‘On dissipation time scales …’ preprint, response to Anonymous Referee #3

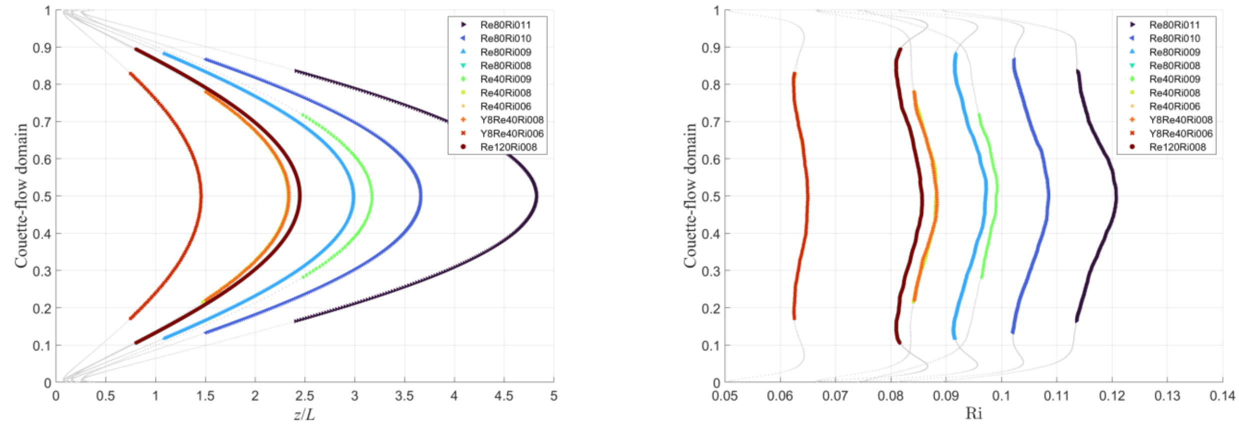
Review by Anonymous Referee #3 (https://egusphere.copernicus.org/preprints/egusphere-2023-3164#RC3).

Authors' response in italics, highlighted by light grey colour.

This manuscript aims to provide additional insight to the effects of stratification on turbulence properties and consequences for how we model those. The results are based on DNS data and show some promising results and valuable discussion, however, there are some odd choices in the analysis that result in more confusion than clarity. Furthermore, the abstract and introduction includes discussion of conditions up to extreme static stability which the results of the paper does not provide results for. At the end of the manuscript (line 310) it is noted that the DNS experiments were limited to gradient Richardson numbers up to Ri=0.2. This statement does not align with the Figures that presents data up to z/L = 5 which is confusing. This discrepancy goes to the heart of my problem with the analysis which is the introduction of performing the analysis using z/L as stability parameter which is just introduced without proper motivation on line 145. At the end of the manuscript, the authors then advocate to go back to a Richardson number (in this case the gradient Ri) with the motivation “for practical reasons”. I would like to see the analysis performed using Ri as the stability parameter throughout which I anticipate would provide a more straightforward analysis. Furthermore, I dislike the extrapolation outside of the DNS parameter space, for example the exponential growth far outside the range of the DNS results in Figure 6d. Presenting the results in this way discredits the results. In conclusion, the manuscript needs considerable rewriting before it can be properly judged for publication.

We appreciate the reviewer's comments. First, we need to apologise for the misleading estimate of maximum presented gradient Richardson number which in fact was only $Ri = 0.12$. This oversight (a typo) has now been corrected.

We prefer to show the z/L dependences rather than the Ri dependences in our analysis for a practical reason. In Couette flow Ri barely changes within the fully-developed turbulence layer due to minimal gradient variations (see new Figure 1 in the manuscript), while z/L provides a better dynamic range, given that L remains practically constant while z is determined by the distance from the walls (for details, see Figure 1 in Zilitinkevich et al., 2019):



This clarification is now added in the beginning of Section 3.

Line 25: There is no analysis of extreme static stability presented.

We have revised the wording to reflect the range of stability conditions studied more accurately.

Line 144: There is no motivation for using this conversion from flux Ri to z/L (which is a stability parameter and not a stratification parameter). I understand from reading the Acknowledgements that Prof Zilintikevich was instrumental for the project, maybe this remains as one of his ideas. However, if it does not make sense for the continued analysis, it should be removed. That would be in the spirit of Sergej, whom I knew and also worked with.

The clarification of stratification parameter preference is now added in the beginning of Section 3.

Line 152: It is not correct to write “empirical validation”. First DNS data is not empirical data, and second, the data is used for evaluation not validation.

The title of Section 3 has been changed to 'Methods' to avoid confusion. Following the recommendation of the Referee we have replaced 'empirical' by a more accurate term 'obtained from DNS experiment'.

Line 171: More details on how the prescribed Dirichlet boundary is imposed to maintain the stable stratification is needed.

We have added clarifications on the DNS setup in the paper. The stable stratification is maintained by prescribed Dirichlet boundary conditions on the potential temperature. This, along with prescribed Dirichlet boundary conditions on the velocity field, allows us to fix the Reynolds number (based on the wall velocity difference and channel height) and the bulk Richardson number (based on the wall velocity and temperature differences and channel height) in each experiment. It is important to note that in this case, the friction velocity and the *potential temperature flux (as well as the Obukhov length scale) are computed during the model run, rather than being prescribed.*

Line 172: Why did you chose to fix a value of the molecular Pr number to 0.7. What is that based on?

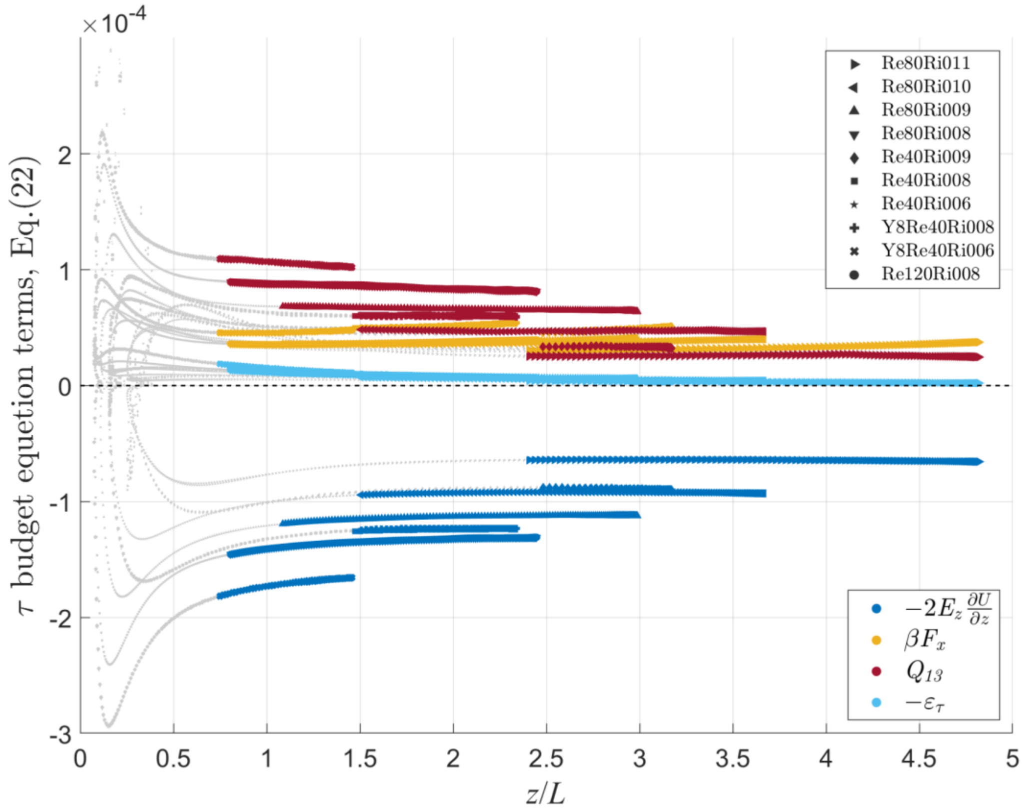
We chose to fix the value of the molecular Prandtl number to 0.7 based on its typical value for air. The clarification has been added.

Line 183: When reaching the end of this description, there are still missing information on how the DNS experiments were conducted. How many simulations? Initial conditions? Time step? At what stratification? When did the simulation reached statistical steady-state, to what accuracy? Again, it is stated "turbulence up to extreme static stability" which is not that case. How do you know that you are resolving all dissipation time scales? Do you have any numerical diffusion? The experimental parameters could be summarized in a Table. It would also add to the manuscript if the various experiments were color-coded in the Figures so they can be identified.

We have made several updates to the manuscript. A table summarising DNS parameters has been added, covering ten different experiments presenting well-developed turbulence. Additionally, we have included a more comprehensive description of the DNS methodology. The figures have been replotted to enable the identification of different experiments.

Line 187: Is it correct to interpret this statement as buoyancy is a dissipative term in stratified conditions?

Correct. Indeed, the molecular-viscosity dissipation term is relatively small, with the dissipative role being largely fulfilled by the pressure-shear correlations and the horizontal turbulent transport of potential temperature (see figure below).



Line 197: It is really not clear to me why you choose to plot the results as function of z/L when you have Ri_f in the equation. Furthermore, I think it would be good to remove the near-neutral DNS results as they are not credible anyway in all figures, that would lead to improved visibility in the various panels. The fitted line in Figure 1 cross z/L = 0 at the value 0.2, is that a given? Do you have neutral DNS to constrain that?

The clarification of the stability parameter preference is now added at the beginning of Section 3. The viscous sublayer points have been toned down in all figures to enhance visibility. The ratio of the effective dissipation time scale of τ *to the dissipation time scale of TKE was found to be 0.2 at* $z/L = 0$ *as a result of fitting DNS data of stably stratified Couette flow; this should be considered an extrapolation. While performing DNS for neutrally stratified flow would confirm or correct this value, we will leave this for future studies as this work is focused on stably stratified turbulence.*

Line 199: Why to you propose a ration of two first-order polynomials? That is a quite advanced fitting, did you try simpler representation of is the proposition based on any theoretical argument?

The ratio of two first-order polynomials is chosen as a simpler fitting function that could provide monotonicity, reasonable smoothness, and clear finite asymptotes. All three adjustable parameters of this approximation are easy to *understand: the function value at $z/L = 0$, the $z/L \to \infty$ limit, and the transition between them. The clarification is now added to the manuscript.*

Figure 2: The labels are very unclearly written, or unnecessary complicated. I assume you are dividing with the whole left part of Eq 31 but the label it is not clear.

Correct. The readability of labels in the Figure are now improved.

Line 252: Would be good with some references here for this discussion, there are empirical results for how asymmetry varies.

Figure 5: Could be interesting to see how this would fair with other assumptions for Az. The DNS results are quite variable.

After lengthy discussion, we decided to approximate A_z as a function of z/L, in line with other dimensionless parameters, to maintain consistency in our methodological approach, without altering the essence of the paper. Additionally, references to existing approximations of A_z were included.

Line 281-285: See discussion above regarding the stability parameter, the discussion here is not very insightful.

We believe that the revised explanation of stability parameter preference makes this part more insightful.

Line 286: Why is the function a polynomial of the 5th order?

Since $Ri = \mathrm{Pr}_T Ri_f$, one might substitute Eqs. (20, 21, 27, 35, 40) into Eq. (36) and perform arithmetic operations, resulting in a ratio of two 5th-degree polynomials. This implies that obtaining z/L after knowing Ri would require solving a polynomial equation of the 5th degree.

However, with the recent changes to the approximations in the revised manuscript, this approach is no longer valid, and the approximation for Ri_f vs Ri is required.

Line 310: I do not understand why you show results that are outside of what the DNS results support. Overall, the figures need to be of better quality.

The concluding remarks were clarified. The Figures were redrawn for better quality.