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Interactive comment on "Microscale model comparison (benchmark) at the moderate complex forested site Ryningsnäs" *by* Stefan Ivanell et al.

Anonymous Referee #1

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Review of the manuscript WES-2018-20

Microscale model comparison (benchmark) at the moderate complex forested site Ryningsnäs

By Ivanell et al.

Summary: This paper reports on a benchmark study in which a series of numerical models for wind energy purposes is tested for a forested site in the north of Sweden. Several codes are tested for a cases under neutral conditions, and permutations are being made for closure constants. It appears that the models somewhat fit the observed profiles, although a few don't. In more detail the modelled wind shear and TKE differ substantially from the observations for some of the models. Model intercompar-

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isons can be very powerful efforts to learn about differences in model behavior (and not so much as a competition who is the best). The difficulty here is that the models that took part are so different in forcing and domain and grid settings, that it is difficult to draw general conclusions easily. There are a number of points that the paper can be improved upon, which has been listed below.

Recommendations: major revision needed

Major remarks:

Abstract: I think the abstract should be rewritten, it is a bit short and not so informative on what you really learnt. E.g. from the statement that the turbulent closure constants have a huge impact not much is learnt. Try to come with real recommendations.

Benchmark description: Coming from the meteorological community I have some difficulties with understanding the forcings. First the term target wind is unclear. In my view the wind at 100 m is a result of all external forcings which are the pressure gradient, advection and surface friction. So one should better defend the choice to go for a target wind. Personally prescribing a geowind would be more feasible and fair. Secondly it is unclear to me for which day and time the wind profile is forecast. Do we look at a single time slot, or averages over multiple days? In additional the lead time of the forecast is never mentioned. Obviously models needs some spin up, too short spin up may result in poor scores. At the same time relatively long forecasts with a long lead time are expected to be more off from the observation (predictive skill horizon). In addition I was missing whether the models also apply a temperature budget equation that allow for katabatic winds/slope effects since the slope is substantial. Finally, the paper should mention whether advection of momentum in u and v was applied or not.

Discussion: Overall I have the feeling the discussion section of the paper can be improved. Hence a more in depth discussion about the quality of the used observations, a more elaborated discussion of the model results with respect of the model results already reported elsewhere in the literature (i.e. do models behave the same as in the benchmark study). Moreover, also the representativeness of the case has to be improved since it was (only 1 day?) for neutral conditions? Finally, what is the next step in case a new intercomparison should be developed, what should we focus on, a case with more stability, or a case with more/less complex terrain/vegetation? For the scientific progress in the future, some more lights could be shed on these aspects.

Remarks:

P1, ln 2: unclear whether you performed tuning here or whether it is a free forecast. I think readers from the meteorological community have difficulty with understanding the target wind, since the wind at 100 m should be a results of the forcings and the dynamics.

P1, ln 12; reword: new areas. You mean new sites, not new techniques or so.

P1, ln 16: elaborate on wind and turbulence: what is the problem/are the challenges with these variables. You probably mean that wind shear and turbulence should be limited for wind turbines, for load assessment etc..

P2, ln 17: add a comma behind as such

P2, ln 18: coming benchmarks: please be more concrete if you are aware of current initiatives.

P2, end of section 1: please add a few lines that model intercomparison studies have been very fruitful methods to improve the model scores by adjusting parameterizations after the confrontations with other model results and observations, and add a few examples, e.g.

Bosveld, F.C., P. Baas, Gert-Jan Steeneveld, Albert A.M. Holtslag, Wayne M. Angevine, Eric Bazile, Evert I.F. de Bruijn, Daniel Deacu, John M. Edwards, Michael Ek, Vincent E. Larson, Jonathan E. Pleim, Matthias Raschendorfer, Gunilla Svensson, 2014: The GABLS third intercomparison case for model evaluation, Part B: SCM model intercomparison and evaluation, Bound.-Layer Meteor., 152, 157-187.

P3., ln 19: make clear whether the 138 m is above ground level or canopy top level. Idem for P4, ln 6.

P6, ln 3: please correct the definition of u*, a square is missing in the vw part

P6, ln 5: please mention which percentage of the observations fulfilled the criteria. As such we can learn something about the representativeness of the case.

P6, ln 13: Why the experiment wasn't opened for non NEWA colleagues? Perhaps new people could have been entrained in the community

P6, ln 21: notation: above you use the overbar for time averages and here you switch to <>. Please make consistent. Idem for P7 ln 4 were the overbar suddenly denotes the resolved scales of the LES.

P6, table 1: explain how the PALM LES can run in column mode? Is it thereby a RANS model?

P7, section 4.2: here it is a bit unclear to me whether all models use the same descriptions that are presented and general for all of them, especially eq 9 and 11. Equation 11 is also just a parameterization, though not much in use anymore in meteorology since it appeared not to be very useful. Idem for table 2: did all models use these coefficients?

P9 and further: structure. I think it is better for the readibility to make a section 4.2.1 Meteodyn and then 4.2.1a for model description and 4.2.1b for numerical setup. And then for all models.

P9, table 2: elaborate the table caption. Unclear which model uses which closure in this way. Also defend why the combinations of exactly those values were taken. There seems to be no strategy, or the strategy is not presented in the paper.

P10, ln 18: please defend the 13 m/s that was set. Earlier I learnt that the geowind should be predicted and should be compared in the model outcome. Also what was the

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source for the 13 m/s? Was it taken from ERA-Interim or so or from a radio sounding? Please defend.

P17, figure 3: caption: Modelled and observed wind speed profiles...... Please add day and time of the forecast, as well as the lead time. Also help the reader by adding that def are zoom in plots of abc.

P17, ln 7: please correct the overbars.

P17, ln 8: order of referencing of figure confusing.

P18, figure 4, caption: Modelled and observed wind shear

P18, ln 6: overestimate (plural)

P18, ln 8: superscript the 1 of ms-1

P19, ln 18: As can be seen: avoid passive sentence.

P19, ln 19: half of the veer; please refer here to the literature. It is a common problem that NWP models underestimate the turning of the wind with height.

P19, ln 26-27: I am not convinced pressure gradient is not important, since it is the major driver of all flows. Though I agree its relative impact will be smaller than aloft. Please reword or quantify all terms of the momentum equation.

Figure 6: The family of runs that has a low TKE value has a TKE value that appears to be exactly a factor 2 smaller than the others. Some of those model report 2*TKE as the prognostic variable. Could that explain the difference, or perhaps different modellers applying the same model but one of the two did not divide by 2. It is just a suggestion.

Figure 8: The results of this figure should be discussed in more detail. So far I can see different values but the reason behind them remain unclear. Perhaps also better to plot them as histograms of the values over the domain so we can better see the mean value and percentile differences. Also: what time of the day is this?

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P23, ln 4: please unravel the causes behind the differences in more detail.

P23, ln 12: too little

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