Reply on RC2

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-AC5, 2021

Response: Thank you for your efforts reviewing the manuscript and for providing helpful comments for discussion. A first response to the comments can be found below.

The manuscript presents a novel approach to combine distributed temperature sensing and thermal imaging instruments to study near-surface turbulence. The new technique enables detailed spatio-temporal analysis of both scale and shape of temperature structures and opens new opportunities to advance micrometeorological research. The manuscript is well written and provides a detailed overview of various aspects related to the application of the new technique. In my opinion, it might make the manuscript more accessible if the data science techniques would be briefly explained in the main text (and not only in the appendix).

Response: I fully agree with the reviewer that a restructuring of the text regarding the data sciences techniques would be helpful to the reader.

It would be also helpful to explain the meaning of variance events since they are at the centre of a part of the analysis. What is their physical meaning?

Response: It is valid to ask what the physical meaning of a variance event is; if there is momentum involved, what processes drive the observed patterns. In this manuscript, a variance event is a significant deviation from a temperature background signal (and noise floor) of less than a few minutes. Their physical meaning can often be explained from context, but not always. For instance, temperature ramp series during unstable conditions have been reported and interpreted from experimental data and fluid dynamics model simulations, often linked to a rolling mode of coherent propagation near the surface. However, single events during unstable conditions can be more elusive in nature. Events coinciding with substantial local destruction of stable stratification may be driven by a non-local process, e.g., waves generated remotely. I refer to AC2 (https://doi.org/10.5194/amt-2020-500-AC2) for an overview of the multi-scale observations that were operated to provide such additional context. Explaining the physical meaning of the events and their impact would be more meaningful when those observations are included in analysis, i.e., following up on this study.

I would also find it helpful if the potential of this technique for long-term monitoring would be discussed. It appears if the instrumentation was only deployed during an intensive measurement campaign. How realistic is it to deploy these instruments year-round?

Response: I agree with the reviewer that a discussion on long-term monitoring would be helpful, but it would be somewhat speculative as it was not the aim of this experiment.

Long term monitoring would be possible. I think this primarily depends on the stability of the support structure used to suspend the fibre-optic cable. In addition, it would be important to prevent accidental damage by animals, particularly wildlife. Precautions can be as simple as increasing the visibility of the set-up during the night with a floodlight and marking the area with bright warning tape. Fibre-optic cable can deteriorate under mechanical stress, but I have not seen evidence thereof based on the used set-up. However, optical cable can be repaired in the field in case of damage or, if the budget allows, can be replaced. A suitable reel of fibre-optic cable cost approximately 500 USD/EUR in 2014. The instruments, particularly the TIR and DTS models used in this study, are designed for long-term (industrial) operation.

Please see below some comments:

Line 17: *Please clarify "about additional details contained in such data". What were the specific research questions that were addressed in these studies? It would also be informative to further elaborate to which research questions the presented new measurement techniques could contribute.*

Response: Thank you for the comment. I agree with the reviewer that the formulation is vague.

There is a tangential connection between this manuscript and the cited studies. These studies aim at separating or computing component scalar fluxes, and hinge on (cross-)correlation structure between scalars. Conditional sampling approaches for heat fluxes (e.g., Klosterhafen *et al.* and the works cited therein) rely on separating 'high' and 'low' frequency variance signal at the height of typical eddy covariance observation, assuming that the isolated scalar signal (water vapour, carbon dioxide, temperature) observed at some distance away from the surface are a result of coherent exchange, which in turn is driven by coherent shapes and scales that preserve the signature of processes at the surface. Here, an approach is presented to observe the spatio-temporal evolution of such coherent structures (temperature). An outlook could be to use that detailed information to improve said alternatives to the eddy covariance technique. That outlook is expressed later in the text, specifically Line 257: 'The ability to trace coherent motion in space and time may proof useful for the development of conditional sampling methods that complement the eddy-covariance technique.'

I think it might go beyond the scope of the manuscript to review the various approaches that are in development to quantify vertical scalar fluxes or derivative variables, in much more detail than the first two paragraphs (Line 17 to 34). My suggestion is to replace 'about additional details contained in such data' (Line 17) with 'about additional details contained **within the scalar (co-)variance data also used by the eddy covariance technique**'.

Line 128: *Which criteria were applied to determine if eddy covariance flux computations were "acceptable"?*

Response: Thank you for the comment. I agree that this needs to be clarified. The eddy covariance computations followed a (self-)validation procedure, e.g., based on tests for stability and stationarity. The tests results are simplified in a classification: Acceptable, Ambiguous or Incorrect. The quality classification is shown in the top three bars of Figure panel 4f. In the text, 'acceptable' indicated that data classified as Incorrect were excluded from the presentation, such as Figure panel 4e. Please note that all computed eddy covariance flux data, correct and incorrect, are included in the support material data sets

and the quality classifications are additional variables.

Following up on RC1, I suggest to rephrase the sentence to 'eddy-covariance flux computations **rarely produced acceptable (classified as Acceptable or Ambiguous) results**…'

Line 156: *Please elaborate how these findings "suggest an interaction between scales".*

Response: I agree with the reviewer that this could be elaborated. The simultaneous occurrence of multiple scales at the same place (height) and time suggests interaction, perhaps cascading energy from one scale to another.