Review report “Tall Wind Profile Validation Using Lidar Observations and Hindcast Data”, Cheynet et al.

In this manuscript three model-based datasets (NORA3, NEWA and ERA5) are validated using Doppler wind lidar data obtained from five locations, including North Sea (FINO1, FINO3) and coastal and complex terrain locations in Norway. Emphasis is given to long-range scanning Doppler wind lidars, providing wind profiles far above the atmospheric surface layer that are relevant for modern wind turbine designs and airborne wind energy (AWE) systems. These altitudes that are not feasible for in-situ wind measurements in tall masts (especially offshore) or the more extensively used short-range Doppler lidar wind profilers that are limited to 200-300m. The validation focuses on altitudes between 100m and 500m, using various error metrics, and their corresponding capacity factors, based on power curves for various wind turbines and AWE systems. The authors claim an increasing agreement between the models and the measurements with height, and argue that those models are valuable R&D on AWE systems.

In general, the manuscript addresses an important point, namely the need to validate models at altitudes relevant for future wind energy systems, and the lack of continuous, long-term measurement campaigns to do so. The authors point to the need of dedicated Doppler wind lidar profilers with sufficient height range, which are indeed lacking commercially right now. The manuscript also highlights the complexity of comparing the performance of various models, and that the best choice really depends on their actual application (type of location, relevant height range, …).

General comments

1. I have some objection to the term “tall wind profile”. Tall is used for physical structures, like masts or wind turbines, but a wind profile cannot be tall. I am not aware that “tall wind profile” is a commonly used term in our community, however, if I am wrong in this (i.e. it is used in more papers), I will drop my objection.

2. The FINO1 measurements are not suitable for validation due to the presence of an operating wind farm (and the models do not include that). Therefore it should be not be included here, also because FIN03 is already available to cover the offshore situation. Only if the data could be filtered to minimize the effect of the wind farm (for instance, if the influence is only present for certain wind directions), its inclusion would make sense.

3. The authors note that the conclusions on the model performances for the different sites might be hampered by the quality of the different Doppler lidar instruments. However, those measurements have been validated with other measurements, as described in Section 2.2. Wouldn’t it therefore not be possible to quantity whether the validation results are significant in terms of the measurement uncertainty or bias?

4. For the wind measurements at the relevant altitudes the authors immediately jump to Doppler lidar in the introduction. But there also other remote sensing instrument that can measure wind between 200 and 500m, like radar wind profilers and sodar. In fact, for this validation study, their temporal and vertical resolution would be more than sufficient. The choice of Doppler lidar should be given a bit more context and motivation.

5. The conclusion that there is an increasing agreement between models and lidar measurements, as stated in the abstract, is not explicitly stated in the main text, including the conclusion. Either the main text is underselling the results, or the abstract is overselling it.

6. I was a bit surprised that although the paper emphasis the need for wind profile beyond what can be reached by traditional masts and (floating) short-range wind lidars, still most of the presented results are at an altitude of 150m (for which, by the way, there are much more lidar data available, including offshore). Why this particular choice?

Specific comments

A. Title: "validation using lidar observations and hindcast data". Are you not validating hindcast data using lidar observations?

B. Table 1, why this table is in the manuscript? To make the point that there are very limited amount of tall towers with in-situ wind measurements, such a table is not required.

C. Section 3.3: It is not explained how the wind profile is used in the calculation of CF for wind turbines. Is this wind speed at hub height taken or a rotor average. Table 4 provides hub height and rotor diameter of the various wind turbine types, but nowhere it written how this information is used. This is in contrast to the extension discussion on the AWE system.

D. In correct usage of term "In-situ" throughout the manuscript. Doppler lidar is a remote-sensing instrument and definitely not "in-situ"! However, in distinguishing between model and measurement data, in several parts of the manuscript the term "in-situ" is used for Doppler lidar, which is wrong. This needs to be corrected.

E. The distinction coastal and complex locations from Figure 2 is not clear (at least for the non-Norwegian reader). Would a zoom-in of the map help to clarify the difference between the Sola and Lista as coastal/non-complex, and Bjerkeim as complex terrain?

F. Section 5.2: Could you be more explicit, or give examples, on what you mean with "microscale models".

G. Section 5.2: At the end of this section the issue of Doppler lidar wind profiling measurements in complex terrain is mentioned. This is a relevant point, but doesn't belong to this section (which is about the models). Maybe this issue should be discussed much earlier in the paper. Are there solutions to this issue, or would validation in complex terrain remain problematic?