Response to Anonymous Referee #2

We are very grateful to reviewer for careful and friendly review of our paper. All the comments are very useful and helped us to improve the manuscript.

The following corrections were made:

P.1, Line 14: "Over years the problem" should be "Over the years, the problem of …"

Corrected.

P.1, Line 15: " the process of dissipation which takes place" should be "….that takes place".

Corrected.

P.2, line 1: drop 'topical'.

Done.

P.2, line 11 g is the gravitational acceleration, reads better.

Corrected.

P.2, line 22 – there are inconsistencies in the definition of the stability parameter. L, the Obukhov length, is capital whereas z/l is used throughout – it should be z/L. Same issue on p.3, line 2.

Corrected.

The coefficient Cu in equation (8) …

The difference is due to definition of the Obukhov length-scale. We define it as $L = \frac{\tau^{3/2}}{-\beta F_z}$, while in papers on the Kansas experiment it is defined including the von Karman constant: $L = \frac{\tau^{3/2}}{-\beta F_z k}$. This just makes approximately two times difference in empirical constants.

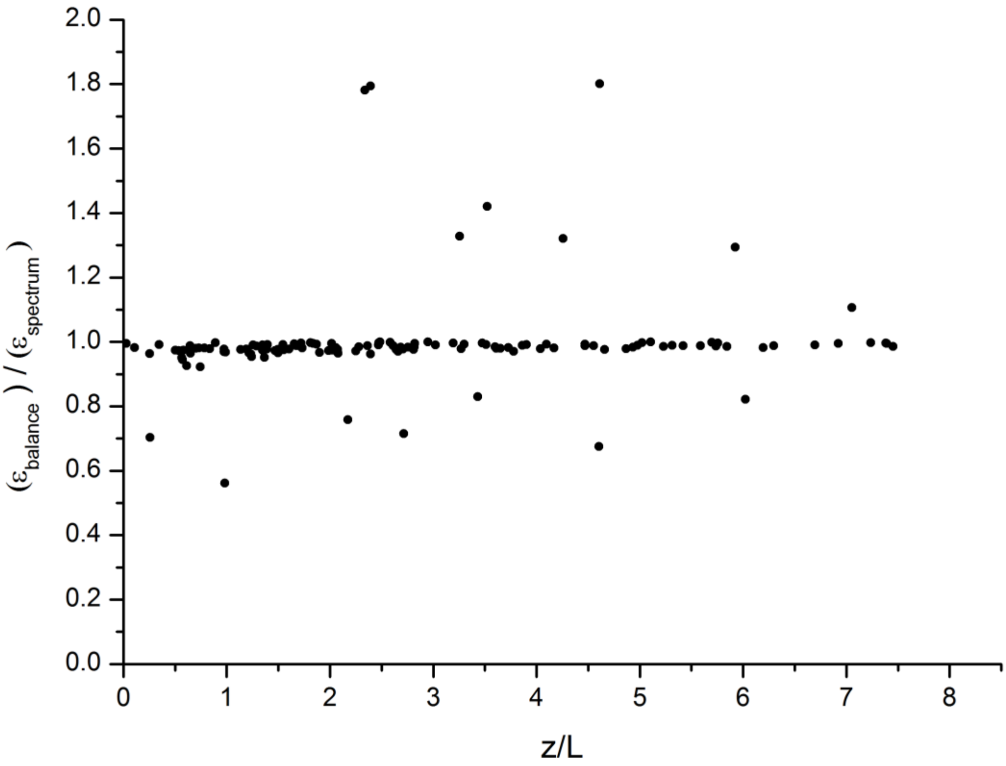
The value of R_∞ …

P.2, line 6, the following sentences were added:

It is worth noting that R_∞ can be derived from well-established phenomenological constants of turbulence in the inertial subrange (Katul et al., 2014). The actual value in this case is slightly higher ($R_\infty = 0.25$) but still within reasonable range.

Page 4, lines 27-28: I think it is worth showing a 1:1 comparison of the mean turbulent kinetic energy dissipation rate estimates from the spectrum and from the residual of the TKE budget.

Such comparison surely deserves consideration. Different types of data used in our paper show very good correspondence between the dissipation rates estimated (i) from spectrum and (ii) as the residual of TKE budget. The difference is within 5% (see the figure below).



In this paper we relay basically on DNS and use atmospheric data only to illustrate principal agreement between surface-layer data and DNS data. We would not like to include additional figures for the following reason. Additional (and not very necessary) figures inevitably diverge the attention of readers from the basic subject.

Page 6, Equation (20) is really the main result as it shows how the turbulent potential energy and the turbulent kinetic energy play a role in shaping the mean turbulent kinetic energy dissipation rate with stability. May be worth expanding this connection in the conclusion

Thank you for this valuable remark. We add the following sentences.

1) In the very end of section 3:

There is essential advantage of Ri_E as criterion of stratification in numerical modelling. Turbulent fluxes are usually calculated through the diagnostic down-gradient formulations: $\boldsymbol{\tau} = -K_M \, \partial \mathbf{U}/\partial z$ and $F_z = -K_H \, \partial \Theta/\partial z$, where K_M is eddy viscosity and K_H is eddy conductivity. Then, finite-difference approximation of the gradients causes uncertainties in $\boldsymbol{\tau}$, F_z and, hence, the Obukhov length, L [Eq. (3)], flux Richardson number, Ri_f [Eq. (5)], and gradient Richardson number, Ri [Eq. (4)]. Contrastingly, TKE and TPE are defined from the prognostic budget equations accounting for turbulent diffusion that smooths the energies and assures quite certain calculation of Ri_E.

2) In the very end of concluding remarks:

Universal analytical formulation of $(kz/\tau^{1/2})\,\partial U/\partial z$ versus z/L yields the single-valued relations linking z/L as criterion of stratification in the surface-layer flow or $\tilde{z}/L$ as the same criterion in Couette flow with alternative criterions: flux Richardson number, Ri_f [Eq. (5)], and the newly introduced "energy Richardson number", Ri_E [Eq. (13)], applicable to any turbulent regimes. This opens prospects for extending the obtained dependence of dissipation rate on static stability to any stably stratified turbulent flows.

Figure 3 – worth adding the best-fit line from the Kansas data as well.

Same reasoning applies here as in comment on comparison of dissipation rate estimated from spectrum and as the residual of TKE budget: in this paper we relay basically on DNS and use atmospheric data only to illustrate principal agreement between surface-layer data and DNS data.