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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Towards the North Sea wind power revolution" *by* Jens N. Sørensen and Gunner C. Larsen

Anonymous Referee #1

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Review wes-2018-53

This paper aims to determine the optimal price and spacing for wind turbines deployed in the North Sea to provide 50% of Europe's electricity.

Assumptions made in this analysis such as Ct=0.8 and the mean wind speed of 9.7 m/s at 100 m height are unnecessary. WRF modelling would be able to provide a complete wind resource estimate for every location in the North Sea or wind speeds from satellite maps that are derived at DTU could have been applied. See e.g. Hasager, C. B., Astrup, P., Zhu, R., Chang, R., Badger, M., & Hahmann, A. N. (2016). Quarter-Century Offshore Winds from SSM/I and WRF in the North Sea and South China Sea. Remote Sensing, 8(10), [769]. DOI: 10.3390/rs8090769. Equally information about the turbines deployed is available.

The fundamental assumption is (p3): 'As scale parameters we employ λ = 11 m/s and k = 2.2, corresponding to an average wind speed of 9.7 m/s, at a 100 m altitude. The numbers are taken as averaged values from measurements and simulations of selected locations in the North Sea (see Pena and Hahmann, 2017).' In other words, the wind resource used in the analysis doesn't vary according to the location in the North Sea, which we already know from DTU Wind Atlas (and many others) is incorrect. The mean wind speed at a given height over the North Sea in the DTU wind Atlas varies by at least 2m/s from north to south, even disregarding the impact of (land) topography, distance to coast and so on. In other words, we already know there is a $\pm$20% variation on the mean wind speed, and more on the potential power which is not utilized in this study. How big is the uncertainty introduced here? Is it greater than the uncertainty in the spacing proposed? How does this propagate through the pricing?

After further consideration in the manuscript, an expression is derived for the power density per unit area that depends on the spacing of the turbines given the rated power and an assumed power coefficient. This calculation is one that has been reworked by quite a few authors already – it might be good to cite some of them to give an idea of the range.

The section on bathymetry is quite confusing. Why can't a contour/image map or similar be provided rather than the bathymetry along a line? There are quite a few (web) services that offer (free) download of bathymetric data and it is already stated that the authors have access to this dataset.

Similarly, the cost models seem to be based on rather straightforward assumptions, some which are a bit out-of-date.

The results for power density are quite similar to a few other studies that indicate what is generally known – there is a very large uncertainty – we know the power density for wind energy is somewhere around 2-6 Wm-2 (see also Jacobsen PNAS and then a whole raft of papers by others who argue it is around 1 Wm-2) and agrees with studies like the ons cited (by Frandsen 2009).

The capacity factors seem low here (Figure 6) – cf Wiser who states the actual average in the US (not offshore) is around 41%?

Given the assumptions in the above with a non-varying wind speed, Figure 7 is a scaled version of the bathymetry distribution in Figure 3 and Figure 4 likewise to Figure 8 (The authors state this in the text). Some of the figures are redundant or could be combined to provide a more interesting analysis.

Fundamentally then, it is incorrect then to state that Figures 9, 10 and 11 are: Figure 9: Area required to produce Europe's power demand as function of spacing and rotor diameter. Figure 10: Installed power required to produce Europe's power demand as function of spacing and rotor diameter Figure 11: Number of turbines required to produce Europe's power demand as function of spacing and rotor diameter.

because all of these depend on the assumed wind resource and will be different for different wind climates. (At the very least this should be stated in the captions).

Much of this analysis could be done by simply looking up that in 2016 Europe's electricity demand was 3.1 million GWh or 3100 TWh equivalent to (as stated in the paper) around 0.4TW and assuming wind energy extraction of 1-4 Wm-2

(https://ec.europa.eu/eurostat/statistics-explained/index.php/Electricity_production,_consum

gives an area as stated in the manuscript of around 400,000 km-2 for wind energy extraction of 1 Wm-2 or maybe half or a third of that if the energy extraction rate is 2 Wm-2 or 3 Wm-3 respectively which you can then compare to the area of the North Sea (590,000 km2). You could assume a capacity factor of 0.4 and simply calculate the number of 13 MW turbines based on that. The number of turbines/power per unit area is given essentially as the inverse of the square of the spacing (from Frandsen).

We know already that a major factor in the cost is the actual resource – so if this is not factored in it ought to be included in the uncertainty. The scaling applied here is

about what power coefficients are assumed since wind speed variability and its impact on wake losses are not considered.

The O&M numbers in Figure 16 go against results indicated in terms of turbine lifetimes and O&M on land (do costs decrease over time?). There are a number of simplifying assumptions used such as such scaled turbine costs from 2003 and 'to arrive at reasonable realistic LCoE estimates we will, in line with Mahulja (2015), assume that cost of WT's, internal WF grid and foundations accounts for 75% of the total investment costs, which is based on experiences from the Danish Horns Rev and Nysted offshore wind farms. The remaining 25% is mainly due to electrical infrastructures, such as onshore cables and substations.' This is of course directly reflected in the results but a more sophisticated analysis would show some variation with distance to the coast etc.

If the authors decide to proceed, at the very least they must make clear that their results are entirely based on 'an average wind speed of 9.7 m/s, at a 100 m altitude' over the whole area and list the other simplifying assumptions in the other parts of the manuscript in the conclusions and the abstract and comment on how these assumptions are propagated as uncertainty. If on the other hand they are able to use the results they have available in house on the wind resource etc, this would make the work significantly more relevant.

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