Response to reviewer 1

We would like to thank the reviewer for the interesting insights and constructive comments made during the critical assessment of our work. Their thorough review has greatly enriched the quality and depth of our manuscript. In what follows we address the reviewer's concerns point by point. We give a motivation to the changes, and we mention the additions to the manuscript (*in italic*).

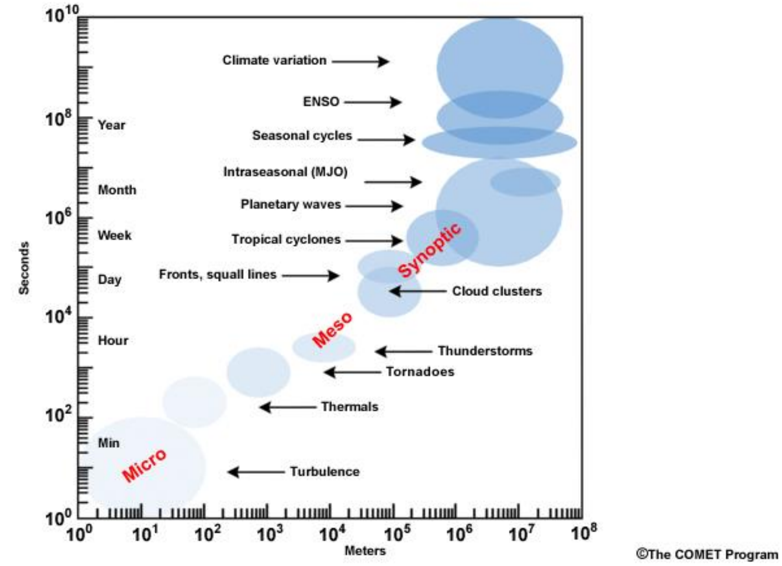
This is a well written paper on an important topic. The paper examines the spatial and temporal variation of so-called mesoscale winds over an area with high off-shore wind energy potential. The authors show that although considering the mesoscale part of the wind speed variability doesn't change the annual power output significantly, it introduces variability into the power output. This variability is shown to peak in summer, and to have a more pronounced diurnal cycle with a daily peak at around 5pm. It is also shown to peak around coastal areas, although the authors do discuss the fact that model spin-up away from the boundaries may influence this result.

Reply: Thank you for pointing out the importance of this paper.

General comments

1. I really like the attempt to separate the 'mesoscale' and 'synoptic-scale' winds. However, I'm not really sure what physical processes are being picked up by this approach. The 'mesoscale wind', is designed to align with the effective resolution of the model, rather than any physical arguments. The 'synoptic-scale wind', calculated over a 45kmX45km grid box, is still well within the stated length-scale of 100km for mesoscale weather systems. For example, a sea-breeze disturbance could cover the whole larger box, as could a large thunderstorm complex. Can the authors try to quantify/justify this choice of length-scales? Some kind of spectral analysis or spatial filtering with known spectral properties could be helpful here. Either the length-scales should be clearly justified by physical arguments about the scale of mesoscale weather systems, or by practical arguments about the scale of variability that is of relevance to large offshore wind farms.

Reply: We agree with the point you bring up here and therefore added the following text to the manuscript. See also the figure hereunder which is not added to the manuscript. We also would like to mention that even though the sea-breeze indeed covers the whole box, several sea breeze episodes are picked up by the algorithm, since large variability occurs within the 45kmX45km grid box. The effective resolution is used since variability at smaller scale is not accounted for by the model.



The size of the small window is determined by the effective resolution of the simulation, since variability at smaller scales is not accounted for by the model. The effective resolution of COSMO is approximately 10 × 10 grid points (Kapper et al., 2010) (∼15 km). (...) With the 45x45 km² window we average out thunderstorms and smaller mesoscale systems, whereas cloud clusters and fronts are not. This is also the scale of variability that can result in sudden power ramping events for wind farms. Even though this is not strictly speaking the mesoscale length scale (100 km), we will refer to it throughout the rest of the manuscript.

2. When discussing the variability that the authors call long time-scales (6-12 hours), the focus is on diurnal processes over the land influencing the wind over the sea. However, there can also be a diurnal cycle over the water, especially in shallow areas where the water can warm or cool more rapidly. The authors did not state how the SSTs were specified in the model, nor whether they were being updated on daily or sub-daily time-scales.

Reply: In the model setup section we have added the following statement regarding the SST's being provided by ERA5, and we have added a section to the appendix discussing our findings from a test run we did with a constant SST.

The sea surface temperatures used in COSMO are inferred from the ERA5 reanalysis dataset. The SSTs do change in our simulation on a sub-daily timescale, but the diurnal cycle of these SSTs is at 1 to 2 K relatively limited. This cycle is comparable to the diurnal cycle found in potential sea surface temperature of the Baltic Sea Physics Analysis and Forecast provided by CMEMS (Lindenthal et al., 2023).

(Appendix): We used some test runs to get to an optimal simulation setup, and in one of these test runs the sea surface temperature (SST) was kept constant at the initial (January) level. When comparing the spectrum for both runs, we find on longer timescales no difference the intensity. On the shorter timescales there is however a higher intensity for the simulation with changing SST. As the SST does not change significantly over the daily timescale this higher intensity on short timescales might be because of the average SST in winter being higher than the SST from the first of January, thereby inducing more convective situations. In summer we find a higher intensity for the longer timescales for the simulation

with a constant January SST. An enhanced contrast between land and sea surface temperature might thus result in more wind speed variability on long timescales related to the land-sea breeze system.

3. The authors looked at both temporal and spatial variability, but it would have been nice to see a greater attempt to relate these results. In particular, given that the temporal and spatial analysis should be capturing the same thing, why was it necessary to work with the spatial methods that place limitations on coastal areas? Why could the integrated periodograms over 'mesoscale' and 'synoptic-scale' periods have been compared, in a similar way to the MSVI? This would have given well-resolved maps of where the mesoscale variability was playing an important role?

Reply: Mesoscale systems are indeed by definition bound in both space and time, and therefore we examined mesoscale variability in both space and time. In the spatial analysis there was a limitation placed on coastal areas in order to limit the influence of the wind speed-up when transitioning from land to sea. The results of the MSVI metric become harder to interpretate when one of the windows contains a disproportionate amount of land pixels. The MSVI and the Welch method are quite complementary to each other, as one method bundles temporal information into a spectrum and the other bundles spatial information into a metric. The MSVI metric was designed to identify moments in time with strong spatial variability within an area of 45x45 km^2, thereby identifying the intense mesoscale systems. It therefore is not so interesting to create a map of the MSVI. The maps of the spectra integrated over different timescales have been included.

The Welch method and the MSVI metric complement each other. The Welch method produces a spectrum with information about wind speed variations over different timescales for every pixel. Bundling this information in a spectrum does however remove the temporal resolution of that time series. The MSVI on the other hand aggregates spatial information into a metric, and in doing so gives up some spatial resolution. The temporal resolution in this method stays intact. As mesoscale systems are by definition bound in both space and time these two methods together offer a more complete view on offshore mesoscale wind speed variability than one metric would yield on its own.

Specific comments

1. Page 3, line 41: "The effects of turbulence can be taken into account in Large Eddy Simulations" -> I think it should say "partly taken into account", since LES models only capture the larger part of the turbulence.

Reply: This is changed in the manuscript.

2. Page 3, line 53: "the majority of research is based on the onshore extent of the systems" -> There are a few references that look at the offshore part of land-sea breeze circulations that might be missing here. For example, Short et al. (2019) and Gille et al. (2005). In this context, the authors should also mention the land-breeze, which may be more relevant for offshore winds.

Reply: These two references are indeed very interesting and are added to the manuscript

The offshore part of sea breezes can have an influence on the power output of a wind farm as it, in general, opposes the synoptic wind flow (Steele et al., 2015). During the night land breeze systems have the potential to generate offshore mesoscale wind speed variability (Gille et al. 2005 and Short et al. 2019).

3. Page 2, line 38: There are more recent versions of the wind speed spectrum that you could refrence - eg. Kang et al (2016).

Reply: The data used in Kang et al. (2016) is taken from a continental location. For our paper we therefore opted to include a spectrum from a more coastal climate, such as Van der Hoven (1957) and Larsén et al. (2016).

4. Page 3, line 45: Change first sentence to "Less is known about the impact of mesoscale weather systems on wind variability, for example in organised convection"

Reply: This whole paragraph has been restructured and does no longer include the sentence: "*Less is known about mesoscale weather systems, for example in organised convection.*" Now this paragraph starts with: *"Variations in wind speed can also arise from mesoscale weather systems."*

5. Page 3, line 55: Add reference to Trombe et al. (2014).

Reply: This interesting paper is quite relevant and is added to the paper.

6. Page 3, lines 45-60: I think this section is missing discussion of a major source of mesoscale variability, which is from organised thunderstorms or MCS.

Reply: A few lines on the topic of MCSs are added to the manuscript.

A class of convective systems that is not completely understood yet are the mesoscale convective systems (MCS) (Houze Jr, 2004). An overview of the different theories explaining the mechanisms behind MCS is summarised in for instance the introduction of Short et al. (2023). The land-sea transition can lock these MCSs in place (Xu et al., 2012), potentially affecting offshore wind farms.

7. Page 4, line 100: What height is the ERA5 wind speed accuracy quoted for?

Reply: As suggested by the other reviewer, this statement is removed from the paper.

8. Page 5, line 109: 'aggregated' - clarify - is this interpolated, or averaged?

Reply: We have clarified in the manuscript that we mean averaged here.

9. Page 5, line 130-133: The authors state that the time series is cut in overlapping sections. Later, it says that a ‘Hann window is used to cut the signal into sections’. Is this duplication?

Reply: This paragraph is slightly restructured to remove the duplication.

The spectral density of a signal is estimated using a periodogram calculated with the Welch method (Welch 1967). Due to the uncertainty in a signal such as a time series of the wind speed, we can only make an estimation of the underlying spectrum. In the Welch method the 10-year time series of 10-minute interval wind speeds is cut using a Hann window in overlapping sections of approximately seven days (1024 output intervals) in our case, with an overlap of 50%. Subsequently the spectral density of every section is calculated using a Fast Fourier Transform (FFT) algorithm. The Hann window reduces the reflections arising from performing an FFT on a finite time series (Blackman and Tukey, 1958)

10. Page 5, line 134: ‘fast natural variability’ - is it ‘fast’, or just removing the noise?

Reply: We can’t really call it noise, as noise is considered an alternation to the original signal due to measurement imperfections. Our simulation should in principle not introduce something that can be classified as noise.

11. Page 6, line 135: ‘good estimate’ -> how do you know it’s ‘good’?

Reply: Good was meant to indicate that we consider the number of periodograms that are averaged to be large enough to converge to the underlying spectrum of wind speed variations. As this is never proven in the paper this statement of it being good is confusing, and “good” is removed from the sentence

12. Page 6, line 137: ‘integrated over a 3-month interval’ - what does that mean?

Reply: This is indeed a confusing statement so we removed it.

13. Page 6, line 144: ‘period’ -> can the authors choose a different word? This could mean ‘a period of time’ or ‘periods from the spectrum’.

Reply: We have used “time interval” where possible to alleviate confusion for the reader.

14. Page 7, line 169: suggest changing to “As the small window is contained within the large window’

Reply: This is changed in the manuscript.

15. Page 7, line 170: ‘everywhere’ -> ‘everywhere else’

Reply: This is changed in the manuscript.

16. Page 10, line 220: The comparison with lidar data at high frequencies is mentioned, but as far as I can see, this is not shown in the figures.

Reply: A reference to the figures of the comparison of the model periodogram and the lidar data in the appendix is added to the manuscript.

17. Page 10, line 225-227: Why would the diurnal effects only show up on the 12h time-scale, and not the 24-hour time-scale?

Reply: Masouleh et al. (2019) show that quite a large portion of sea breeze events have a duration of less than 12 hours. That is the reason why there is not one single peak in the spectrum shown in figure

5. While we believe that the temperature difference between land and sea is a driver for wind speed variability at these time scales (since the variability on these timescales is larger in summer compared to winter) the systems that this temperature contrast induces are apparently not always following this diurnal cycle.

18. Page 11, line 230: It would be useful to contrast/compare the results to Vincent (2011).

Reply: Indeed, the results of Vincent et al. 2011 are quite similar and a comparison is added to the manuscript.

19. Page 11, line 240: The authors mention the issues of spin-up around the edges of the domain. This is an interesting problem, but raises the question of whether the boundary removed from the edges was sufficient. Can we really trust the results, give these effects?

Reply: The appendix on the nesting strategy includes a small discussion on the tests we performed with a larger simulation domain. A reference to this discussion has been added to the model setup section.

Different simulation setups have been tested for 3-month integrations. These tests included a larger domain (340 x 360 grid points compared to 180 x 184 grid points), adding an intermediate nesting at 12 km resolution and applying spectral nudging. We found no added value of using a larger domain, of adding an in between nesting step and of applying spectral nudging compared to the wind data from scatterometer. This result is in line with the findings of Ban et al. (2021) where different nesting strategies of COSMO-CLM in ERA-Interim do not show any substantial differences.

20. Page 11, line 246: Can the authors show the periodogram for power, as well as the integrated maps? The power time-series presumably has some constant sections where the wind speed is greater than 15 m/s or less than 3 m/s, and possibly some sudden jumps due to the cut-out speed being reached. What impact do these shocks have on the periodogram?

Reply: These shocks elevate the spectrum quite a lot compared to the wind spectrum, but as these shocks don't necessarily happen at specific time intervals there are no clear peaks in this spectrum. The shape of the spectrum remains similar to the wind spectrum.

21. Page 14, figure 9: I don't find this graph very useful. What is it supposed to be showing? Could the authors show it as an average annual cycle, averaged over the 10-year period? Or overlay a smoothed version so that the curve is more obvious?

Reply: This graph is supposed to make clear to the reader that the MSVI results in a 1-D time series. This point is clarified in the text, and the figure is removed from the paper.

The MSVI metric quantifies spatial variations in wind speed, and high MVSI values should indicate the presence of a mesoscale weather system. Indeed, looking at the wind fields associated with these peaks, a variety of mesoscale systems are clear, such as convective and sea breeze systems developing over the Kattegat.

22. Page 15, line 269: Why are the 'mesoscale winds' always more than the 'synoptic wind speed'? This is attributed to the land-sea breeze circulation and other mesoscale phenomena. In some cases, this might not be the case, since if the sea-breeze opposes the background flow, then it will weaken the wind overall. I agree that usually, mesoscale phenomena would lead to more windy conditions, but it should not be assumed that this is the case.

Reply: It is true that the MSVI detects a locally elevated wind speed in each time step. Some mesoscale systems do indeed generate lower wind speeds, but due to the local nature of mesoscale weather systems a place with elevated wind speeds should be found in the vicinity of that mesoscale system. While our method is definitely not perfect the MSVI has the potential to capture this variability in wind speed created over these relatively small spatial scales.

23. Page 15, lines 270-272: This section lacks references

Reply: A reference for the maximum of the surface temperature in Denmark has been added to the manuscript.

Both the mean MSVI and its diurnal amplitude is higher in summer (JJA) than winter (DJF) (fig. 10). In winter, wind speed variability is found to be more or less constant throughout the day. In summer the MSVI clearly peaks in the afternoon, when the surface temperature over land reaches its maximum (Jensen, 1960), confirming an influence of the land on the mesoscale winds over sea.