Rebuttal EGUSPHERE-2025-6125

We thank the reviewers for their comments. While they are positive about our attempts to quantify the turbulence intensity by means of CT2 with the DTS technique, they are also critical about our choice to work with structure parameters as these make assumptions on turbulence behaviour (specifically Taylor's hypothesis) and the techniques used yield different results. Reviewer 1 provides a number of turbulence arguments why other variables than CT2 should be preferred. Reviewer 2 stays closer to the work done (based on structure parameters) and questions the variability between the used methodologies to quantify CT2.

Thanks to both reviews we realised that we did not frame our study correctly. Through the title and the introduction we gave the impression that we aimed to study the structure of turbulence, whereas in fact the main goal was to compare different ways to determine a turbulence parameter, CT2, with the DTS technique and baseline these against in-situ CT2 estimates with a sonic anemometer and a path-averaged CT2 measurements with a scintillometer. A minor motivation was to learn about the structure of turbulence itself, but as reviewer 1 points out other techniques are more suitable for that. Our main motivation has been to determine CT2 itself. At optical wavelengths CT2 mainly determines the structure parameter of the refractive index, Cn2. There is an increased interest to determine Cn2 spatially both in the horizontal and in the vertical as Cn2 to explore the feasibility of optical sensing and optical communication links. Second, CT2 provides an alternative way to estimate the sensible heat flux from a dry DTS cable. With both a dry and a wet DTS cable potentially both CT2 and Cq2 can be determined and from these the sensible and latent heat fluxes can be estimated. Currently gradient measurements are used to determine fluxes from DTS measurements, which are much more difficult to measure accurately and the link of gradients to fluxes through Monin-Obukhov Similarity Theory (MOST) is less robust compared to structure parameters.

The current study explores the various ways to determine CT2, based on temperature variations over both space and time temporal and through spectra versus structure functions and we discuss the (in)consistencies of these approaches. The overall goal is to determine a state-of-the-art for CT2 estimation using DTS. AMT is an appropriate journal for this kind of technical study that explores a new CT2 estimation methodology.

To better frame our paper along the lines given here we rewrote parts of the introduction and changed the title to: **"Fiber-Optic Distributed Temperature Sensing to quantify the structure parameter of temperature, CT2, over space and time: A feasibility study"**

In our response to the reviewers comments we show the reviewers' comments in blue and place our reply in black below. The main line in the rebuttal follows the arguments given above.

Reviewer #1

GENERAL - POSITIVE ASPECTS

“This manuscript evaluates DTS derived temperature structure functions relative to baseline observations from sonic anemometers and a scintillometer. The authors present four ways of calculating the structure function which are equivalent when the assumption of ergodicity is valid and for the inertial scales: one spatially-explicit, one temporal using ergodicity, and two using spectra along space and time dimensions. The methods are evaluated over a flat experimental site using a harp of DTS observations along a 70 m transect. The authors find that the DTS-derived structure functions are biased relative to the baselines, but evaluates well enough to be used as a sensor for small-scale turbulence. The methods are robust and well-presented and I am happy to see someone attempting to get relevant turbulent quantities out of DTS data. The approach is interesting!”

Both reviewers recognize that estimation of DTS based turbulence parameter CT2 and its comparison against scintillometer measurements is novel and interesting. We thank both reviewers for pointing this out.

GENERAL - CRITICAL REMARKS

“However, it is not well justified why we care to investigate such small-scale turbulence using a device like DTS, and thus it is not clear that deriving the temperature structure function is a useful step. One of the major benefits of DTS is that we are not limited by temporal sparsity (e.g., LIDARs) or spatial sparsity (e.g. point observations) all at a relatively high spatiotemporal resolution. Thus, we do not need to invoke Taylor’s Hypothesis. Structure functions, and similar statistics, are what I will refer to as a “uni-scale” approaches: they collapse higher-dimensional and multi-scale information. Uni-scale approaches aggregate over scales and ignore the simultaneous information in space and time. This collapsing of dimensions is an unfortunate necessity when we have sparse observations but not for the highly multi-scale information from DTS.
This is not to say that deriving the structure function is not without value, but in my humble opinion we should not seek to cram DTS observations into the uni-scale approach necessary for traditional statistics and theory and instead embrace the multi-scale nature of the data we get from DTS. The authors thus need to address two issues. (1) What is the value and meaning of a baseline that is only valid when classical assumptions are met? (2) What is the value and meaning of uni-scale methodologies for high-dimensional data?”

As stated in our general introduction to this rebuttal, our main goal is not so much to investigate small scale turbulence as it is to determine CT2 as a variable of interest in itself; e.g., for optical sensing and communication. Additionally to explore an alternative means, with respect to the currently used gradient method, to determine the sensible flux with DTS measurements. We acknowledge that our framing was ambiguous, this point has now been made much clearer in the introduction.

Concerning the uni-scale approach, we argue that this issue is not as black-white as the reviewer portrays it. True, in-situ techniques need a time-series long enough to determine a statistically stable CT2 estimate (say between 10-30min) and they do not provide any CT2 information in space. The DTS provides data in space and time. For ideal turbulence measurements we would like that these represent point measurements in space and time, but the actual DTS measurements represent temperature averaged over a section of the optical fiber cable, typically ~25 cm. This complicates the evaluation of CT2 with the 4 methods that we explore (spectral attenuation at higher spatial or temporal scales, having to include a path averaging correction, etc). To make the CT2 estimates statistically robust, some additional averaging in space and/or time is needed. Hence, the spatial CT2 estimates are also averaged over time and the temporal CT2 estimates are also averaged over space. To facilitate the CT2 validation against both the sonic and the scintillometer the spatiotemporal estimates are space-averaged over the entire DTS line (same as the scintillometer) and time-averaged over 30min (same as the sonic).

In future studies, taking the lessons learned from this work, it is indeed worthwhile to explore what are optimal, minimal spatiotemporal timescales that can be achieved for DTS based CT2 estimates. The present setup did not allow us to do this.

SPECIFIC - CHOICE OF BASELINE

The citation the authors use of Cheng et al., 2017 explicitly uses DTS to question the validity of Taylor's hypothesis in the inertial subrange and thus undermines the choice of baseline. At larger spatiotemporal scales the data (e.g., Figure 3) clearly show that Taylor's hypothesis would not be valid for the entire period, even if it is valid for the sub-period. Thus, what is the value of the baseline evaluations that invoke Taylor's hypothesis? I encourage the authors to consider the possible drawbacks from invoking a questionably valid hypothesis as the baseline.

As we compare with a sonic anemometer, we are inherently connected to Taylor's hypothesis. Since we have spatiotemporal data, we find our symmetrical approach of applying the same analysis over both time and space a useful (and novel) approach. The common baseline is that we can use CT2 to directly compare DTS with the sonic anemometer and scintillometer and this comparison is one of our main aims. We see that Cheng et al., 2017 questions the validity of Taylor's hypothesis and that this reflects in the results between DTS domains. We do think the baseline we use is suitable for our purpose, as the validity of Taylor's hypothesis is not our main research question.

We want to elaborate on the distinction between the spatial and temporal evaluation of CT2 below.

For the temporal evaluation (both with DTS and with the sonic), we indeed assume frozen turbulence over the artificial spatial separation, r, which we take as r = 2 m or r = 5 m and is approximated by U x dt. In addition, we assume that the advection wind speed of the frozen turbulence, U, is represented by the mean wind speed over 30min, such that for each 30min we can use a fixed time-separation, dt, to evaluate CT2. Ideally the period would be shorter, but we find that we need this timespan to resolve the DTS temporal spectra (which we elaborate on in the minor comments section). As the reviewer points out, the assumption for frozen turbulence is not valid for this full scale, but it is a conventional assumption. The true frozen turbulence assumption is for the distance r, which in terms of time relates to an order of seconds and falls well in the inertial range. The variability shown in Figure 3 affects the assumption that a fixed dt can be used based on the average windspeed over 30min. Note that Figure 3 doesn't show the true variability in the wind vector U. It shows the wind component orthogonal to the DTS line, and thus also depends on variability in wind direction.

For the spatial evaluation (only with the DTS) no frozen turbulence assumption needs to be made. The structure parameter, a spatial statistic of turbulence over distances that fall in the inertial range of the temperature spectrum, is determined spatially over distances that we assume are in the inertial range ("definition method") or are shown to be in the inertial range ("spectrum method"). Both spatial methods are independent DTS results and do not require Taylor's hypothesis.

The authors need to better motivate what additional value the evaluation of the structure function offers over the existing evaluations they mention. Most of the issues in the error of the structure function seem to be regarding the smallest resolvable scales and under-sampling, all of which were already known. What makes the structure function evaluation distinct and necessary?

As mentioned already, CT2 is a parameter of interest in itself (e.g., optical sensing or communication) and it provides an alternative way to estimate fluxes from DTS measurements. In addition, it is a parameter that quantifies the turbulence intensity. Current methods for estimation CT2 are very costly (scintillometer) or limited in space (anemometer), all good reasons to test the capability of the DTS technique to estimate spatiotemporal CT2. This is a novel approach, it has not been investigated yet.

SPECIFIC - IMPOSING UNI-SCALE ASSUMPTIONS

I question the value of deriving a measure of small-scale turbulence in a spatially-explicit fashion that is only valid in the limit of existing theory. We, as a field, like to focus strongly on representing the smallest scale turbulent fluctuations because that is where our assumptions are most valid. I argue that the novel value of DTS is that it gives us information that are missed by traditional observational platforms, explicitly at the scales excluded in Figure 9, without the need to invoke questionable assumptions. The major, open questions in turbulence are related to multi-scale interactions (Jimenez 2018, Stiperski et al., 2025), in particular at larger scales at which similarity theory is less valid. These scales are uniquely observable with DTS.
I suggest the authors examine approaches like from Zeeman 2021 in which a method that is not uni-scale is presented. The results from that study also place a limit on the smaller scale eddies that are resolvable but then push into the rich information contained in DTS that is missing in something like a sonic anemometer.

This is all very true, with DTS turbulence measurements you can do other types of analyses than what we have done, but those are beyond the scope of what we had in mind with this paper. We are to blame here by using an ambiguous title ("...to quantify turbulence over space and time…") and not delineating clearly enough what is the goal of this paper. In our revision, we have changed the title and added a section to the introduction to clarify this.

The existing DTS studies largely evaluate DTS observations at a point or over very short spatial distances. This is an fantastically interesting gap in the literature that could be filled by considering larger separation distances! If Taylor's hypothesis is not valid at certain wind speeds and you have a definition of turbulence intensity that does not need to invoke it and you have continuously varying separation distances, you can explore something that could not be measured before!

Indeed, however, in this paper we really want to focus on CT2, albeit not a scope as rich as other approaches that the reviewer invites us to explore. This could be the scope of another study. Above we already argued that the Taylor hypothesis assumption is not as much of a limitation as the reviewer suggests.

In essence, I challenge the authors to transform their paper from a simple methodological evaluation that invokes questionable assumptions to one that challenges the limitations of traditional approaches. The disagreement between the structure function from the traditional observation platforms are potentially the most interesting aspects of this study and one which could help transform the use of DTS.

A "simple methodological evaluation" is exactly what is the goal of this paper. This is also why we chose to submit our study to this particular Journal, which focuses on measurement methodologies. If it were, as the reviewer would better like it to be, a paper that uses DTS turbulence measurements to reveal new insights on turbulence, we would have submitted to a different type of Journal. This first "simple methodological evaluation", that has not been done before, will open the path for future, more research-oriented, applications.

MINOR - TECHNICAL DETAILS

Section 5.4: I think every point mentioned has already been discussed in other manuscripts and thus does not bear repeating here. I know it is frustrating that we all seem to rediscover that same issues with each DTS experiment.

We agree that these recommendations are not unique. Nonetheless, these 'lessons learnt' do impact our results and therefore need to be mentioned. Also to avoid that potential follow-up researchers have to rediscover similar issues. However, we did change the structure of the Discussion Section (see next page) and linked the lessons learned more explicitly to our findings.

References

Stiperski, I. et al. Open questions in atmospheric turbulence: A synthesis from the centenary workshop "100 years of turbulence: Innsbruck 1922 -2022". Journal of the European Meteorological Society 3, 100022 (2025).

Jiménez, J. Coherent structures in wall-bounded turbulence. Journal of Fluid Mechanics 842, P1 (2018).
Zeeman, M. Use of thermal signal for the investigation of near-surface turbulence. Atmospheric Measurement Techniques 14, 7475–7493 (2021).

Reviewer #2

GENERAL - POSITIVE ASPECTS

"The authors evaluate the feasibility of the distributed temperature sensing (DTS) technique for doing spatiotemporal observations of turbulent flows in the atmospheric boundary layer. They cross compare temperature structure parameter (CT2) values derived from the DTS measurements against values derived from sonic anemometer and scintillometer data.
Instrument cross-comparison studies are essential for establishing confidence in measurements; however, the novelty of such studies diminishes once they have been repeated several times. DTS observations have been compared against sonic anemometers already in several published articles and hence in my view such comparisons do not warrant a publication. However, turbulence statistics calculated from DTS data over space have not been compared with reference instruments before and the comparison against scintillometer is the main novelty of this study."

Both reviewers recognize that estimation of DTS based turbulence parameter CT2 and its comparison against scintillometer measurements is novel and interesting. We thank both reviewers for pointing this out.

GENERAL - CRITICAL REMARKS

"The manuscript lacks clarity in places and reasoning is partly difficult to follow. In my view major revisions are needed before accepting this manuscript for publication. Please see more details below."

The manuscript has been screened for clarity. Both following the reviewer's detailed list of minor comments, for which we are grateful, as well as a thorough re-read by all the authors (see track-trace version of the manuscript).

Several major points concerning the method have also been revised and clarified. These points will be addressed pointwise below, based on the reviewers comments.

Additionally, to improve clarity we decided to revise the structure of the discussion, so that it is more streamlined to the main issues of noise and sampling rates.

The previous structure was
5.1 Underestimation and sampling rate
5.2 Offset and noise
5.3 Height and length scales
5.4 Calibration and experimental design

The new structure will be
5.1 Sampling rate
    This will contain mainly instrument and experimental specific points. Arguments made in previous sections 5.3 and 5.4 will be mainly housed here.
5.2 Signal to noise
    This will be split into a section about signal (why is our signal limited and how could more signal be obtained?) and a section about noise (what are our main causes of noise and how can it be limited in the future?)

SPECIFIC(1) - SONIC vs SCINTILLOMETER

Like stated above, the main novelty of this manuscript is in the comparison between DTS spatial statistics and scintillometer results. Hence, I argue the emphasis should be put on that comparison instead of comparing DTS temporal data (time series in a fixed location) against sonic anemometer data. Currently, scintillometer results appear only in one figure and the text revolves around the DTS vs sonic anemometer comparison. The focus should be put on spatial statistics and comparison against scintillometer.

Our aim for this study has been to demonstrate the application of DTS for the first time for estimation of the structure parameter CT2. We have clarified this framing more explicitly now by revising our Title and Introduction. In this context, both the spatial and temporal estimation of interest, this is why we present them both. We see great value in comparing the DTS against both the sonic and the scintillometer. The scintillometer provides spatial CT2 estimates over the same path as the DTS, using light-intensity fluctuations in a laser beam and relating these, through a scintillation model, to the structure parameter of the refractive index, which at optical wavelengths is most sensitive to fluctuations in temperature and can therefore be related to CT2. The sonic on the other hand provides measurements at a time interval of 0.05s and each measurement takes a fraction of that time interval. So they represent “ideal” turbulence measurements. This allows us to evaluate CT2 with the same methods (structure function definition (or SOSF) and spectra) as the DTS and, by evaluating against both methods, to separate methodological aspects from measurement aspects of the DTS estimates. We emphasized this reasoning in the introduction and added the scintillometer timeseries to our analysis in the main text.

SPECIFIC(2) - SOSF vs SPECTRUM

Second-order structure function (SOSF; numerator in Eq. (1)) and power spectrum contain the same information about the flow, but expressed in different domains (structure function: spatial or temporal domain; power spectrum: wavenumber or frequency domain). Hence, I argue that the CT2 values obtained from Eq. (1) or Eq. (2) in the manuscript should be equivalent with each other. If they are not, then this first and foremost tells something about the algorithms you use in the CT2 calculation, and it is not necessarily linked to instrument performance. This should be acknowledged throughout the text. In addition, I suggest you add some figures showing the SOSF since they are underlying the “definition method”.

Figure 10 shows CT2 based on the SOSF definition and from power spectra from both the DTS and sonic data. Looking at the results from the “ideal” sonic data, we see indeed that CT2 estimates based on SOSF and spectra give the same results, or at least similar enough to confirm that the used algorithms are implemented correctly. This leaves instrument performance of the “non-ideal” DTS data (influence of noise, data not representing point data but some average over space and time, etc.) as the main culprit for the differences with the sonic CT2 as well as between the DTS CT2 estimates based on different algorithms.

We find that for the spatial DTS results there is equivalency between the SOSF and spectral method (Figure 10b). For the temporal DTS results this is much less the case. Here we note that the spatial DTS methods do not require Taylor’s hypothesis, but the temporal methods do. This dependency on a bulk wind speed, along with our averaging algorithm, is a likely cause for this discrepancy. Both points we will address in the discussion of a revised manuscript.

We do not really see the added value of showing SOSF in addition to CT2, which in essence is the SOSF scaled by a fixed number.

What we take from the points raised by the reviewer is that we do not adequately discuss our results, in particular:

1. Acknowledge the obvious, i.e. that CT2 from spectra and SOSF definition yield the same result and that this confirms that the algorithms used are correct
2. Having established point 1, provide a better explanation how the DTS limitations explain the found difference with the sonic but also between the algorithms.

Last, we like “SOSF” as a method indicator for what we now call the “structure function definition method” or “definition method” in short. We have changed this throughout the revised manuscript so that the methods will now be denoted as t,SOSF, t,SPEC, x,SOSF and x,SPEC.

SPECIFIC(3+5) - NOISE

SOSFs are also contaminated by noise, but this is not taken into account in your “definition method”. I argue that you should first estimate the noise variance e.g. by estimating to which value SOSF approaches when the separation distance r (or time separation Dt) approaches zero and subtract the obtained value from Eq. (1) before estimating CT2. This should be done separately for each 30-min period you analyse. To be exact, SOSF at r=0 gives you the noise variance multiplied by 2, this is straightforward to show for uncorrelated white noise. Hence, consider the noise also in the case of “definition method”. This likely explains why e.g. in Fig. 10 CT2 values derived using Eq. (1) are never close to zero.

This is a nice idea! Evaluate CT2 for a range of r and extrapolate to r = 0 m to quantify the noise. However, given the fact that a) we measure close to the ground (z = 2m) in line with scintillometer CT2 measurements to estimate fluxes, so r=2m is a soft upper limit in estimating CT2 and b) the spatiotemporal resolution of the data (1s and 0.127m) combined with the fact that these do not represent point measurements but are a spatiotemporal average (~1.2s and ~0.35m) limits the range of r for which CT2 can be determined in this setup. Given these constraints we were happy to even have one value of r to evaluate CT2 with. To achieve this one value we even had to put an upper limit to the wind speed. Interesting as it is, this approach is not feasible given the constraints of our set-up. We included this approach in the discussion to be used when estimating DTS based CT2 at larger z.

We agree with the reviewer that it is likely that the reason the CT2 values for several DTS methods never reach zero is due to noise and/or algorithm limitations. We have rephrased the terminology around this lover limit to reflect that.

Somewhere in the text you need to describe the noise level of your DTS measurements and compare it to the actual temperature variance, i.e. you should report signal-to-noise ratios. You could estimate the noise from the SOSF (see comment 3 above) and the actual temperature variance from the sonic anemometer data. This is crucial for assessing how much your observations are affected by noise.

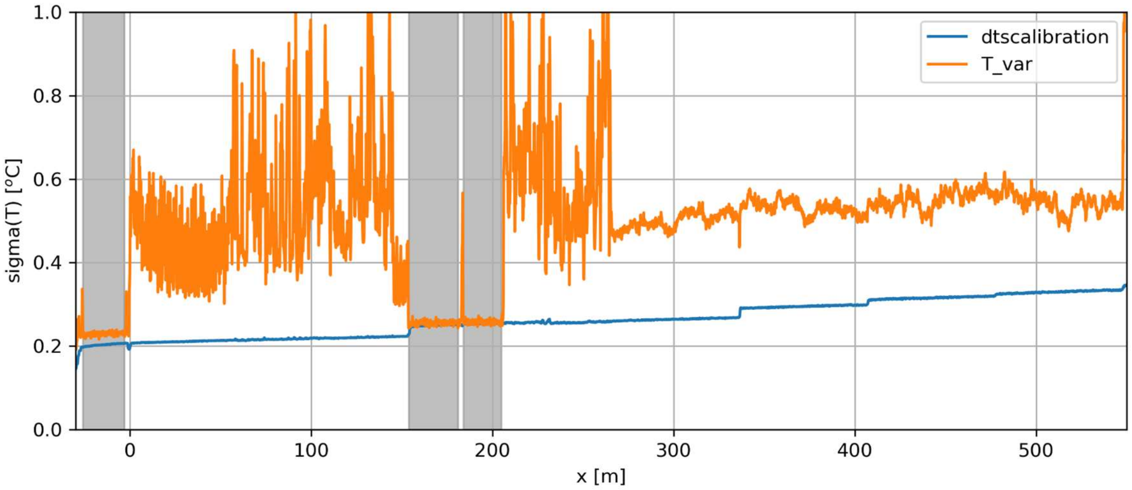
We fully agree with the reviewers that reporting on noise levels and SNR ratios is currently lacking and is essential to include. Below we show a figure illustrating the uncertainty calculated through our calibration procedure (labeled ‘dtscalibration’) along with the standard deviation (sigmaT) of the actual temperature measurements (T_var). The data used is for the same 30 minute period that is used in the manuscript as example period. The grey areas mark the internal coil and the calibration bath sections. The temperature measurements at z = 2.05 m used for the CT2 calculations are located at x = [265,335] m.

Calibration was done using a WLS single-ended routine, with the uncertainty based on linear error propagation of the signal variance and verified by Monte Carlo estimation. The uncertainty was decomposed into contributions from parameter estimation and noise in (anti)Stokes measurements. From this it was determined that essentially all our uncertainty is measurement noise, hence we take these uncertainty values as noise values.

The calibration results show instrument measurement noise of 0.24 K (at x = 0), which matches the expectations from our DTS machine (2 km variant Ultima S), as shown in previous experiments (Schilperoort 2022). The noise increases with 1.16e-4 K/m along the cable. One cable splice is present at 150 m, to connect the 1.6 mm routing cable to the 0.5 mm cable required for the CT2 measurements, which increases noise by 0.025 K. Additional step increases are visible - these are cable bends in the turbulence harp, to transfer the cable to different heights. They do not affect the measurements at z = 2.05 m.

This results in SNR values of around 2-3 for daytime and 1-2 for nighttime conditions (not shown) . This matches with for example the criterion of SNR > 0.5 used in Peltola 2021, although here autocovariance of the DTS signal is used (as was suggested by the reviewer in the comment above) rather than noise estimation from calibration.

We included the analysis of instrument noise values and SNR in the Discussion section of the revised manuscript.



SPECIFIC(4) - TIME vs SPACE

It is unclear why power spectra calculated over time and space are treated differently in this manuscript and the treatment of the spatial spectra should be rectified in the manuscript. For temporal spectra, noise floor is first subtracted from the temporal spectra and then the high frequency attenuation is accounted for with a transfer function before estimating CT2 from the inertial subrange. To me this approach makes sense, since you are trying to recover the unattenuated, noise-free inertial subrange from the observations and the noise sits on top of an attenuated signal. However, the treatment of spatial spectra follows a different approach which is difficult to understand, and I argue to be false. The authors low-pass filter the spatial spectra and argue that this filtering recovers the inertial subrange. Why would you apply an additional filter to a signal that is already attenuated and contaminated by noise? I argue you should follow similar approach as is done for temporal spectra, i.e. first remove the noise floor and then try to recover the inertial subrange with the inverse of the low-pass filter and not apply an additional filter to the spectra. If this suggested procedure does not recover reasonable inertial subrange in the spatial spectra, then there might be artefacts in DTS spatial measurements or peculiarities in the flow which are worth discussing. One should also note that there are studies that argue that the eddy advection velocity depends on scale (Higgins et al., 2012; Cheng et al., 2017; Hilland and Christen, 2024) which suggests that the conversion from frequencies to wavenumbers is not linear. This means that power spectra calculated over time from sonic anemometer data is not a perfect point of reference for power spectra calculated over space when standard Taylor's hypothesis is used to convert frequencies to wavenumbers.

The treatment of the spatial signal is different because temperature measurements taken by DTS are not point measurements but (similar to a sonic anemometer) have a finite length scale. The temperature is sampled every 0.127 m along the fibre optic cable, but with a more limited spatial response of 0.35 m because of dispersion in the fiber and laser pulse length, as illustrated in Figure 5 of the original manuscript (Section 3.2). As a result, adjacent spatial temperature measurements are correlated with each other; the effective length of a temperature response is approximately 35 cm (termed measurement length L in the manuscript). This warrants a different treatment of the spatial temperature signal than for the temporal signal. Further, fluctuations below the scale of 0.35 m are averaged out, causing a decrease in the spatial spectrum at highest wavenumbers. The result of both these effects is an enhancement (followed by a decrease at highest k) of the spectrum visible in Figure 7 from k = 2 and higher. This enhancement is present for all conditions, i.e. also at nighttime. To correct for the effect of spatial smoothing, a low-pass filter is applied, but now as a spatial filter (termed Hx in the manuscript) with the cut-off length taken as five times the spatial response (5L). The choice of this length (5L) is a compromise between ensuring all correlated data is omitted while not exceeding the maximum measurement height (2.05 m).

We agree that using a similar approach as for the temporal spectra would be ideal as it would make the overall methodology more consistent. We have attempted to do this, however we ran into issues with this method being unreliable. The combination of the relatively high noise floor (compared to the temporal spectra), the spatial smoothing inherent to spatial DTS data and the presence of the poles made the detection of the -5/3 slope very prone to error or failure. Therefore we chose to apply this somewhat less fundamental, but working approach. We agree that this is worth discussing, as this indeed means there are artefacts in the DTS spatial measurements. We also realize that the description of this alternative method is ambiguous in the manuscript, which is why we revised it.

We do not expect the eddy advection velocity to play a significant role, given the limited range of length scales used in the study (r = 2-5 m) at z = 2 m.

MINOR

L13 Please specify in the text what is meant with crosswind

We define cross wind as the wind component perpendicular to the harp. We added this to the text.

L15 Underestimates what, please specify in the text

"The spectral DTS underestimates the CT2 by a factor 3-4..". This has been added to the abstract.

L16 At this point the reader does not know what "definition method", please adjust the text

In L10-11 we say that we determine CT2 both spatially and temporally through its definition as well as through the temperature turbulent spectrum. We will add here for clarification the method names: SOSD method and spectral (SPEC) method.

L16 Please add unit for 0.04. This offset is likely due to the fact that you do not consider the noise in the definition method, see major comment 3 above.

Thank you for indicating this omission. We will add the unit K2 m-⅔ to L16, but also to Figures 10, 11, 12b, A2, A3 and lines L288, L342 and L427 and abstract.

L23 “convective turbulence” are you claiming that turbulence produced by shear is not transporting heat, e.g. in a weakly unstable situation? Please adjust

We meant to say that convective turbulence is the most efficient transport principle for sensible heat, which is linked to the temperature fluctuations central in this paper. For clearness, we will adjust the sentence.

L25 “However, this is currently not technically feasible.” DTS has been used for atmospheric flows already from 2012 onwards (Thomas et al., 2012), please adjust the text.

We agree that DTS has been used before for atmospheric flows, but less in 3D space plus time. We changed this to “However, only recently methods that allow capturing 4D spatiotemporal turbulence have been explored”. An additional reference has also been added.

L79-80 If it does not appear later, please add typical Bowen ratio or evaporative fraction + midday sensible heat flux during the campaign. This helps in evaluating the magnitude of typical heat fluxes and corresponding T fluctuations. Also, you need to describe what were the typical turbulence intensity levels (sigma_u/U) during your campaign, since you rely on Taylor’s hypothesis in several locations and the hypothesis is not valid in highly turbulent conditions.

We added the mentioned variables that describe the meteorological conditions during the experiment.

Fig. 3 caption: Add information on how far the DTS locations shown in subplot d were from the anemometer. Fig. 3d looks like that DTS and anemometer were not sampling the same eddies. Compare e.g. with Fig. 8 in Peltola et al. (2021), there you can see clear correspondence between DTS and sonic anemometer since they were colocated. If you plot the time series like this, the reader is expecting to see similarities between the time series.

In Figure 1 the location of the sonic anemometer with respect to the DTS harp is indicated; the distance between DTS point B and the sonic is 27m. For our study, we do not compare DTS and sonic on individual eddy level, we only compare a turbulence statistic. Relevant here is that the footprint of the turbulence measurements is the same. Given the low installation height (z = ~2m for both sonic and used DTS line) and relative proximity of the measurements it is fair to assume that their footprints match.

Fig. 4 I argue that this figure is not needed, consider removing it

We removed it from the manuscript.

Fig. 7 Consider adding the individual ensemble members also in this figure, similarly as in Fig. 6. It is a nice way of illustrating the variability between locations/time steps. Dotted black line in the figure is the inverse of Hx given in the text, please correct. Hx given in the text should increase with k, black dotted line decreases.

We added the ensemble members to Fig. 7. Initially, this was not done so as not to overcrowd the figure, but we agree that it is a useful addition. The error with Hx (and Ht) has also been resolved in Equation 5 and line 240.

L217 usually H is used for transfer functions, this is inverse of transfer function. For clarity I suggest adjusting

We adjusted equation 5 and the equation in L240 so they express first-order LPF transfer functions directly and adjusted the text referring to H and the inverse of H accordingly.

L231-232 Why not to remove the data from the pole locations? And then replace with e.g. linear interpolation

For the time series analysis we did remove all the data within 4 sample points (50 cm) of the pole (L106). For the spatial series this was not done, as this would make the data non-continuous.

We chose to omit data points (for the temporal analysis) rather than interpolate to make sure we only use actual temperature observations to test for the -5/3 slope. The points are omitted after creating the spatial spectrum, as this made the spectrum easier to determine than using a non-continuous FFT.

L236-237 I argue that this is false. Noise is an additive component in the measurements that is on top of the attenuated signal and hence noise should not be attenuated as is proposed here. I argue that you should see similar flattening in the spatial spectra as you see in the temporal spectra. Please adjust the text. This relates to the major comment 4. Only if the DTS instrument spatially smooths the primary observations after the measurement (e.g. running mean over space) you should see attenuation of noise. Indeed, this is exactly what happens, the DTS temperature signal is path-averaged over a section of optical fibre cable. See our explanation at the earlier review point SPECIFIC(4) - TIME vs SPACE.

L253-254 This relies fully on Taylor's hypothesis and further assumes that the same advection velocity applies at all scales. There are studies that argue that the advection velocity is scale dependent (Higgins et al., 2012; Cheng et al., 2017; Hilland and Christen, 2024). This should be considered throughout the manuscript. We do not have a solid understanding on how to accurately link spatial and temporal scales in the flow. Taylor’s hypothesis provides a handy tool for this, but its limitations should be acknowledged.

We do not expect the eddy advection velocity to play a significant role, given the limited range of length scales used in the study (0.35 to 2.05 m). We added a comment on this aspect about Taylor’s hypothesis in the discussion.

Fig. 9 Good illustration, however relies fully on Taylor's hypothesis. Eq. (4) describes how fPA is calculated and based on that, fPA does not depend on U. Please clarify why the blue dotted line is slanted in this figure. "Scales too large to capture" is misleading. I argue that fluctuations at these scales are captured with your DTS setup, since you had 70 m long measurement section. 70 m is ten times 7 m and hence you can observe ten of this scale of eddies with your setup at the same time. However, these scales likely start to be outside the inertial subrange since they are clearly larger than z. Please adjust the text so that it is clear that fluctuations at these scales are captured with your DTS, but they start to fall outside inertial subrange and hence not usable for estimating CT2.
We agree that ‘scales too large to capture’ is misleading. The scale is indeed too large, but this is because it falls outside of the inertial range, rather than that it cannot be captured. This has been adjusted in Figure 9, its caption and the related text.
In the expression of Eq.4, fPA indeed does not depend on U. However, when concerning time series, the expression does depend on U, as the measurement length L changes with wind speed (L = t_s*U, with t_s sampling time). We mention this now explicitly both at the introduction of Equation 4 and at Figure 9. For the spatial series fPA is independent of U and large enough to be relevant (for r = 2 m and L = 0.35 m, fPA = 1.16). Therefore it is not apparent in Fig 9 and also was not mentioned in the text.

Fig. 10 Explicitly mention that the sonic curves are the same in both subplots and derived over time domain, otherwise confusing (you cannot estimate spectra over wavenumber from sonic data). Please add scintillometer results into Fig. 10b or clearly describe in the text why this is not possible.
We added the scintillometer data as a third reference method to Figure 10. Additionally, we will point out in the caption that the three reference time series are the same and that the sonic data is derived over time.

L277 Please remind the reader that the time scales related to the length scales r= 2m and r=5m are estimated with the Taylor's hypothesis separately for each 30-min period. Otherwise this is very difficult to understand. “TimeDef and SpaceDef results” TimeDef is in Fig. 10a and SpaceDef in Fig. 10b, right? Please adjust the text, now it reads like these both are in Fig. 10a

We repeated the length scales and refer to Taylor's hypothesis for clarity. However, it is not correct that Figure 10a only shows the TimeSOSF-results (formerly named TimeDef). It also shows the TimeSPEC in blue. Similarly, in Figure 10b we show both the SpaceSOSF (formerly named SpaceDef) and SpaceSPEC (in blue). We agree that the references to the subplots are not clear, therefore we removed those to avoid confusion.

L278 "TimeSpec and SpaceSpec results" Similar comment as for TimeDef and SpaceDef, please see the previous comment. Please fix

Adjusted as requested, see previous answer.

Fig. 11 I suggest adding a comparison between sonic and scintillometer for giving a point of reference. Such comparison is given in the Appendix, but I suggest moving it here.

We moved Figure A3 to the main text.

L292 Why wind speed would limit the applicability of SpaceDef or SpaceSpec? Please adjust the text

As can be seen in Figure 9 the applicable range of our spatial method depends on the wind speed. We added this information to the text for clarity.

L301-302 You have estimated the fiber time response and you account for this in your calculation procedure. Please clarify.

In the manuscript we use the terms temporal (and spatial) response to indicate the effective size of a measurement. So we have a sampling time (t_s) of 1 s and a response time (tau) of 1.18 s (measurement+processing) and a sampling length (x_s) of 12.7 cm and a response length (L) of 35 cm.

This response time tau does not take into account heat transfer between fiber and air through conduction and convection. Which makes it a different metric than the time response from e.g. Thomas 2012, while they are unfortunately called the same due to our naming convention.

However, using Thomas 2012 (Equation 4) we can also estimate the thermal response time of our cable, which is dominated by conduction. For our cable, with a diameter of 0.5 mm and an estimated thermal diffusivity of 1.5e-7 m^2/s (mainly from our acrylic mantle) we estimate the response time as 1.67 m/s. Assuming Taylor's hypothesis and taking a typical inertial range eddy size of 3 m, we then arrive at an upper limit of 1.8 m/s for advection speed. This corresponds with our limit for strong crosswind, denoted by the red data points in Fig 11.

We included these values along with the reference to the time response of Thomas 2012 in the revised manuscript.

L320-321 CT2 should be constant with f in the inertial subrange. Please correct the text

We removed this sentence, as besides this error, we found it to be redundant.

L328-330 I do not understand this sentence, please adjust

This point has been clarified. It concerned the fact that temperature is sampled for 1 s, and processed for ~0.18 s. Therefore temperature measurements are not strictly continuous.

L333 what overestimation? It has not been mentioned before. Please adjust the text

The overestimation by a factor 2 of the CT2 of the scintillometer in comparison to the CT2 of the sonic (Figure A3). We clarified this sentence.

L333-335 I disagree with this sentence. Based on your results, the CT2 values estimated from DTS with your algorithms were clearly underestimates when compared to the CT2 values derived from sonic anemometer data. You should discuss why this is the case

We agree that this sentence is confusing. What we meant is that the DTS underestimates CT2 by a factor 3-4 in comparison to the CT2 of the sonic (figure 11cd), while we also showed that the CT2 by the scintillometer is a factor 2 larger than the CT2 of the sonic(figure A3). Hence the factor 3-4 is not that far off in comparison to the factor 2 for the scintillometer. We adjusted the text to clarify this.

L341-343 I think this minimum CT2 value is because you did not consider the noise in Eq. (1), see major comment 3 above. Hence 0.04 is not the lower limit of detection for this DTS configuration, but rather lower limit for this DTS configuration + calculation algorithms. Please adjust the text

We have explained the noise treatment for the DTS measurement in our reply to the specific review point SPECIFIC(3+5) - NOISE. A paragraph has been added to the Discussion section in the revised manuscript addressing the signal to noise and noise treatments of the DTS measurement data. We also changed the terminology to include that the algorithm is part of the lower limit, as suggested by the reviewer.

L369-370 I would argue that it is not difficult to resolve the spectra on the time scale of 2 minutes if you calculate it over time. It is difficult only over space due to your finite cable length. Please adjust the text

Agreed, it is not so much that it is difficult to resolve the spectrum at a 2 minute timescale. It is however difficult to resolve CT2 from a 2 minute spectrum, as the inertial range extends beyond 2 minutes and is then not fully captured (see for example the spectrum in Figure 12). Besides that, a 2 minute temporal spectrum has to be resolved from about 90 temperature measurements, while the 70 m cable length contains 553 measurements. This has been included as a comment in the revised manuscript.

As we have removed Figure 4 concerning this 2 minute slice of DTS data, we have also removed this reference to the time scale in the discussion. The text has been adapted to focus on the difference in length scales, with a reference to Figure A4. Figure A4 has also been adjusted to represent a full 30 minutes of the data, rather than the current 20 minutes.

L373-374 Unclear, please clarify

We removed this sentence also to comply with our goal to more tightly focus on the CT2 application with DTS and not extend this to 3D turbulent temperature fields in general.

L379-383 Unclear, please modify

We modified this sentence. We aimed to point at the overlap between the spatial spectra and the temporal spectra (converted to k-space) and the potential of combining these.

L384-389 I suggest removing this part since here you discuss results that are not shown in this manuscript

Agree, we removed this part.

L399-400 Such solid copper blocks have been built already, see Thomas et al. (2022).

We added this reference.

L421 I suggest replacing “turbulence intensity” with “structure parameter”

Changed.

L423-425 or measuring with the current instrumentation higher above the surface where the eddies are larger, consider mentioning

This suggestion has been added

TECHNICAL CORRECTIONS

L129 & L131 I think you are referring to a wrong figure here, please correct
Yes, indeed we meant Figure 1c in L129 and Figure 1b in L131.

L210 Consider replacing “correct” with “account” or similar
We replaced ‘correct’ with ‘account’.

L212 remove “corrected”
agree

L222 replace “and over” with “to over”
agree

L238 replace “frequencies” with “wavenumbers”
agree

L295 replace “SpecDef” with “SpaceDef”
agree (changed to SpaceSOSF)

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