Egusphere-2024-2163- Response Letter 3

Dear Editor,

We would like to thank the editor for his comments that have allowed us to further clarify some aspects of the manuscript in this revised version. Hereafter, we report editor's comments and our replies (*in italics*). For yours convenience we have put the corresponding major changes introduced in red color in the revised version of the manuscript.

Comments:

In this paper the authors use a wind lidar to derive the terms in the TKE-budget. The two reviewers both had objections to some of the authors text and explanations and the authors responded to these in an adequate fashion. I have now reviewed the paper and I have some major concerns that I want to have answered before finally deciding on this paper.

Major comments

I am a big fan of using remote sensing instruments for this type of study and I think the time-height distributions shown in the last section of the paper illustrates their potential. There are, however, two potential major flaws in the representation of the TKE budget terms, especially relating to the dissipation term and the buoyancy term. Most of the comments below are based on the authors not properly explaining the choice of parameters in their Reynolds averaging for the two different instruments. I would also point out that with one of the terms in the budget equation, the dissipation, being estimated indirectly and another being calculated as a residual, the average budget profiles, like in Figure 10 & 12 must balance to zero; hence, if the dissipation is larger than the shear production, the buoyancy has to pick up the rest to balance the results. Therefore, Figure 12a is puzzling since that doesn't seem to happen here.

Starting with the buoyancy term, this is - contrary to the authors claims - never measured; it is estimated as a residual – after ignoring the pressure transport term which in my opinion is OK. But the reason to believe in the method comes down to the residual – assumed to be buoyancy – being much larger compared to other leading terms; that the residual cannot be explained in terms of errors in the other terms.

One could have assumed the time tendency term to be small and then proceed to calculate the buoyancy term as a residual, but the authors choose not to do this which is interesting. Instead they calculate the time tendency by taking a 5-second finite time-difference on a signal that appear to have a 20-minute averaging window. So, I can't help wondering what would happen if that time difference was taken over one minute or maybe even 20 minutes. The way this was done means that the magnitude of the residual – which is assumed to be the buoyancy production – could have been substantially smaller if the time difference had been substantially larger. Maybe taking the time difference over another time window has a small effect; maybe one could even assume that the time tendency is negligible.

Response: *As the reviewer suggests, we present the spatiotemporal distributions of turbulent kinetic energy, tendency term, turbulent transport term, dissipation rate, shear generation term, and buoyancy generation term under finite time differences of 5 seconds, 1 minute, 5 minutes, 20 minutes, and 25 minutes, as shown in Figures 11 to 15. It can be seen that as the difference time increases, the tendency term tends to stabilize, and even approaches 0 most of the time. When reaching 25 minutes, the results of the tendency term and buoyancy generation term become very different. Therefore, we can conclude that the smaller the difference time, the smaller the error between the tendency term and the buoyancy generation term.*

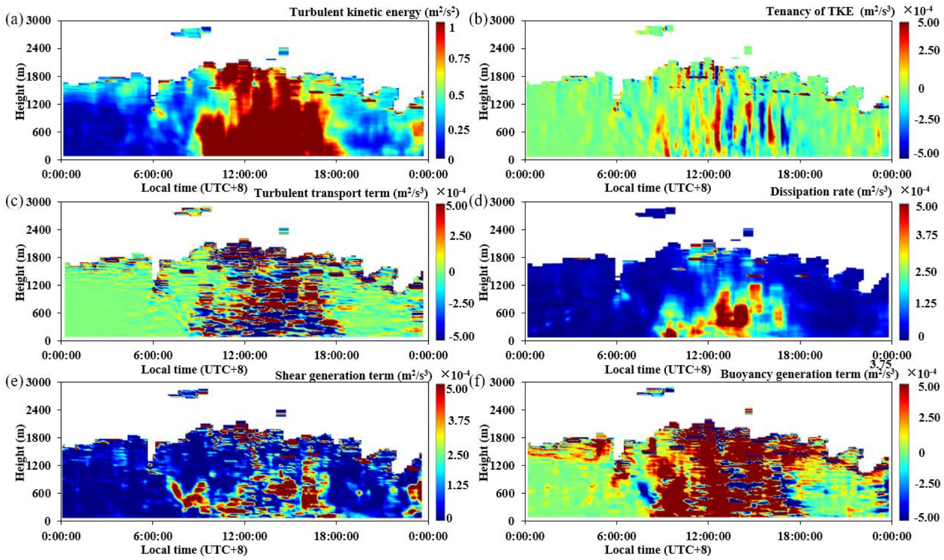

Figure 11. Temporal and spatial distributions of the TKE (a), tendency term (b), turbulent transport term (c), dissipation rate (d), shear generation term (e), and buoyancy generation term (f) on October 5, 2022 (a 5-second finite time-difference).

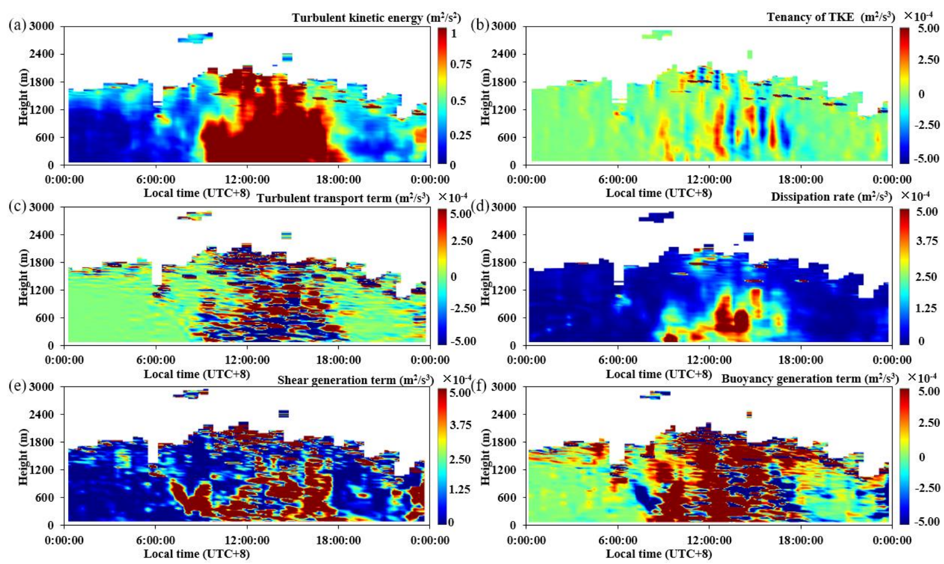

Figure 12. Temporal and spatial distributions of the TKE (a), tendency term (b), turbulent transport term (c), dissipation rate (d), shear generation term (e), and buoyancy generation term (f) on October 5, 2022 (a 1- minute finite time-difference).

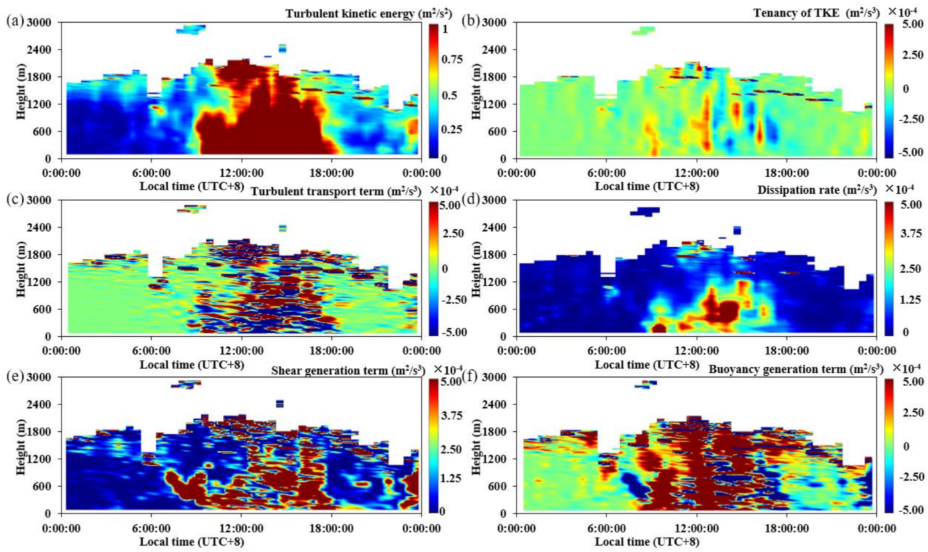

Figure 13. Temporal and spatial distributions of the TKE (a), tendency term (b), turbulent transport term (c), dissipation rate (d), shear generation term (e), and buoyancy generation term (f) on October 5, 2022 (a 5- minute finite time-difference.

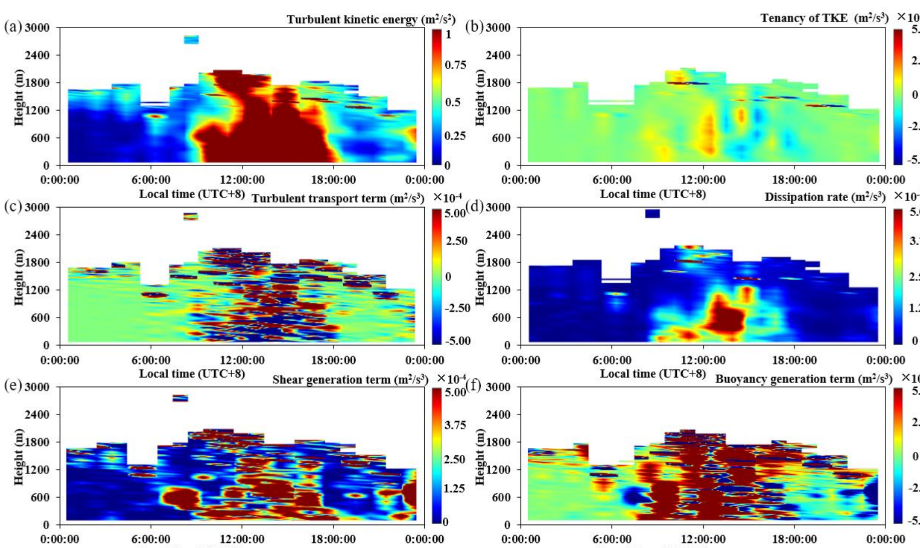

Figure 14. Temporal and spatial distributions of the TKE (a), tendency term (b), turbulent transport term (c), dissipation rate (d), shear generation term (e), and buoyancy generation term (f) on October 5, 2022 (a 20- minute finite time-difference).

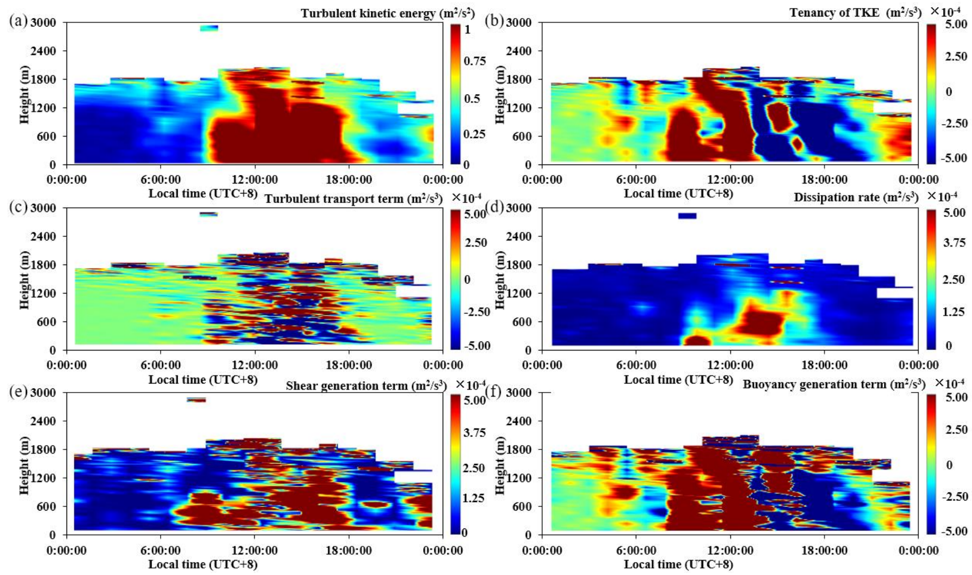

Figure 15. Temporal and spatial distributions of the TKE (a), tendency term (b), turbulent transport term (c), dissipation rate (d), shear generation term (e), and buoyancy generation term (f) on October 5, 2022 (a 25- minute finite time-difference).

Either way I would have preferred to: 1) Never say you measure something that is not actually measured, instead use "derive" or estimate"; 2) all terms in the budget be derived over the same time Reynolds averaging time window and; 3) describe carefully the way the turbulence terms are estimated in the Reynolds averaging for both sonic and lidar measurements.

Response: ***(1)****: As the reviewer suggests, we have made modifications in the revised version. (See lines 18 and 247) .*

(2) and (3)*: In this study, for wind lidar, the time resolution of all budget terms is the same, which is about 20 min. We have added the text in the revised version*, "*These fluctuation components can be obtained by subtracting the average of the observed wind speed data within a time window of duration N. In the subsequent estimation of turbulent energy dissipation rate, the value of N needs to simultaneously meet the requirements of the Fast Fourier Transform (FFT) method. In discrete FFT, 2^m data points are required, where m is an integer. Therefore, in this paper, we conducted FFT calculations for $2^8 \times 5$ s points (i.e., approximately N $\approx$ 20 min) using the data obtained from the wind lidar. All budget terms use the same duration window N. For the ultrasonic anemometer, $2^{14} \times 0.1$ s points (i.e., approximately 27 min) are required." (See lines 191 -196)*

Moving on to the dissipation term, the whole theory behind the suggested way of estimating the dissipation rate relies on the Kolmogorov theory and the existence of an inertial sub-range in the power spectra. Here the authors deviate from this by allowing the exponent to deviate from the expected -5/3 slope; one of two criteria for determining that such a range to exist in their data. They compare the results to observations from the sonic anemometers with good results, but they never discuss how the latter was estimated. Presumably it was estimated using the same type of method since the sonic sampled at 10 Hz cannot actually measure the dissipation directly. However, with an 0.2 Hz sampling rate for the lidar, it seems more likely that deviations from the expected slope that is discussed is due to never actually reaching the inertial sub-range and not from actual deviations of the exponent as explained in the text. Still the comparison to sonic observations, that sampled at 10 Hz, looks good, so why is that? If the estimates from the sonic are done for the same frequencies as for the lidar, maybe both estimates are wrong and that is why they compare so well.

To believe in this as a robust method, we should at least get to see some spectra from both sonic and lidar and time series of the calculated exponents (also from both) along with a discussion of what this would mean in case the lidar measurements at 0.2 Hz never reaches into the inertial subrange. If it does not reach into the inertial sub-range, the Kolmogorov no longer applies and Equation (2) cannot be used – at all. If it it just barely reaches this frequency range, equation 2 would allow calculation of dissipation from a single-frequency spectral estimate assuming the exponent is indeed prescribed to be the expected -5/3. I wonder what the results from such an approach would give?

Hence for the dissipation rate estimates I would like: 1) To know if both instruments are interrogated using the same technique with the same parameters or, if not, what was the difference(s); 2) to see examples of power spectra from both instruments to be able to judge if the spectral estimates used are indeed inside the inertial subrange and; 3) see time series of the value of the exponent arising from the method the authors claim to have used.

Response: ***(1)****: For ultrasonic anemometers and wind lidar, the same technology is used in the calculation of each budget term. The only difference is the length of the time window, which is mainly due to the inconsistency in resolution between the two. As the reviewer suggests, we have made modifications in the revised version.* "*These fluctuation components can be obtained by subtracting the average of the observed wind speed data within a time window of duration N. In the subsequent estimation of turbulent energy dissipation rate, the value of N needs to simultaneously meet the requirements of the Fast Fourier Transform (FFT) method. In discrete FFT, 2^m data points are required, where m is an integer. Therefore, in this paper, we conducted FFT calculations for $2^8 \times 5$ s points (i.e., approximately N $\approx$ 20 min) using the data obtained from the wind lidar. All budget terms use the same duration window N. For the ultrasonic anemometer, $2^{14} \times 0.1$ s points (i.e., approximately 27 min) are required." (See lines 191 -196)*

(2) and (3)*: Thanks for the editor's professional comments. In our previous researches, we demonstrated that in most weather conditions (e.g. sunny or cloudy), turbulence spectrum estimated by wind lidar can reach the inertial subrang. In some cases, the turbulence spectrum estimated by wind lidar cannot reach the inertial subrang, resulting in statistical errors (Xian et al., 2024c). This is mainly due to the detection frequency of wind lidar being only 0.2 Hz. This also provides a basis for increasing the detection frequency of wind lidar in the future. Here, we present our previous research findings as shown in Figure 2. It provides a comparison of turbulence spectra in three directions (U, V, W) between wind lidar and ultrasonic anemometer. From the graph, we can see that there is a high degree of consistency between the two in all three directions, and they all conform well to the -5/3 pow law. This proves that wind lidar can effectively monitor the wind speed turbulence spectrum in the inertial subrange. Furthermore, we present a comparison of the power exponents estimated by wind lidar and ultrasonic anemometer, as shown in Figure 3. From figures 3(d)- (f), it can be seen that both are quite consistent with the -5/3 power law. We cited two references about our previous research work in the paper. We have also added the texts in the revised version. (See lines 225 -230)*

For more information, please refer to the papers:

Xian, J. H., Lu, C., Lin, X. L., Yang, H. L., Zhang, N., and Zhang, L.: Directly measuring the power-law exponent and kinetic energy of atmospheric turbulence using coherent Doppler wind lidar, Atmospheric Measurement Techniques, 17, 1837-1850, 2024.

Xian, J., Luo, H., Lu, C., Lin, X., Yang, H., and Zhang, N.: Characteristics of the atmospheric boundary layer height: A perspective on turbulent motion, Science of The Total Environment, 919, 170895, 2024.

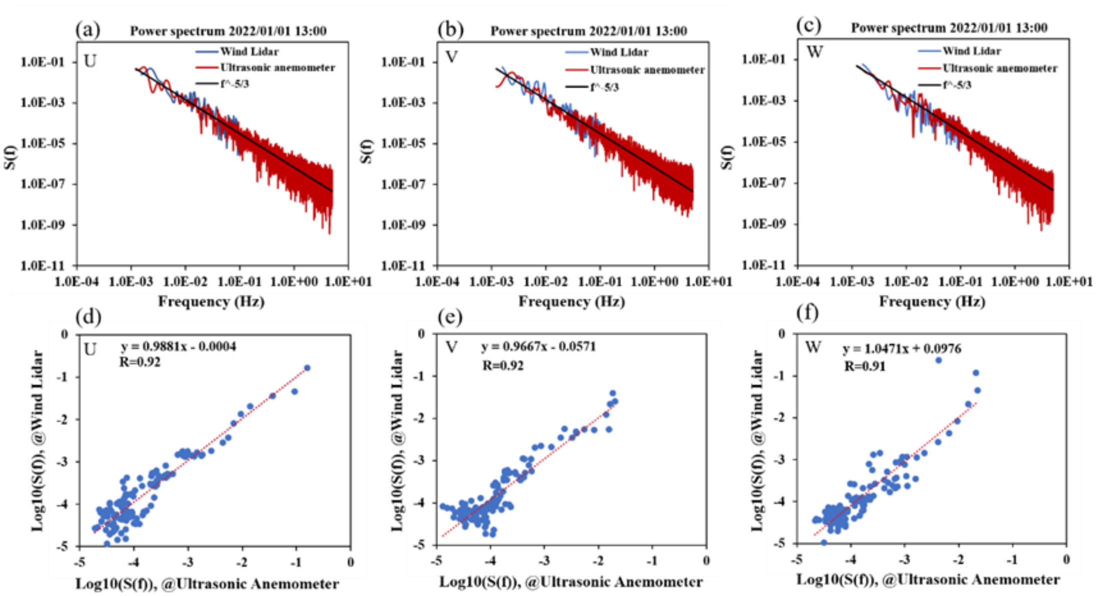

Figure 2. Comparison of the turbulence spectra obtained with the wind lidar and the ultrasonic anemometer in three directions: (a) *U*, (b) *V*, and (c) *W*, and the corresponding correlations (d), (e), and (f).

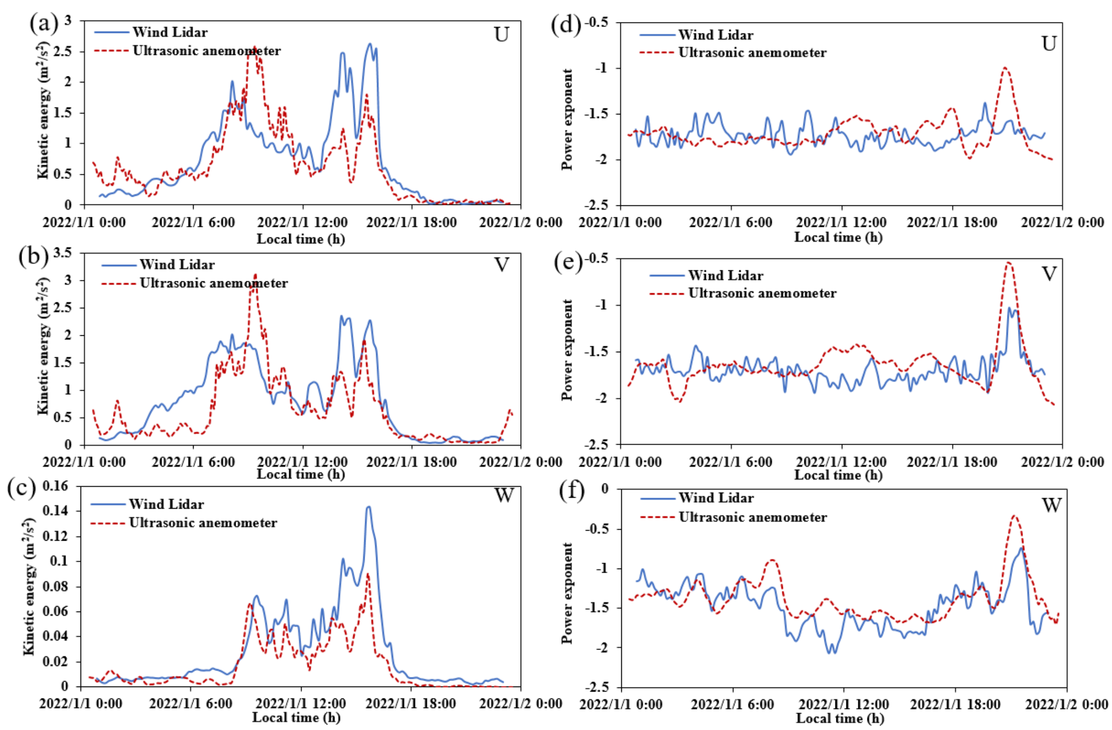

Figure 3. Comparison of the turbulent kinetic energy obtained from the wind lidar and three-dimensional ultrasonic anemometer on **January 1, 2022** in the (a) *U*, (b) *V*, and (c) *W* directions and the power-law exponent distribution in the (d) *U*, (e) *V*, and (f) *W* directions.

Detailed comments

Lines 17-19: Please repharse, as the buoyancy is in fact not measured by the lidar.

Response: *As the reviewer suggests, we have made modifications in the revised version. (See lines 18 and 253).*

Lines 33-34: Langauge: "… terms, which are key physical quantities …"

Response: *As the reviewer suggests, we have made modification in the revised version. (See line 34).*

Line 43: Maybe this is nit-picking but all these parameterization attempts to model turbulence. To simulate turbulence you need a DNS or possibly (stretching a bit) LES.

Response: *Thanks for the editor's professional comments. We have added the texts in the revised version. (See lines 38-40).*

Line 66 and elsewhere: Why do you not start out with the full reference and then skip it at the end: "Nilsson et al. (2016a) used …".

Response: *As the reviewer suggests, we have revised the citation way of this reference. (See line 69).*

Lines 84-85: Not to mention the cost of maintaining a tower compared to running a lidar!

Response: *As the reviewer suggests, we have added the texts in the revised version. (See line 86).*

Section 3.4: Why are we not shown a comparison between the shear-production terms instead of the shear stress terms?

Response: *Due to Δz = 160 m, the shear generation term obtained by the ultrasonic anemometer has a relatively large error. Therefore, we compared the u'w' and v'w' obtained by the wind lidar and ultrasonic anemometer. As the reviewer suggests, we have added the texts in the revised version. (See lines 250-252).*

Line 305: What do you mean by "gleaned"? Did you not actually measure this term?

Response: *As the reviewer suggests, we have modified the text in the revised version. (See line 314).*

Figures 9 & 11: There are some regions of these plots where the results seem quite noisy. Is there a way by which you can quality control these results, maybe looking at signal strength or pulse return rates?

Response: *Thanks for the editor's professional comments. Currently, in data processing, the signal-to-noise ratio has been used to remove noise points. We will further optimize the noise removal method in future research.*

Figures 10 & 12: By definition these profiles must add up to zero; that doesn't seem to always be the case, especially with in Figure 12a.

Response: *As the reviewer suggests, we have modified Figures 10, 12, and 14 and added the indicator line of Et-B-S-T+D in the figures, which can clearly and intuitively show the balance of turbulent kinetic energy.*

On behalf of all authors,
Sincerely,
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