

The manuscript "Norwegian hindcast archive (NORA3) – A validation of offshore wind resources in the North Sea and Norwegian Sea" by Solbrekke et al. is validated the offshore wind using NORA3 dataset. Although the mesoscale NORA3 dataset is new, there is no innovative contribution to the literature. In my opinion manuscript need a major revision to publish. I have following major and minor comments.

Major comments:

1. It is not clear that why the authors use NORA3 for their study. They gave Table1 which shows the related datasets and also, they emphasized that the NEWA data also prepared similar to NORA3. But they did not make neither qualitative nor quantitative comparison between these two datasets and explained why they used or what is the superiority of NORA3. So, it is not clear why the authors prefer NORA3 instead of NEWA.
2. I believe that the length of the simulation does not provide enough information about the climatology of the region to select the best offshore wind farm areas. In my opinion, if the authors want to define the best offshore wind production areas, they should keep the simulation period as long as possible to include extreme atmospheric conditions and interannual variability. Moreover, If I would have preferred NORA3 instead of longer simulation and finer temporal resolution NEWA, I would rather simulate the model for fine horizontal simulation such as 1km. Thus, it might help to solve the mesoscale circulations such as sea-land breezes at near-coastal wind farm that can affect the power productions.
3. It is not clear to me that how the model downscaled 31km ERA5 data to 3km HARMONIE-AROME domain. According to the authors explanations, I think ERA5 is directly downscaled to 3km that I have concern about this sharp interpolation affects the performance of the model.
4. Wind speed interpolation should be conducted by using log-profile wind calculation. The authors did not give information about model general tendency to atmospheric stability or any characteristic of the domain stability. Therefore, logarithmic wind profile can be used for the interpolation.

Minor comments:

Line 52 “WRF model running 10 separate model runs for 10 independent regions” should be ...running independently for 10 domains.

Line 80 “model runs with a horizontal resolution of 31 x 31km” should be 3x3km.

Line 83 “by many European countries” instead of the countries Weather Services should be proper.

Line 95-96 “from the National Centers for Environmental Protection (NCEP) (1° resolution)” there are different horizontal resolution NCEP data be specific for that. I think it should be NCEP-FNL data.

Line 190 “Hence, the observed wind speed is somewhat more intermittent and variable than the modeled wind speed, indicating that HARMONIEAROME is missing some of the high-frequency variability embedded in the wind field”. I think this sentence conflict with the previous sentence, the authors stated that the observation wind speed

is between 4.7-6.5 m/s. I understand that the modeled wind speed is more variable but wind speed between 4.7-6.5 m/s is less seen from the model outputs.

Line 191 “HARMONIEAROME” should be HARMONIE-AROME.

Line 193-196 Weibull distribution notation I recommend to use common parameters terminology for Weibull distribution which the readers are familiar instead of  $a$  and  $b$  notations.

It is interesting to see the dominant direction missed by the model especially at Sleipner where the flow is channeled. I would expect the almost bidirectional wind as we can see on the observation in Figure A0 for Sleipner (NW-SSE direction). How does the authors explain this? Do they believe the model capture the common flow patterns?

I recommend the authors use meteorological wind direction notation instead of given the direction in a degree unit. For example, 0:29 NW, 30:59 NNW in the text and also in Figure B2.

Also I saw “wind park” many places in the manuscript. I recommended to use “wind farm” (line 146,228,230).