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Interactive comment

Interactive comment on "Departure from K-theory in the planetary boundary layer" *by* Pedro Santos et al.

Anonymous Referee #1

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This paper presents lidar measurements and NEWA-WRF simulation data for two sites, one located onshore and another offshore. The main focus is on the divergence between the turbulent fluxes and velocity vector (and its gradient) so as to link how valid K-theory, i.e. eddy diffusivity approach, is in WRF-like numerical models. Results suggest that WRF struggles to capture the wind velocity vector and turbulence statistics compared to the lidar data, which authors suggest is due to the adoption of the turbulent-diffusivity approach so as to compute the Reynolds stresses. Overall, the paper is well-structured and well-written with interesting discussions from the authors.

The reviewer finds two major issues with this papers, being the first and foremost important the self-plagiarism with the paper Santos et al. 2020 JPCS: 1618. This this cite work authors present already the FINO3 offshore measurements and WRF results

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Discussion paper

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(which is somehow OK) but the text is almost a copy and paste from this other paper. And this leads to the next issue which is the limited contribution to the field in the format of a journal paper and/or lack of sufficient validation to confirm that K-theory is not good enough. Most of the WRF LES results, e.g. this inability of WRF to capture the wind dynamics in the PSL, have been already reported by other researchers as authors cite those works. At the end of Section 3, authors suggest that sub-grid-scale effects are playing a role on the LES results (again citing papers that have shown this already), but this can be analysed refining the grid near the wall so the sgs model contributions diminish, or computing the ratio of turbulent-to-natural viscosity.

In the eyes of the reviewer, the scope of the paper is well thought but the evidences here are not enough. Authors could expand more on more sites to generalise this instead of using two limited locations. Also inferring the physical constrains of the numerical model could be explored much more to validate the main question of the unsuitability of K-theory for the PSL.

Unfortunately, the reviewer recommends this paper to be rejected.

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