Egusphere-2024-2163- Response Letter 1

Dear Editor and reviewers,

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

Reviewer 1:

This manuscript presents a new method for obtaining turbulent kinetic energy (TKE) budget estimates from wind lidar measurements. The budget term estimates are compared to values based on ultrasonic anemometer data. The main benefit of the proposed new method is that it can provide values of the different terms of the TKE budget throughout the vertical column and not limited to point measurements such as from the ultrasonic anemometer. The manuscript is well written and easy to follow, with a clear structure and mostly clear figures.

My main concern with this manuscript is how the accuracy of the new lidar-based method compared to the established ultrasonic anemometer-based method is judged and communicated. In general, the basis on which the authors conclude that the method is accurate is somewhat vague and I would like to see more critical discussion of the results.

On multiple occasions (such as on lines 189, 219, and 234) it is stated that the lidar data is consistent with the ultrasonic anemometer data but this claim is vague; is it possible to quantify the agreement?

Response: *As the reviewer suggests, we have added correlation coefficients to quantify consistency in the comparison of wind lidar and ultrasonic anemometer results. For example, in Figure 3, it can be seen that the two data sets have a very high degree of consistency, with a correlation coefficient of 0.98; in Figure 3(b), it can be seen from this plot that the wind lidar data can reflect the trend of turbulence changes very well, with a correlation coefficient of 0.97; in Figure 4, the consistency between the two is relatively good, with a correlation coefficient of 0.96.*

And how do the authors view the discrepancies that do exist? For example, in Figure 5(a) there are multiple positive peaks in the momentum flux from the ultrasonic anemometer that the wind lidar does not capture, why is that? Please include some discussion of if there are any conditions under which this method may not be suitable to use (as well as some comment on possible deficiencies of the ultrasonic anemometer data, while that is a more established method it is not the "truth").

Response: *Thanks for the reviewer's professional comments. There are multiple positive peaks in the momentum flux detected by the ultrasonic anemometer, but the wind lidar did not observe the same phenomenon at the corresponding time. It is speculated that one of the reasons is that the spatial resolution of the wind lidar is 30 m, which means that its results are the average effect within these 30 m, equivalent to smoothing the data, resulting in a difference in peak size between the two. Another possibility is that ultrasonic anemometers have a higher monitoring frequency (10 Hz), compared to wind lidar's 0.2 Hz, which can detect faster changes in high-frequency energy. As the reviewer suggests, we have added the texts in revised version. (See lines 247 to 254)*

Based on the detection principle of wind lidar, the method proposed in this study is applicable during sunny and cloudy conditions; however, it is not suitable for deployment during periods of heavy rainfall. Furthermore, this method holds potential for elucidating turbulence convection interactions and convective initiation before precipitation occurs. It is imperative to acknowledge that due to the inability to measure pressure transport terms

and monitor high-frequency turbulent energy, the error of the proposed method may increase in weather processes dominated by these two factors As the reviewer suggests, we have added the texts in the conclusions. (See lines 447 to 451)

Furthermore, in the abstract and conclusions it is emphasized that the errors are "less than 0.0001 m^2/s^3, for at least 47% of the data", which to me does not sound very convincing as it leaves the possibility that the remaining 53% of the data could have large errors. Figure 8 shows that this is not the case, but I recommend that the authors comment on this both in the abstract and in the main body of the text. In the abstract and conclusions it should be made clear that those numbers pertain to the buoyancy term as this is currently not mentioned.

Response: *Thanks for the reviewer's professional comments. By comparing these data with those obtained with a three-dimensional ultrasonic anemometer, the results indicate that the error of the buoyancy generation term detected by the proposed method is relatively small, with an average absolute value of less than 0.00014 m^2/s^3, which verify the accuracy and reliability of our method. As the reviewer suggests. we have added the texts in the abstract and conclusions. (See lines 444 to 451)*

Overall, I find this an interesting paper and the proposed method provides information that is useful for understanding the generation and dissipation of turbulence in different atmospheric conditions, and with the inclusion of a more rigorous discussion of the validity of the results as well as consideration of the specific comments below I find it suitable for publication.

Response: *Thanks for the reviewer's professional comments.*

Line by line comments:

1. Line 11: "The turbulent kinetic energy (TKE) budget term, as a key physical quantity […]" The subsequent sentences discuss budget terms in plural, should it be plural here as well? The use of the words "TKE budget term" is used at multiple occasions throughout the manuscript, please make it clear if you are referring to a specific term in the budget or the budget as a whole (for example on line 32).

Response: *Thanks for the reviewer's comment. It should be "TKE budget terms". We have modified the texts in revised version. (See line 11)*

2. Line 38: "(YSU, MYJ, MYNN2, ACM2, etc.)" These acronyms should be defined and ideally references for the schemes should be provided.

Response: *As the reviewer suggests, we have modified the texts in revised version. (See lines 38 to 41)*

3. Line 107: What does "strong representativeness" mean? Please elaborate on what the data is representative of.

Response: *As the reviewer suggests, we have modified the texts in revised version. The wind field and temperature data obtained by the three-dimensional ultrasonic anemometer and thermometer on the gradient observation tower can represent the environmental characteristics of the region. (See lines 108 to 111)*

4. Line 134: When it says that "the wind speed measurements are checked every 30 min every day", what is involved in the "checking"?

Response: *Thanks for the reviewer's comment. We calculate the average and standard deviation of wind speed and direction every 30 minutes, and use the triple standard deviation principle to remove outliers. We have made revisions in the text for better readability. (See lines 138 to 139)*

5. Line 143: Is "[29]" a literature reference? If so, please write it in the same format as the other references. If not, please explain what it means.

Response: *Thanks for the reviewer's comment. "[29]" is a literature reference. We have updated the citation of this literature. (See lines 148 to 149)*

6. Figure 2: In this figure it is very difficult to judge the agreement between the blue and orange lines since they mostly overlap such that the orange line covers the blue one. Consider plotting the difference between the two lines (or some other measure of the difference) rather than the absolute values to facilitate comparison. To some extent this is true also for the other figures that compare two timeseries, but the problem is the biggest in Figure 2.

Response: *As the reviewer suggests, we have updated the Figure 2 in revised version.*

7. Line 174: θv usually denotes virtual potential temperature (so also in Stull, 1988 and Nilsson et al., 2016a which are referenced); it makes it easier for the reader if conventions for variable naming are kept.

Response: *As the reviewer suggests, we have modified the texts in revised version.*

8. Line 176: I believe it should be "tendency" rather than "tenacy"; also on Line 188 and in the caption to figure 3.

Response: *As the reviewer suggests, we have modified the texts in revised version. (See lines 181 to 185)*

9. Line 185: The text says that Figure 3(a) shows data from the wind lidar obtained at both 150 and 160 m height but the figure seems to show only data from 150 m.

Response: *Thanks for the reviewer's comment. We have modified the texts in revised version. Due to the spatial resolution of the wind lidar data being 30 m, the TKE at a height of 150 m was selected from the vertical profile obtained by wind lidar (shown in orange) and compared with the results obtained by the three-dimensional ultrasonic anemometer at a height of 160 m (shown in blue) on the tower from October 1 to 9, 2022, as shown in Figure 3(a). (See lines 190 to 193)*

10. Figure 5: What is the value for delta z for the ultrasonic anemometer?

Response: *Thanks for the reviewer's comment. Ultrasonic anemometers were installed on the tower at heights of 160 m and 320 m. For the ultrasonic anemometer, Δz = 160 m. We have added the texts in revised version. (See line 241)*

11. Line 266: Please provide a reference to the statement that the pressure transport tern is negligible in practical operations.

Response: *As the reviewer suggests, we have added citations in revised version. "In some cases, the pressure transport term is estimated through residual calculations, which indicate that it negligible in practical operations (Kaimal and Finnigan, 1994; Wyngaard, 2010; Pozzobon et al., 2023); therefore, it is ignored in this study." (See lines 280 to 282)*

12. Line 276: "affect" should be "be affected by", since B is derived as the residual and thus cannot affect the accuracy of the other terms.

Response: *As the reviewer suggests, we have modified the texts in revised version. (See line 291)*

13. Line 290: "gleamed" -> "gleaned"

Response: *As the reviewer suggests, we have modified the texts in revised version. (See line 305)*

14. Line 289-291: Please describe how the errors were calculated (what definition of the error is used).

Response: *As the reviewer suggests, we have modified the texts in revised version. The buoyancy generation term (B') gleaned from the three-dimensional ultrasonic anemometer data was used as the standard value. The error ($\Delta B = B - B'$) of the buoyancy generation term (B) detected by the wind lidar was calculated, and its distribution was statistically analyzed, as shown in Figures 8(a) and (b). (See lines 304 to 307)*

15. Figure 8 caption: I find that this caption does not accurately describe the contents of this figure.

Response: *As the reviewer suggests, we have modified the caption in revised version.*

16. Paragraph starting with Line 311: The period after 12:00 is not mentioned in the text, please provide some comment on the interpretation of that part of the figure.

Response: *As the reviewer suggests, we have added the texts in revised version. From 12:00 to 19:00, due to the cover of low clouds, there was less ground radiation, and the buoyancy generation term was basically negative, which mainly suppressed and dissipated turbulence. However, the shear generation term caused by the still existing low-level jets had a relatively large value, occupying the main guiding role in the generation and maintenance of turbulent energy, resulting in strong TKE. After 19:00, although the shear generation term remained relatively large, the dissipation effect of the buoyancy generation term also increased, leading to a weakening of TKE. (See lines 344 to 350)*

17. Figure 9: Since all the data is from the same day the date (2022/10/1) can be removed from the x-axis labels, this would make the hours on the axis easier to read (the same is true for figures 11 and 13).

Response: *As the reviewer suggests, we have updated the figures in revised version.*

18. Figure 9: Please emphasize somehow that the range of the color scales differs between the panels (and potentially change the color scales such that zero has the same color in all panels).

Response: *As the reviewer suggests, we have modified the texts in revised version. The color scale range of panels varies for different TKE budget items, but for the same TKE budget item, the color scale range remains unchanged. (See lines 330 to 332)*

19. Line 331: "observed by (Nilsson et al., 2016a)" -> "observed by Nilsson et al. (2016a)"

Response: *As the reviewer suggests, we have modified the texts in revised version. (See line 352)*

20. Figure 10: A thin vertical indicating zero would be helpful for seeing if the terms are positive and negative.

Response: *As the reviewer suggests, we have updated the figures in revised version.*

21. Data availability statement: According to the ACP data policy, "The best way to provide access to data is by depositing them (as well as related metadata) in FAIR-aligned reliable public data repositories, assigning digital object identifiers, and properly citing data sets as individual contributions". Providing the data in an easy to access format, in a public repository, is much preferable to providing it only upon request.

Response: *As the reviewer suggests, we have stored the dataset on Github and generated a DOI through Zenodo. The wind field data measured by ultrasonic anemometer can be downloaded from the Shenzhen Data Open Platform (https://opendata.sz.gov.cn/data/dataSet/toDataDetails/29200_00900273). Data to generate the figures of this paper are available at https://doi.org/10.5281/zenodo.13624484(Xian et al., 2024a). (See lines 465 to 468)*

On behalf of all authors,
Sincerely,
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Egusphere-2024-2163- Response Letter 2

Dear Editor and reviewers,

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

Reviewer 2:

A scientifically sound turbulent energy budget analysis is required for better understanding of the generation and dissipation processes of turbulence. However, current research on the generation and dissipation mechanisms of atmospheric turbulence energy is mainly based on ground or tower base observations, leading to unknown vertical TKE budget term. The authors propose a new method based on coherent wind lidar to detect TKE budget terms and compare them with data from a three-dimensional ultrasonic anemometer for verification. The results indicate that their proposed method can comprehensively reflect the impact of each budget term on the vertical structure of TKE, providing a new perspective and method for atmospheric turbulence research. The expression of this paper is clear, the argument is reasonable. It is suitable for publication. I think there are some small issues that can be improved, which is shown as follows:

Response: *Thanks for the reviewer's professional comments.*

Minor comments:

1. Lines 176 and 188 : "tenacy" should be "tendency".

Response: *As the reviewer suggests, we have modified the texts in revised version. (See lines 181 to 185)*

2. In section 3.7 Determination of the Buoyancy Generation Term: I suggest the authors elaborate on the sources of errors.

Response: *As the reviewer suggests, we have modified the texts in revised version. Due to the ability of wind lidar to obtain accurate three-dimensional wind speeds, the terms Et, S, D, and Tt are accurately obtained in turn. Therefore, the error mainly comes from the assumption that the pressure transport term, Tp, is negligible. (See lines 293 to 295)*

3. Figure 8 shows that at the height of 160 m, 48% of the results have an error of less than 0.0001 m2/s3; At the height of 320 m, 47% of the results have an error of less than 0.0001 m2/s3. The error statistical method is not rigorous enough and should be given as mean error or standard deviation.

Response: *Thanks for the reviewer's professional comments. By comparing these data with those obtained with a three-dimensional ultrasonic anemometer, the results indicate that the error of the buoyancy generation term detected by the proposed method is relatively small, with an average absolute value of less than 0.00014 m^2/s^3, which verify the accuracy and reliability of our method. As the reviewer suggests. we have added the texts in the abstract and conclusions. (See lines 444 to 447)*

4. The caption of Figure 8 does not effectively convey the meaning of this figure.

Response: *As the reviewer suggests, we have modified the caption in revised version.*

5. Lines 293 and 294: How do you calculate the error? The calculation method for the error should be provided in the text.

Response: *As the reviewer suggests, we have modified the texts in revised version. The buoyancy generation term (B') gleaned from the three-dimensional ultrasonic anemometer data was used as the standard value. The error ($\Delta B = B - B'$) of the buoyancy generation term (B) detected by the wind lidar was calculated, and its distribution was statistically analyzed, as shown in Figures 8(a) and (b). (See lines 304 to 307)*

6. Can this method proposed here be applicable in other circumstances? e.g., How about the implications for elucidating the turbulence-convection interaction, and convection initiation.

Response: *As the reviewer suggests, we have added the texts in the conclusions. Based on the detection principle of wind lidar, the method proposed in this study is applicable during sunny and cloudy conditions; however, it is not suitable for deployment during periods of heavy rainfall. Furthermore, this method holds potential for elucidating turbulence convection interactions and convective initiation before precipitation occurs. It is imperative to acknowledge that due to the inability to measure pressure transport terms and monitor high-frequency turbulent energy, the error of the proposed method may increase in weather processes dominated by these two factors. (See lines 447 to 451)*

7. Lines 53-56: "...including changes in surface heat flux, atmospheric stability, and topography". More recent references are needed to support this statement. The authors can refer to https://doi.org/10.5194/acp-21-17079-2021.

Response: *As the reviewer suggests, we have added some relevant references in revised version.*

8. Lines 58-59: Radar wind profiler can provide such high-resolution turbulence measurements (doi:10.1016/j.uclim.2022.101151), and can be mentioned here..

Response: *As the reviewer suggests, we have added some relevant references in revised version.*

On behalf of all authors,
Sincerely,
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