RC1 rebuttal

An Adaptable DTS-based Parametric Method to Probe Near-surface Vertical Temperature Profiles at Millimeter Resolution

The manuscript introduces a reproducible method of developing a coiled-DTS array capable of observing air temperature at the millimeter scale. The authors go through various constraints necessary for a good design and present a material capable of fulfilling these design requirements. A parametric method for developing the DTS coil is developed, from which one can estimate the vertical resolution the array is capable of. The design's vertical accuracy was then verified in lab experiments followed by a field deployment for assessing temperature accuracy and the effect of artifacts. Some exemplary profiles of air temperature were presented. Radiative artifacts were evaluated against a standard reference probe. Extensive documentation for the code and assembly of the DTS coil are provided.

Generally, I think this is a fantastic concept and the manuscript is worthwhile of publication. However, there are a number of items to address first. The writing can be unfortunately repetitive and a general edit is necessary to create a more fluid text. I noted some of these instances. The introduction could do with a bit of reorganizing so that a non-DTS expert can more easily understand the justification of the problem. There are some issues with the description of the lab experiments (e.g., potentially flipped axes, saying experiments will be discussed later and not discussing them). The biggest need is creating a more robust statistical comparison against the reference probes, otherwise the statements being made are too ambitious given the limited results shown. The documentation is extensive and commendable. One thing to potentially add is a piece of code that converts the DTS from LAF to height. I look forward to seeing the revisions as I believe this work is important and provides a powerful method.

We thank you for your peer review on our manuscript. We appreciate the encouraging words on the merit of the method we have developed. Your comments have been extensively discussed and are individually addressed below. If a comment has been highlighted in green, this means that it has been directly implemented in the manuscript without the need for further comment.

2 Major Comments

The paragraph starting on line 57 introduces concepts that a non-DTS expert would need earlier to understand the discussion of previous work. I recommend moving this concept to be much earlier, especially since this paragraph in essence introduces the entire problem and makes the literature review clearer.

We agree with the reviewer that this paragraph needs revision. After some internal discussion we decided to move the introduction of the coil geometry to an earlier section, while giving a short description of the geometry:

"Therefore, adapted, compactly positioned fiber optic configurations were implemented to attain sufficiently high resolution vertical temperature profiles. Fiber optic cable was attached to a net to form a two-dimensional harp-like structure. Additionally, novel configurations have been developed where helically wound coils further increase vertical fiber density (Hilgersom, 2016, Sigmund, 2017, Izett, 2019, Zeller, 2021). These campaigns implemented rigid, vertical frames, around which the fiber optic cable is wound. As a result of these enhanced geometries, resolutions and accuracies up to the centimeter scale were achieved."

Furthermore, we agree that the explanation of the geometrical resolution limitations was convoluted. We changed this paragraph to the following:

The vertical probing resolution is a critical limitation in quantifying temperature gradients. Previous campaigns have achieved resolutions of several centimeters, but capturing near-surface gradients within the upper meter requires sub-centimeter resolution and accuracy. Beyond the inherent spatial resolution of DTS devices, this limitation is largely geometric in nature and can be divided into two aspects. The first is that of **vertical fiber density**: increasing the length of fiber per unit vertical distance enhances resolution. This issue is addressed by the enhanced fiber geometries, such as the aforementioned coil geometry. The second aspect is that of **positional accuracy**: the temperature measurement at any point depends directly on the precise location of the fiber, which requires the cable to be fixed securely in space. Even with increased vertical density, uncertainties in fiber positioning remain a limiting factor. Thus, further increasing fiber winding density in isolation is insufficient; both vertical density and positional accuracy must simultaneously be addressed to achieve meaningful improvements in vertical resolution.

I also recommend expanding the studies that used coiled DTS setups in your review of previous work to better incorporate studies not originating from the same institution (e.g., more directly include Sigmund et al and Zeller et al).
These studies are now more explicitly mentioned and explained

Line 82-84: I was left confused because the cited studies did study the specified media but the sentence suggests these media have not been studied.
We have rephrased this section to more explicitly state that these media have been studied, but not at such high resolution:
“This potentially extends such high resolution observations to applications beyond the grass region towards other media, such as snow (Zeller, 2021), ice (deBruijn, 2014) or water surfaces (Paaijmans, 2008).”

Line 134: Citing a thesis, while accepted, should be done only when strictly necessary. I am certain you did great work in it, but I do not want to read a thesis to understand your manuscript. If the design considerations were not relevant enough to include in the manuscript then I suggest not mentioning them at all. If they are important, they should be discussed, even if briefly, in the text. I do not want to diminish the work you did, but I also do not want to read another document.

The reviewer is right: the present manuscript already contains the full information for the reader to repeat the experiments. Also reference to a master thesis should be minimized as those typically are less accessible. However, we supply the thesis as additional reading material for interested readers. We suggest the following:
We removed the old line, which might suggest that the thesis contains essential material for the understanding of the method, not already described in the manuscript, which is false.
As some readers might be interested to see more photos of the laboratory setup we added a reference in the caption of figure 4: "note that more photo material on the manufacturing and lab-tests is available in ter Horst (2024)."

Likewise we added a sentence in the authors contribution:
"part of this manuscript is based on the master thesis by the first author (ter Horst, 2024). ..."
This is sufficient for the reader interested in further details, while it is not essential indeed for the general reader.

Material selection: It seems like there is a trade off between the materials depicted in gold and blue in Figure 3. The materials in blue minimize thermal conductivity while those in gold have the potential to further minimize heat capacity at the expense of larger thermal conductivity. Could you comment on the reason to minimize one over the other, e.g., how was the ”pareto front” chosen?
Defining a meaningful pareto front, often referred to as the ‘material index’ in ashby plots, is difficult, if at all possible. Relating the two parameters in a mathematical way, such that a slope can be found is not trivial and requires extensive physical modeling. Luckily, in Figure 3 it is observed that all of our relevant non-grey materials align under each other vertically. As such, picking blue material is still defendable for practical reasons here. PMMA comes with many additional practical advantages, apart from its thermal properties. These are described in text and as such constitute a sufficient justification of the material choice, in our opinion.

The parametric design lists coil radius as one of the critical parameters. But, this parameter is also limited by the minimum bend allowed by the fiber. I think including a short warning of that limitation would be beneficial. Experimental validation and discussion
We added:
"Note that the minimum coil radius is constrained by the minimum bend radius allowed by the type of fiber that is used."

Naively, based on the affiliation of the authors of the study, I would assume you are using the ‘dtscalibraton‘ python package. If so, please include a citation for the paper describing the method as well as a citation for the code. Upon further reading I see that I am correct in which case the information needs to be consolidated.
Here we refer to line 276 where we listed the paper by Des Tombe. However, we will add an additional citation to the code, in addition to the paper that is already cited. Furthermore, we specified that it is a python library.

The lab experiment for assessing the vertical accuracy has the water level

decreasing with time in sub panel (c) while the water level appears to be increasing in time in sub-panel (b). Could you please clarify what the axes mean? Additionally, how was the true water level assessed?
The z-coordinate is relative to the coil itself, where z=0 is at the bottom of the coil (the side with the stakes). The coil was positioned upside down in the container, as this was more convenient. We will add a note clarifying this in the figure caption.

The water level is assessed by measuring the start and end level, under the assumption that the rise in water level is linear.

End of section 3.1: A lab experiment for assessing the effect of rain was performed and a later analysis of the experiment was promised in Section 3.3, but no such analysis was presented.
The reviewer is right that no description is given on the results of the lab tests, as the observed artifacts also appear in field data. Describing these observations twice would be superfluous. The initial motivation for mentioning the lab tests is that they isolated environmental factors, making it easier to link them to observed artifacts. This allowed us to more confidently explain our observations in the field. However, we acknowledge that this is not of significant interest to the reader. We have therefore removed all mentions of the environmental lab tests and describe artifacts only in the field observation section.

Section 3.2.1: For describing the field setup it is also necessary to include the separation distance between the observations. How consistent is the grass height between the reference temperature probe and the DTS coil? Please address.
Indeed the reviewer is right here. The grass-height is maintained and mowed at an average height of approximately 10 cm. However, grass is a living material and the 10cm is only approximate.
While we inspected both sites which looked very similar, there is no absolute 'proof' (as for Wimbledon-grass or soccer grass). In the text we will therefore add a disclaimer sentence:

line 288 (at approximately ~ 0.1 m height).

And line 251" The site is well suited for observations as it is located in a relatively homogeneous, mostly agricultural area. with grass at the Cabauw site that is mowed and maintained at approximately ~ 0.1m height (though it is noted here that grass height may vary somewhat and as such the 0.1m is more indicative than absolute).

And in the figure caption: "The grass canopy region is shaded in light green, which should only be considered indicative, rather than absolute (section 3.21.).

DTS uncertainty versus resolution: On line 109 the instrument time and temperature resolutions are specified, but it is known this is different than the instrument accuracy. Later this is recognized through the calibration bath validation, with an instrument uncertainty of 0.13 C. I think the introduction would benefit from highlighting the literature assessing the actual uncertainties and resolvable scales in addition to the manufacturer supplied resolutions.

Although we see the point by the reviewer, we disagree here, that we should address more literature on this, as for the goal of the paper, the present quantification sufficiently supports the conclusions. Adding a lot of extra detail and discussion here would distract from the main message.

Note that apart from an order of magnitude (mm rather than cm as previously) it is not justified to give very hard numbers, as multiple (unknown) sources of uncertainty will play an additional role.

3.2.2 and Figure 5: I am left puzzling if the DTS device is measuring artifacts from the grass contacting the DTS. Could you comment on this either here or in the manuscript?

This is a valid comment. However, it is virtually impossible to quantify. Fortunately we observe no large outliers in the grass layer and spikes such as with the wetted rings. As such we suspect no major role. We will however keep this in mind for our follow up research.

3.2.2: Time-averaging to 30 minutes is a substantial amount of time aggregation. In night time conditions the temperature structures will almost certainty include processes at minute time scales, as seen in Figure 7b. Further, this is a total of 180 observations (correct?), which seems like an unnecessary number of observations needed in order to "reduce noise". I recommend carefully evaluating if this level of temporal aggregation is necessary.

In this case, we choose our data aggregation time on the basis of the KNMI data, which we use for validation. As such, we have to aggregate to half-hour intervals. Though, higher time resolutions are definitely possible with this setup and such large aggregations are not necessary. It would be interesting to look at shorter averaging times and 'times-to convergence' as to study e.g. nighttime intermittency in turbulence, in a style similar to the study of hartogensis et al, fig. 6, who did that for scintillometry.

Boundary-Layer Meteorology 105: 149–176, 2002.

3.2.2: I am quite certain I read much of this material in the introduction.

Indeed, we have shifted some essential information to the introduction and made a reference to the introduction in section 3.2.2.

Figure 8 and 9: Given the logarithmic shape and the fine scale features, I recommend moving these plots to an ln(z) spacing. Further, the variability of the observations should be indicated (I anticipate the variability will be large which is part of the reason I think a 30 minute average is inappropriate).

Indeed, we explicitly considered this in our research group (plotting ln(z)), however, we decided to plot the 'unmodified/raw' data, as this is the most direct way of plotting. Plotting ln(z) plots can also 'hide' uncertainties. Moreover, the main purpose of this paper is to present technical methods rather than in-depth scientific analysis. A more in-depth scientific

analysis will be the subject of a separate paper that is currently under development in our group.

3.2.2: I strongly disagree that extrapolating an observation to a distance 100% outside the fitted region counts as a validation, as suggested in the Figure caption and in this section. If you want to make this statement, I strongly argue for the inclusion of a more robust statistical fit, including uncertainty as well as a statistical test comparing the extrapolated value to the reference observation. In fact, I think I would like to see a statistical comparison generally. The 'statsmodel' in python is a useful package for performing such statistical inferences and tests. I also think a general illustration of the error distribution is necessary.
We agree with the reviewer. The confusion may be caused by our use of the word 'validation', which is too strict in a scientific sense. The extrapolation is much more a qualitative comparison, as it uses a theory/model (i.e. the log-law) in itself. Indeed, the comparison is outside the raw observational range.

We therefore change our text. We delete:
"indicating the validity of the measurement."

We add: "The coil temperature profile is
logarithmically extrapolated to 1.5 m to compare it to KNMI air temperature, as to allow a qualitative comparison (see text)."

Also in the text we modify :
"The validity of the measured profile is underscored by the air temperature measurements done by the KNMI (grey dot), as compared to the coil-extrapolated profile (blue dot)".
To:
"For qualitative comparison it is interesting to extrapolate the profile and compare it to the KNMI observations at standard height (1.5m). This may indicate whether there are large biases in absolute temperatures measured by the coil."

Finally, we changed the plots by substituting the extrapolated point by a dashed line.

As we intend for qualitative comparison only here to inspect large absolute biases a full statistical analysis (apart from those already in the paper Figs 10 on this.) is beyond the scope of the present work.

The paragraph starting on line 311 needs to be re-written. It is currently too informal for a publication. Further, many of the assessments come off as overly confident given the sparsity of information presented.
The sense of informality and perhaps overconfidence is perhaps triggered the the exclamation mark behind the number 80 (line 315) and the 'bold' printing in line 319. In fact this was point of discussion between the authors themselves. So those 2 items will be removed.
We do think that the core of the paper is very innovative in a sense that such in-grass inversions and in-grass temperature dynamics have never been reported before, so that

some excitement in the language is not out of place. But yet, it should be scientific language of course, on that we agree.

We also modify: "Such inversions have qualitatively been observed before at much lower resolutions (Jacobs et al., 1992; Jim, 2011)." which may sound overconfident by:

“Similar inversions have been observed before for taller canopies like maize and forests (e.g. Jacobs et al., 1992; Jim, 2011).”

Section 3.3: There is no mention of wind-mitigated radiation artifacts. Including an analysis on the basis of wind and net radiation simultaneously would benefit the statements being made regarding the accuracy of the system. It also seems like the Sigmund et al., 2017 manuscript could also be cited in this section. Finally, it is stated that radiative effects can be compensated for, but this was not performed here, which seems a bit odd.
We have investigated the effect of wind on the temperature bias, but did not find a significant correlation that was worth presenting.
An analysis linking temperature bias to both radiation and wind simultaneously would be interesting in future campaigns, but remains outside the scope of this research for now.

Indeed this paragraph is a good place to add a reference to Sigmund et al., 2017. It was added in this paragraph.

We have also addressed the line about compensation and changed it to:

"Radiative effects on DTS fibers have been widely studied and corresponding data can give insight into the accuracy of observed temperatures (deJong, 2015). "

Which more accurately represents the role of radiation data in our research.

Line 405-406: It is stated that this resolution and accuracy have never been achieved before, but this was not discussed directly in the text and seems hard to verify in any case. Specifically, part of the motivation for the study was that it is hard to verify what the resolution and accuracy was for other studies. I recommend amending this statement to be consistent with the motivation.
We deleted the part: "which has not been attained before using DTS-based setups.'
Indeed the resolution in the order or 1-2 mm may already show the potential enough. No need to emphasize more than that (in accordance with your earlier comment line 311 paragraph.).

The abstract reads a bit disjointed and could benefit from making the sentences flow better into each other.
We have decided to fully rewrite the abstract, taking into account this comment and that of other reviewers.

Line 10: ”different, identical” I think this sentence needs to be clarified.

Line 34: Zeller et al do use a coiled DTS setup, but they do not specify this is to observe the insulating plant canopies as implied.
Line 52-53: Vertical accuracy is unclear here. I think re-organizing the introduction as suggested could help make the intent clear.
Line 118: I do not believe that you need to make the parenthetical statement.
Line 164-166: I naively would assume that reducing the specific heat would reduce the lag between a temperature change and the change in the signal observed by DTS.
Indeed, this is the case. We added a small note of this for further clarity:
“Reducing the amount of heat stored within the frame diminishes the potential heat transfer to the fiber optic cable, decreasing equilibration time and thus improving response speed.”
Line 181: The sentence starting here needs revision.
Section 3.2.1: Many sentences begin with ”This site” or similar. Please re-write to be less repetitive.
Line 264-266: The sentences are disjointed and incomplete.
Line 319: ”the” is an odd choice for a word to bold.
Line 341: ”In the worst case” and ”sub-optimal conditions” convey the same concept.
Line 417: I am confident I read this statement previously in the manuscript.