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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.

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General Assessment

Interesting paper extending the range of idealized ABL models to include more realistic scaling in unstable conditions in connection to wind energy design tools. I appreciate the effort of the authors in explaining the derivation of the extended model from original models dating back to Ekman (1905). This, in itself, makes the paper worthy of publication to understand a historical perspective on ABL modeling. The authors convincingly

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Discussion paper

demonstrate the scaling properties of the ABL limited-mixing-length model when using Rossby-based length scales which is convenient to reduce the dimensionality of ABL parameterization. The model has a good theoretical basis to provide a more realistic framework for design tools than traditional surface-layer models while it still struggles at reproducing real ABL profiles that are inherently transient and driven by non-uniform forcing. This is demonstrated with a series of validation cases.

There is some clarification to be made on the use or not of local-scaling in stable conditions and, in general, the use of u* vs G and how this could affect Rossby similarity. This is discussed in page 13 but maybe I should be motivated before the model derivation.

In the conclusions, I miss a more extended discussion about the applicability of this model for wind energy applications and challenges that will arise when dealing, for instance, with complex terrain or wake effects. Will the same length scales apply? Will there be additional length scales?

Major Remarks

In the derivation of the model, we end up having three Rossby numbers related to z_0, l_max (or l_ABL in stable conditions) and L_ length scales. All three use G/abs(fc) to come up with the non-dimensional number. I understand the convenience of using the same velocity scale for all three parameters but somehow it implies that there is global scaling for all stability conditions. Since we know that the stable ABL depends on local scaling (e.g. Nieuwstadt, JAS-14, 1984) through z/L I would like to confirm if this is already implicit in l_max through equation (13). If so, I would suggest that L is defined as the local Obukhov length when it is first introduced in equation (11). Otherwise, if L is the surface Obukhov length (as suggested in page 10-21) then you could expect some difficulties of the model to fit very stable profiles as it might be the case in Figure 8.

Along this local-scaling reasoning, I would find it more appropriate to use u* as a velocity scale in relation to l_ABL which dependes on z/L, with L being a local quantity. Actually, you can generalize l_ABL to unstable conditions by simply using the equivalent Dyer functional forms of the stability function phi_m(z/L). In this alternative formulation you would have z_0, l_max (Blackadar) and L as length scales with z_0 and l_max being global scales and L being local scale. I'm not sure if this would work out in terms of Rossby similarity, as you mention at the beginning of page 13.

Page 6, Equation (13): I would define l_ABL = l_max for z/L = 0 and l_ABL = [1/l_max + beta/(kappa*L)]ˆ-1 and use l_ABL in the definition of Ro_l (21) since l_max is originally associated with Blackadar's mixing length for neutral conditions and you will use Ro_l for both neutral and stable conditions. Then, equation (21) would be based on l_ABL, and not l_max right?

Minor Remarks

Page 1-15: "Such a model should be simple enough to be applicable in the wind energy industry" It sounds a bit like industry could not handle complex models. Maybe, ". . . to be efficiently used in design tools".

For completeness, I think you should mention in the introduction other hypothesis that apply to the model even if some of them are mentioned in the derivation later on: dry-atmosphere, no mesoscale advection, no vertical wind speed, etc.

2-16: Even if you mention them later, please provide references to the "Blackadar-type" models to provide a more meaningful introduction of the type of models that you try to improve from.

5-18: Consider changing the title of the section to "Limited mixing-length model in stable conditions" since the objective is to define l_ABL which, in this formulation, do not include unstable conditions

7-9: For clarity, ". . . all have the same coefficient: C_epsilon,2."

8-12: A rough-wall boundary condition

9-7: L was used before to denote the Obukhov length. Although I think it is clear from the context, I would rather use U* and L* or anything else to denote that these are generic velocity and length scales, not to be confused with U and L elsewhere in the paper.

9-10: “Ro_l is analogous to the reciprocal of a dimensionless boundary layer depth” Maybe you could add the dimensionless boundary layer depth for clarity. This will help when you interpret equation (23) as a ratio of z_i/z_0 (page 10-1)

Equation (22): Following previous comment, you may consider naming l_b = G/abs(fc) as a “master” length scale (not sure about the most appropriate name) since this is present throughout the paper and you also use this to plot non-dimensional height in Figures that follow.

16-9: Why the blue line and not the yellow line with A= 2 and B =4.42 being somehow closer to A = 1.8 and B = 4.5 (Troen and Petersen)?

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