another confusion in terminology. When we say wind speed measurement, we merely mean measuring the air flow. In principle one can measure air flow velocities even at 1 m away from the mountain (or rock). We postulate that the referee meant that the turbulence statistics will be affected by the distance to the mountain? Even this is a little strange to us since turbulence field in confined geometries, e.g., channel and pipe flows, are commonly studied with essentially the same experimental and theoretical tools as "free" turbulence. Moreover, we pointed out that the most-probable value of the measured correlation lengths of the turbulence is $4 - 8$ m, well below the distance to the mountain.

10. **The referee writes:** *The slope in the small picture of Fig. 8 should be -2/3!*

    The authors reply: Yes. That is of course correct since the inset of Fig. 8 of the previous version shows the compensated plot $fE_{uu}(f)$ and we stated in both the main text and the figure caption that the power spectrum itself well follows the Kolmogorov spectrum $E(f) \propto f^{-5/3}$, which gives $fE_{uu}(f) \propto f^{-2/3}$. We added this now explicitly in the revised version. Note that in the revised version, in response to the comment by another referee, we plot the "local slope" of the spectrum in the inset and show the compensated spectrum $fE_{uu}(f)$ as a new panel in Figure 8. We also added the spectrum measured from the hot-wire anemometry sampled at 1 kHz, which is shown as Figure 4 of the accompanying paper (Siebert et al.), into Figure 8 of the revised version. This extends the covered frequency of the spectrum to 11 orders, from 10^{-8} to 10^{3} Hz.

11. **The referee writes:** *Also the conclusion is very weak and not very quantitative. I have mentioned several points above which should also be concluded.*

    The paper does not fulfil any criterion for describing the flow characteristics and the turbulence structure for frequencies lower than the inertial subrange for a mountain research station. The paper should be rejected. In the event that the second part of the paper can be accepted, the discussion about the dissipation range could be combined with it.

    The authors reply: We feel that the referee's evaluation was strongly influenced by our inadequate attempt to clearly define the relevant range of scales being studied, and the central purpose of the study. Regrettably, some of the comments likely were biased by the resulting misunderstanding; nevertheless, we tried to take full consideration of the comments when revising our manuscript and we hope the referee can give a more positive evaluation of the revised version.

In the revised manuscript, we marked the changes with colored text for easy identification of the revisions. We hope the referee now agrees with us that the revised manuscript has improved its clarity and is accessible to the broad readership of **Atmos. Meas. Tech.**

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