Wind Energ. Sci. Discuss.,
doi:10.5194/wes-2016-26-AC1, 2016

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Interactive comment on "Atmospheric boundary layer modeling based on mesoscale tendencies and data assimilation at microscale" *by* J. Sanz Rodrigo et al.

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Received and published: 11 November 2016

Thanks for your comments. Let us address them one by one following your numbering:

0. About the general comment of "the manuscript fails to argue why these experiments are relevant to wind energy application". We believe that this is well motivated in the first two paragraphs of the introduction. Wind energy flow models in industry are still largely based on surface-layer modeling in neutral conditions while turbines and wind farms are getting very large and and "include relevant physics like Coriolis as well as realistic large-scale forcing and appropriate turbulent scaling, dependent on thermal stratification, from the surface layer to the free atmosphere". Hence, the needs for wind energy are clearly stated. When developing these models one has to make

sure that relevant physics are included and their impact quantified systematically. The GABLS series of benchmarks, developed by the meteorology community, is an excellent verification suite for the design of ABL wind energy models. Further motivation about meso-micro is provided in the review paper (Sanz Rodrigo et al., 2016).

1. About "data assimilation" or not. We don't agree that the method can't be called data assimilation. Nudging is a well established data assimilation technique, widely used by the meteorological community. Its simplicity shall make it also a popular method in wind energy where we typically count with site measurements. Anyway, we could agree that changing "data assimilation" by "nudging" will be more specific about the method and will avoid the controversy.

2. About double-counting. We demonstrate that adding advection has value to the predictions before using nudging. Nudging is used at microscale to correct the errors of the WRF-SCM simulation towards the observations. This is not double-counting, advection is a genuine atmospheric force while nudging is not.

3. About the WRF set-up not being appropriate. We don't agree that using a higher resolution domain will miss large scale forcings that a lower resolution domain would capture. On the contrary, if the time step still yields under-critical CFL numbers, the same forcings from the coarser domain will be better resolved with the higher resolution domain. The guidelines from NCAR suggest a resolution ratio of 3 to 5 when changing from one nest to the next as trade-off between the scales resolved in each domain and the computational cost. We use a higher resolution parent domain to have all the nests with the same grid size and use a larger number of cpus in the simulation. This is particularly important in WRF-LES simulations of a follow-up work.

4. About the strong coupling of geostrophic wind and surface temperature. This literal conclusion comes from the assessment of the GABLS2 case, where another diurnal cycle was under discussion. In GABLS2, the surface temperature was prescribed while in GABLS3 it was allowed to respond to the forcings as a result of the surface

model. Holtslag et al. (2007) showed the impact of prescribing the temperature or not and found significant differences in stable conditions. That's why, when designing GABLS3, they decided to allow coupling of forcings through the surface model instead of prescribing the surface temperature.

5. About using velocity magnitude and direction instead of U and V components. We agree that the large changes of advection direction coincide with low advection velocity magnitude. We'd rather use magnitude instead of components because we are talking about forcings at rotor level and, hence, it is more meaningfull to talk about rotor-based quantities of interest that are later use in the validation. Nevertheless, we'll change the test to: "Interestingly, the advective wind direction makes a 360° turn throughout the cycle, although at relatively small advection speed".

6. About Fig.3 meaning. We believe that the caption of the Figure, together with the explanations in the main text, is comprehensive enough to explaing what's going on. Based on the GABLS1 set-up that runs a uniformly cooled ABL to a quasi-steady state, we quantify some characteristics of the resulting profiles based on changes on the driving forcings (cooling rate and geostrophic wind). The colors show different stability classes. We don't add a legend because the contours already include labels.

We'll review the text to include the editorial changes and avoid jargon or informal english.

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Discussion paper