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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Rossby number similarity of atmospheric RANS using limited length scale turbulence closures extended to unstable stratification" *by* Maarten Paul van der Laan et al.

Anonymous Referee #2

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Review of the manuscript:

Rossby number similarity of atmospheric RANS using limited lengthscale turbulence closures extended to unstable stratiïňĄcation Maarten Paul van der Laan, Mark Kelly, Rogier Floors, and Alfredo Peña

General comments:

In this paper, a one-dimensional RANS model is used to simulate idealised ABL profiles and shows that, for neutral and stable conditions, the Blackadar limited length

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scale model, and the limited length-scale k-e model of Apsley and Castro produce profiles that can be described by two Rossby numbers: one using z0 as the characteristic lengthscale, the other one using the maximum turbulence lengthscale defining the model. For unstable conditions, an extension is proposed to the limited lengthscale k-e model, which produce profiles characterised by a third Rossby number depending on the Obukhov lengthscale. The model derived drag law parameters A & B fit well within the range of observed typical A & B values. The model's ability to reproduce measured ABL profiles is tested with a varying level of success. The investigation is interesting, and potentially useful to provide inflow boundary conditions to a 3D model from a library of precalculated profiles. The application of the derived profiles within a 3D model hasn't been attempted yet in this contribution. The paper is generally well written, although some points/comments should be addressed as listed below.

Specific comments:

p.1, line 16. What does 'simple enough to be applicable in the wind energy industry' mean? So it can be used for wind turbine design? So it can be used as inflow b.c. for a flow model?

P.1, line 24-25. 'These turbulence models can simulate stable and neutral ABLs without the necessity of a temperature equation and a momentum source term of buoyancy. In other words, all temperature effects are represented by the turbulence model'. This is only true when looking at horizontally homogeneous flows (i.e. the 1D flows modelled in this paper). Once terrain, coastal discontinuities, or even an offshore wind farm is perturbing the flow, gravity waves can develop, which have the potential to affect the wind speed distribution at hub height. And to capture these you need the buoyancy source term in the momentum equation. Please elaborate on these. Your statement as it is can be misleading and let the reader assume that they can generally ignore the buoyancy in the momentum equation.

p.6 equation 13. We have two l_max here, l_max and l_max_eff. Am I right in understanding that when later on there is a reference to l_max it is in fact l_max_eff ? i.e. the Rossby number as a function of lmax or the fitted values of lmax in Table 2 are referring to values of lmax,eff? Is eq 13 used at all in your model? Probably worth clarifying… especially if I have misunderstood.

p.6. extended turbulence model for unstable flows: am I right in understanding that the buoyancy term B added to the k-e equations is only added for unstable flows. i.e. when you model neutral or stable cases listed in Table 2, you only use the original length scale limited model of Apsley and Castro?

p.8. Numerical set up. How is convergence defined? I'm trying to understand how the overall momentum balance is achieved. We have friction at the ground, so a momentum sink, no wind speed gradient at the top (so no shear driven flow). As friction is reduced or increased I'd expect the boundary layer height to reduce or grow vs time. Do you judge convergence based on the boundary layer growth? Do you prescribe a pressure gradient in the flow direction? Worth elaborating?

p.8. Numerical et up. Boundary condition at the ground: is it using the neutral formulation even when modelling stable or unstable cases? Did you try changing the closure at the wall using stability dependent closures?

p.9. line 4. I find the sentence 'We find that if both lmax and z0 are proportional to …' a bit of a back to front way to introduce Rossby number similarity. My first reaction when reading this was that z0 is usually an input parameter that depends on the ground conditions, therefore why should it be proportional to G/fc. Worth rewriting?

P9. Line 6. The general definition of Ro as a function of U and L where U and L are characteristic wind speed and velocity scales is fine on its own. But in the current context, where the symbol L has also been used for the Obukhov length, the use of L for a general lengthscale is a bit unfortunate. Especially since you proceed using L, the Obukhov length, when later defining RO_L-. I would suggest using a different symbol for L here, may be using a different font.

p.14, equation 25. I know it's common to use the A and B notation for the 'constants' in the GDL, but it's unfortunate that B was also the symbol used for the buoyancy term earlier on. I'd suggest avoiding the use of the same symbol for both.

p.16, line 8. The fact that the A and B parameters in the DGL are function of the stability (via lmax or L) is not new. While not necessarily formulated as a function of the Rossby numbers used in the current publication, their dependence on the Obukhov length and on the Brundt Vaysala frequency has been discussed quite a while ago. See e.g. Landberg for dependence in mu (i.e. Obukhov length)

Landberg, L., 1994, 'Short term prediction of local wind conditions', Risoe National Laboratory, Roskilde, Denmark

or Zilitinkevich (1989) already referred to. This would be worth including in the discussion.

p.18, Figure 8. Plot of the turbulence lengthscale. What is happening with the plot of the very stable case at the top of the ABL? Is the black line really the solution of the 1D CFD?

p.21. I find the discussion somewhat lacking on the fact that the profiles obtained by fitting both G, lmax were well captured, while those where only lmax was varied were not so well captured. Could it be because the role of lmax is to reflect the ABL height, while at the same time be accounting for surface stability effects? Should there be a third lengthscale also entering the definition of the stable profiles so that the role of limiting effects at the surface (via L) and limiting effects at the top of the boundary layer (via the Brundt Vaysala frequency) can be treated independently? This might provide the additional degree of freedom that the model seems to require to fit the measured profiles. (degree of freedom which was provided by allowing the model to fit G). This sort of dependency was proposed by Zilitinkevich and Mironov (1996) and it's use suggested in Zilitinkevich et al (1996). Worth discussing?

Zilitinkevich S.S., Mironov, D.V., 1996, 'A Multi-Limit Formulation For The Equilibrium Depth Of A Stably Stratified Boundary Layer', Bound. Layer Meteorol., 81, pp 325-351.
Zilitinkevich S.S., Johansson P.-E., Mironov D.V., Baklanov A., in press, 'An Analytical Similarity Theory Model For Wind Profile And Resistance Law In Stably Stratified Planetary Boundary Layers', J. Wind. Eng. Industr. Aerodyn, 74-76 (1998) 209-218.

P.21, line 25 '. . .dependence upon two Rossby numbers, which correspond to the roughness length and the maximum turbulence length scale' should be rewritten. 'correspond' is not exactly appropriate, may be use 'defined from' instead. p.22. The Obukhov length is dimensional, while the Rossby number is not. So the sentence '. . .the Obukhov length, which can also be written as a third Rossby number' should be changed. Likely something along the lines of 'The Obukhov length can be used to define a third Rossby number'.

p.22. The conclusion feels like it was hastily written. When I first read the abstract, introduction and conclusion, I could not quite understand what various parts of the conclusion referred to. I feel the conclusions should be improved, so that they can provide a clearer summary of what was done, so they can stand on their own without having to read through the whole article. For example, the content of the sentence 'A model validation of the full ABL for a stable, a neutral and an unstable case is performed, with less success for the non-neutral cases.' could be explained a bit more. i.e. what is meant by a full validation? Also, the results of the validation could be detailed a bit more than the rather succint 'with less success for the non-neutral case'.

Technical corrections:

p.2, line 31. Negative sign missing in front of u'w' and v'w'.

p.2 eq (1). Should be a positive sign in front of the diffusion terms

p.8, line 12. Comma instead of full stop after 'at the ground'

p.14, line 6. Should be proportional to minus a log10(Ro_l) (negative slope in Fig 5) p.24. Zilitinkevich reference is missing the journal.

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