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Discussions
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Interactive comment on "Influence of Atmospheric Stratification on the Integral Scale and Fractal Dimension of Turbulent Flows" *by* M. Tijera et al.

Anonymous Referee #1

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Title: "Influence of Atmospheric Stratification on the Integral Scale and Fractal Dimension of Tubulent Flows"

Authors: "M. Tijera, G. Maqueda, C. Yague"

This paper investigates the possibility of correlations between the integral scale of stratified turbulent flows in the boundary layer and parameters characterizing topological features of the wind velocity field, such as the fractal dimension, and its stability properties studied through the Richardson number. Using data of the SABLES-98 campaign, at moderate temporal resolution, authors present here a series of results which

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provide interesting insights on the dynamics of the atmospheric flow in the site under study. The outcome of the paper is relevant for the audience of Nonlin. Processes in Geophysics and I am leaning to suggest it for the publication after a revision of the text along the lines detailed in the following. Such a revision is necessary to achieve a clear understanding of the validity of the results presented and to make them available to a large swat of readers.

1 line 5-6 page 2. The text says: "The aim of this paper is to bridge the considerable gap that exists between the fractal dimension and the integral scale"

This sentence does not make much sense in this form and it should be rephrased. Perhaps the authors mean to say that there is a lack of investigations on the connection between the integral scale and the fractal dimension as estimated in turbulent flows. Please clarify it modifying the text accordingly.

2 line 22-23 page 2. The text is not clear, is it a 5 minutes running average used to remove the mean field? This point needs to be better explicated in the text.

3 line 11-14 page 3. The sentence within this lines is not at all clear, please rephrase it.

4 line 2 page 4. The Richardson number provides a measure of how a turbulent flow is prone to develop instabilities. I thus disagree with referring to this parameter as a number that characterize the degree of stratification in the atmosphere. This definition (used in many places in the paper) is misleading and it is very important to modify it in the text not to drive the wrong message that stratified flows cannot be unstable. Indeed the Richardson number is used to identify unstable regimes also in strongly stratified flows and strong instabilities (and turbulence) can develop in the direction of the gravity, characterized a vertical Froude number of order ~ 1 as described in Billant & Chomaz, “Self-similarity of strongly stratified inviscid flows,” Phys. Fluids 13, 1645–1651 (2001).

5 line 22 page 4. The text says: ".. the integral scale of the mean wind direction u’".

Here the authors probably means the horizontal direction but the way this is written sounds wrong. Please rephrase it.

7 line 1-2 page 6. See comment ## 2 and please specify better the way the data have been processed. This is important for the readers to be able to reproduce (and therefore validate) the results presented in the paper.

8 line 3-4 page 6. Authors claim they resolve vertical integral scales from 1m to 1000m, which puzzles me a bit. Indeed, I am wondering how is it possible to detect (using the Taylor hypothesis) vertical scales larger than the hight(s) at which the sonic anemometer are placed? An answer to this point should be included in the report and and a compelling explanation integrated in the text.

9 line 20 page 6. Details on how the potential temperature has been estimated should be included in the text.

10 line 12 page 7. The text says: "Thus, stable stratification decreases the fractal dimension."

Authors acknowledge they cannot provide rigorous arguments to explain the variation of the fractal dimension with the height. As a consequence the statement above sounds a bit too strong, unless they propose some solid argument to support it.

11 line 14-27 page 7. The text within this lines is poorly written and its meaning is a bit confusing. I suggest to re-written it from scratch and perhaps make it a bit more concise.

12 There is a long standing debate in literature on whether an inverse cascade occurs in stably stratified anisotropic flows (with or without rotation). The inverse cascade mechanism, if any, might also be responsible for the growth of the integral scale in the stratified atmosphere. I thus suggest the authors to address this point, perhaps when they re-arrange the text (line 14-27) as indicated in the previous comment (## 11). On this note/topic I suggest to cite these two papers:

L. Smith and F. Waleffe, “Generation of slow large scales in forced rotating stratified turbulence,” J. Fluid Mech. 451, 145–168 (2002).

R. Marino, P.D. Mininni, D. Rosenberg, and A. Pouquet, “Large-scale anisotropy in stably stratified rotating flows”. Phys. Rev. E 90, 023018 (2014).

13 Conclusions should definitely be rearranged from line 16-24 to achieve a better clarity of the text.

14 Units are missed in some plots, for instance in figure 2 "tau" is indicated but it is not clear whether the lag is given in seconds, minutes or hours. Please double-check all the figures and add the units (on the axes/legend) when it feels needed.

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