Atmospheric Measurement Techniques Discussions

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Reply on RC1 (Specific Comments)

Matthias Zeeman

Author comment on "Use of thermal signal for the investigation of near-surface turbulence" by Matthias Zeeman, Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2020-500-AC3, 2021

Specific Comments

Line 1. Organized motions of what? Please specify.

Response: Thank you for the comment. I suggest the following change for clarity.

'Organised motions **of air** in the roughness sub-layer **of the atmosphere** …'.

Line 6. What is meant with the term "Variance Events"?

Response: Thank you for pointing out the use of jargon. The term 'variance event' is used to distinguish signal in the time series without (obvious) periodic pattern, but with significant or characteristic excursions from a mean or trend. I suggest to rephrase 'variance events' to help the readerto: '**Events in the temperature signal**...'

Line 9. I suggest replacing the "with the naked eye" with visually.

Response: Thanks for the comment, I agree.

Line 13 – 14. The author states that "the available methods to determine energy and scalar fluxes from terrestrial land surface are relatively imprecise due to a multiscale of irregularities in the land surface and the turbulent transport mechanisms". I think that this is statement is not very clear. This imprecision originates from limitations in the precision of the measuring methodologies or does the imprecision refer to the need for spatially distributed measurements?

Response: The line refers to limitations in the measuring methodologies, which happen to relate to spatiotemporal irregularities in surface-atmosphere interactions.

The methods are precise, just not applicable everywhere and all the time. As a consequence, there is a pattern, perhaps bias, as to where and when energy and scalar fluxes are observed today using micrometeorological techniques. In general, complex terrain is avoided, and periods with stable atmospheric conditions (e.g., night) or hydrometeorological events (e.g., rain, fog) are excluded. This introduces a systematic uncertainty.

Many researchers have asked the question: 'what [are we] missing outside the applicable range of the methodology and assumptions?' (line 19). Spatial distributed measurements may not be the final answer, but I think they could be helpful for the assessment of processes and for the development of empirical methods.

Line 24. An abbreviation should be added after the "roughness sublayer". Later in the document (line 26) is referred to as RSL.

Response: I agree, thank you for noting the omission.

Line 45. What it is meant by the following statement: "the quantification of Tb outside a controlled laboratory environment is a challenge, in and of itself"?

Response: There are many possible sources of interference in thermal imaging, which in a controlled laboratory environment can be observed and corrected for. In field studies this is much more difficult.

Line 57. I recommend that this statement about the goal of this study is also mentioned in the abstract. It will give a clearer idea to a reader about the objective of this study.

Response: Thank you for the comment. I agree.

Lines 58-61. I think that references to previous studies that have used the DTS and TIR measuring techniques should be mentioned. An example is:

Dzara, J. R., Neilson, B. T., & Null, S. E. (2019). Quantifying thermal refugia connectivity by combining temperature modeling, distributed temperature sensing, and thermal infrared imaging. Hydrol. Earth Syst. Sci., 23(7), 2965–2982. https://doi.org/10.5194/hess-23-2965-2019

Please note that I am not neither the author or any of the co-authors of the aforementioned study.

Response: Thank you, I was not yet aware of this study. It appears that the the authors applied the techniques in an aquatic environment. Please note that in their study TIR was used to generate thermal maps (one per season), which were subsequently compared to statistics of the DTS time series. In contrast, this preprint presents an approach where DTS and TIR are both used to produce high-resolution spatial time series.

Lines 65: What does ICOS stand for?

Response: Thanks for noting the omission, the definition is indeed missing. The acronym should be added to the text:

'The study was conducted at the DE-Fen station, Fendt--Peissenberg, Germany, which is a TERerstrial ENvironment Observatories (TERENO) and **Integrated Carbon Observation System (ICOS)** core site …'

Lines 68 – 69: In these lines the author gives details about elements of the landscape surrounding the experimental area. It is not clear how this information is relevant to the study. I suggest that the author explain briefly the impact of the landscape to the experiment presented in the manuscript or remove that part.

Response: I fully agree with the reviewer's suggestion to remove this part.

Line 70: What are the ScaleX campaigns?

Response: I agree that this concept is not properly introduced and suggest the following.

'During **intensive field** campaigns **at DE-Fen**, additional experiments were conducted for the investigation of scale interactions between the atmospheric boundary layer and the surface, as well as validation of measurement techniques (**ScaleX**; …).

Line 74: Why is the period between 18 – 22 Jul 2016 considered as a reference period?

Response: This is indeed not a necessary qualification. I suggest to remove 'reference'.

Line 75: What was the purpose of the UAV use? And how could they have an impact on this study?

Response: UAVs were used for mapping surface brightness temperature at a larger spatial scale and for in situ measurement of wind field and air quality properties. Those studies were also part of ScaleX and referenced in the paragraph text. In some instances, horizontal transects were flown above and upwind of the setup. Particularly the heavy airborne platforms, e.g., carrying a gas analyzer, generated a downwash jet that could be sensed at some distance. Also, some UAV operations required teams of people moving in and out of the field, e.g., for hourly off-site charging of batteries. The flight tracks were recorded in detail, the movement of people and vehicles not.

Line 78: What does EC stand for?

Response: The definition used in the preprint is 'Ultrasonic anemometer (EC)' (line 77). EC refers to the Eddy Covariance technique in which these instruments are used. No changes are made to the text.

Line 78: I think that it would be very helpful for a reader if the author specify that is the figure 1c and table 1.

Response: I fully agree. The references should be updated as suggested.

Line: 82: What was the reasoning for the number of sonic anemometers used, the selection of the locations of the tripods and the heights of the sonic anemometers?

Response: The number of sonic anemometers was limited by available hardware at the time. Ideally, only 3-axis sonic anemometers would be used. A compromise had to be made for the number of 3-axis sonic anemometers deployed here and elsewhere during the campaign. The height of the 3-axis type instruments on the tripods was chosen to be similar to the ICOS station and other studies using the Eddy Covariance technique on permanent grassland (including DE-Fen; see also the study by Mauder and Zeeman cited on line 80). There are sensitivity limitations for working with current model ultrasonic anemometers close to the surface. A level of 2-axis sonic anemometers closer to the ground (0.25 m) was planned but the appropriate mounting hardware could not be arranged in time for deployment.

Also, from the Figures 1 and 2 it is visible that the sonic anemometers were located between the supporting poles of the DTS mast. Could there be any interference to the sonic anemometers measurements acquired during the period selected in this study from wakes generated from the supporting poles?

Response: Small-scale interference in the wake is possible. The distance from the DTS masts to the EC profiles (on tripods) was 3 m. DTS masts had a diameter of 0.1 m. Increasing the distance would have required for the suspension cable to be mounted higher and with larger tension force to keep the steel cable straight. This could not

(safely) be realized during the deployment.

Line 97: Where was the TIR system pointed to?

Response: The TIR system was pointed to the ground at a slanted angle to include as much surface within the DTS box as well as static objects for georeferencing. The guyed lattice mast was planned to be taller, but had to be kept below 10 m for safety of nearby glider planes. This limited our options for the camera viewpoint during the deployment.

Line 101: What is meant that the location was determined in post-processing?

Response: Thank you for the comment. This means that it required a georeferencing step as described in the text below the line and Appendix A. I suggest to change the text to make this clear.

'Each EC, DTS and TIR record was stored with an accurate time stamp and locations were **georeferenced** in post-processing. The calibration and **georeference** details are provided in Appendix A.'

Line 116. The air temperature (Ta) is mentioned here, but it is only discussed how it is measured in Appendix A3.3. I would suggest a brief statement about those measurements also in section 2.4.

Response: I agree. This is an issue and I agree with the suggested solution.

'Reference air temperature measurements were made using resistance temperature devices in fan-aspirated enclosures (Table 1; Appendix A)'

Additionally, regarding Figure 3. What is the sampling frequency of the time series presented in Figure 3?

Response: Those are 1 min averages in all panels. I suggest to change the caption accordingly to better inform the reader.

How is the Tc estimated at the presented heights? Is it the average over all the four sides of the box?

Response: Yes, it is the average over all Tc profiles.

Which sonic anemometer's data is being used in the Figure 3 c?

Response: Only the 3-axis sonic anemometer models provide a measurement for Tv, hence these are data for 3.0, 6.0 and 9.0 m height.

Line 127. The author states that "some turbulence statistics were rarely acceptable … "

What is it meant by the words "some" and "acceptable"?

Response: For the application of the eddy covariance technique, it is currently recommended to perform a number of (self-)validation tests, e.g., based on stability, stationarity and friction velocity. The test results can be simplified as a quality classification for the averaging period. During the nights it was rare to find the quality of averaging period results classified as 'acceptable'.

I suggest to rephrase this to 'eddy-covariance flux computations **rarely produced acceptable results**…'

Line 129-130. It is not clear how what are the assessment criteria used here to assess the quality of the flux computations.

Response: The assessment criteria may be the same, the computation to derive a stability classification is different.

Line 132. How were the temperature gradients calculated?

Response: Thank for noting the omission. This is indeed not mentioned clearly. I suggest to add the definition for temperature gradient to Appendix B.

Line 136. Figures 3a-c allow a visual comparison of the time series. However, there is a lack of a statistical comparison of the different methods (e.g. correlation, mean absolute error). I suggest that the author elaborate more on this part.

Response: I agree. Please see the response to the General Comments.

Line 140. In Figures 4b-c time series of the normalized by the Obukhov length scale height and the friction velocity are presented. Measurements from which sonic anemometer were used for those calculations. Which criterion has by used to assess the atmospheric stability is stable or unstable?

Response: Only the 3-axis sonics can be used for the computation. I suggest to make this clear in the text of the method section.

Line 145. Is it the air temperature or the cable temperature presented in Figure 6?

Response: These panels are derived from cable temperature. I think this is made sufficiently clear from the caption and legend.

Line 189. The DTS measurement set-up has a rectangular shape. What is meant here the mean wind was mostly aligned to the set-up?

Response: Thanks for the comment. I now recognize the wording can lead to confusion for the reader and this aspect should be rephrased. What was meant is that for a period of time the mean wind was either perpendicular or parallel to the walls of the box.

Line 191: How does the animations reveal scale interactions? And why they are note easily identified in the statistical analysis?

Response: Thanks for the comments. Scale interactions are revealed in statistical analysis, as the results show. Nevertheless, it can be helpful and educational to review those results visually.

Line 196: I do not understand what is meant with this statement. Can the author elaborate explain this a bit more?

Response: Thanks for pointing this out.

The surface was not homogenous in terms of TIR signature. Some signal in the TIR image time series were revealed when and where the background (the surface) and foreground (air that had interacted with the surface upstream) show a different heat signature. Therefore, motion was revealed from hot air advecting away from relatively hot areas in the plant canopy, against a background of cooler surfaces. I agree with the suggestion to rephrase line 196.

Line 213. Why is there a sudden jump in the TKE in Figure 11 between 00:00 and 12:00 in 21 jul 2016?

Response: The jump is correlated to the passing of a short storm with brief precipitation (See panels Figure 3f and Figure 4a, and the text at line 120).

Line 228. The author gives a very thorough list of the limitations of the current measurement technique. It would be very constructive if the author could provide a short recommendation regarding in which applications this setup should or shouldn't be used.

Response: Thank you for the comment. A short recommendation shall be added here (see the response to the General Comments).

Line 235. Can the author elaborate more on why three-dimensional sonic anemometers at lower heights would be advantageous in this study?

Response: Thanks for the comment. It would have been helpful to show TKE, w' and stability information, as derived from 3-axis sonic anemometers, at a lower level.

Line 249. How is this precision calculated?

Response: This conclusion refers to results discusses in line 241 and shown in Appendix A). As suggested in the general comments section above, more detail on the performance would benefit the presentation.

Line 250. What is the reference scale for the recommendation for the size spatial domain and what is meant with the "2.5 dimensional or better"?

Response: A setup with a combination of 2-dimensional planes as cross-sections provides more than 2-dimensional information, but less than a 3-dimensional grid. Hence the array is referred to as 2.5-dimensional space. A fully 3-dimensional setup would, for example, resolve locations in a regular grid similar to many fluid dynamics models. In principle such a 3-dimensional setup could be achieved in the field, with some effort, using DTS.

Line 260. Does "turbulence" refer here to wind speed or temperature?

Response: Both.

Line 261. Both here and in the abstract, it is mentioned the development of physics-aware machine learning techniques. The current topic is not discussed in the introduction, so it is difficult to understand what a physics-aware machine learning technique is, assess how this study contributes to their development and understand their potential value. I think that it would increase the comprehension of the manuscript if the author could briefly explain this.

Response: I agree, a brief explanation will be helpful. Please note that the subject is discussed in the results section (lines 244-247).

Line 271. How accurate was the time keeping?

Response: The hardware clock data sheet specifies a 2 ppm accuracy. There was no measurable drift on any of the systems.

Line 285. How did the author recognize the period with winds from the north?

Response: Both the sonic anemometer (EC) network and the wind observations from the

DE-Fen station indicated wind direction. The north wind sector is frequently observed in summer due to the proximity of the Alps to the south. The situation is maintained for several half-hour periods during the day. Assumed was, that wind from this sector would have limited wake effects on any of the sonic anemometers by mast structures or topography.

Lines 369 – 380. Why is this paragraph in the appendix? Isn't this part of the results?

Response: Thanks for pointing this out. Yes, I agree that this paragraph should be in the results section.

What is the physical meaning of the grouping of the clusters presented in Figure A7?

Response: The original study on the TED method discusses the extraction of key variance features from idealized data for each cluster (e.g., a sine wave, or more a ramp shape). As far as I can tell, it did not suggest the same clusters for the application on real-world data, just use of the same number of clusters. This is a shortcoming of a non-supervised machine learning methods. Some outcomes are not easily translatable or transferable. In this preprint we do see that TED clusters can be shown to appear with different spatiotemporal patterns (Figure 8), which I thinks highlights further promise. In order to explore the physical meaning would require the development of an appraoch to reliably aggregate (and/or normalize) data corresponding to each cluster.

Also, what is the impact of variations of atmospheric stability in the results presented in Figure A7?

Response: This is a good question. The preprint does not specifically explore possible correlations between the spatiotemporal patterns in stability (Figure 4) and patterns in TED classes (Figure 8 and Figure A7).

Table 2. What is the reason for mentioning the different ways of parameterizing the atmospheric stability? How is this used in this study?

Response: The different parameterizations of atmospheric stability are used as background information for the reader. I am not sure at this point if or how any of the parameterizations can help improve the classification of turbulence events. Personally, I found the differences between a lower (1.0 m) and higher (3.0 m) location in the gradient intriguing and indicative, without exploring possible explanations.

Figure 1. Units are missing from the x and y axis in all three plots, as well as from the color plot in figure 1a.

Response: Thanks for the comment. I suggest to add text to the caption of Figure 1 to indicate the use of UTM coordinates on both axes and add a unit to the color scale.

Figure 4. Over what time scales the friction velocity has been calculated?

Response: Over a 10 s time scale. This information should be added to the caption.

Figures 3,4 5. I suggest changing the color scale in Figure 3a-c, Figure 4a, Figure 5 a-b, the colors are going to be very difficult distinguished from color blind people.

Response: Thank you for the suggestion. The colors were picked using recommendations for color blindness safe color scales (see, e.g., Colorbrewer by Cynthia Brewer). An online Daltonism simulator reveals a diverging gradient with distinguishable colors between blue/purple and yellow. I suggest to leave this aspect of the figures unchanged.