Author response to review

wes-2026-11

We thank both reviewers for their input and comments and have acknowledged your contribution in the acknowledgements.

Our responses to your comments are shown in blue below each point. Line and figure numbers quoted are from the revised manuscript, unless stated otherwise. We have included a tracked changes document.

In addition to changes based on reviewer comments, we have also added some text around two points:

- Coastal low-level jets: We have recently engaged with the literature on these phenomena and believe they may be relevant for interpretation of some of the findings here. We have added some literature and discussion on lines 33-35 and lines 302-305.
- Cut-in wind speed: We have rearranged the text in Section 2.4 to imply that the cut-in wind speed of 3 m/s comes from the National Laboratory of the Rockies (formerly NREL) power curve data for the 10 MW reference turbine (line 195). This is because we realised that the cut-in wind from the power curve speed differs slightly from the cut-in wind speed of 4 m/s reported by the IEA for the same turbine (see Table 9 on page 36 here).

We also found a small error in our code, where a 22.5 m/s cut-out wind speed was used in the capacity factor calculation, rather than the 25 m/s value as stated in the text. We have re-computed the capacity factors with the correct cut-out of 22.5 m/s and updated the relevant figures in the revised manuscript. This makes very little (or no observable) difference to the results and does not impact any of the findings here.

Reviewer 1

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General comments

This paper investigates the impact of sea breezes on the diurnal cycle of offshore wind energy capacity factors across eight potential offshore wind areas in southeastern and southwestern Australia. The authors use a novel sea breeze frontal object dataset derived from the BARRA-C2 reanalysis (1979 - 2024), combined with a moisture frontogenesis diagnostic. The key findings are that sea breeze days tend to have higher afternoon capacity factors (15–30% more available wind resources for six out of eight areas), that the timing of peak wind energy production on sea breeze days aligns favourably with peak electricity demand, and that anti-correlations in sea breeze occurrence across opposite-facing coastlines have implications for portfolio diversification

The paper addresses a timely and relevant topic for the Australian energy transition. With offshore wind farm development actively being pursued in these regions and no operational offshore farms yet, understanding the diurnal characteristics of the resource - and specifically the role of sea breezes - is valuable for energy system planning. The combination of a mesoscale sea breeze identification method with a wind energy resource framing is novel and well-motivated.

Overall, the paper is well-written, logically structured, and makes a useful contribution to the field. The methodology is generally sound, and the authors are transparent about limitations. I recommend minor revisions to address several points that would strengthen the paper.

Thank you for this feedback! We have replied to your comments and suggestions below.

Concerns and suggestions

- P3-4L80-100: One area that could be improved is the description of the filtering and post-processing steps for the frontal objects. The cascade of filters - spatial smoothing, morphology criteria, constraints on object-averaged variables, positive temporal trend in specific humidity - is described qualitatively, but the specific thresholds and implementation details for some of these steps are only available in the companion paper (Brown et al., 2025b). Providing a concise summary table of all filter parameters and thresholds in this paper (perhaps as a supplement) would make the method more self-contained and reproducible without requiring the reader to cross-reference multiple sources.
  We agree with the reviewer that including some more details on the sea breeze object methods would be beneficial for readers, making the manuscript more self-contained.

  In response to this comment, as well as comments from reviewer 2, we have added several details in Section 2.1 of the revised manuscript, including on the smoothing process (lines 88-94), threshold selection (line 96-102), and object filters (lines 103-110). Following the suggestion of the reviewer, we have also put a table in the Supplementary Material with more details on the sea breeze object filters (Section S1). This included moving the discussion on the filter for the time trend in specific humidity, from the main text (in the submitted manuscript) to Table S1 (in the revised manuscript).

- Figure 4 (P11): The authors show that the seasonal cycle of sea breeze occurrences broadly follows the monthly mean daily maximum land-sea temperature difference (T_land - T_sea), but acknowledge that this metric cannot explain all regional variations (e.g., Newcastle peaking in October rather than December-January) and that there is no consensus on how to define it. This is a thoughtful discussion, but the practical utility of T_land-T_sea as a predictor could be strengthened by reporting a correlation coefficient between the two seasonal cycles across all regions.

Good suggestion, we have now reported the Pearson correlation coefficient (0.74) in the Results (line 246) as well as in the Discussion (line 293).

- Figure 5 (P13): This is a key figure. The use of local standard time on the x-axis is practical, but makes direct comparison between SE Australian sites (AEST, UTC+10) and SW Australian sites (AWST, UTC+8) slightly awkward. The authors should ensure this time zone difference is clearly noted so readers do not inadvertently compare absolute times across the two panels.
  Thanks, we have now labelled the horizontal axes with AEST/AWST in bold and mentioned the different time zones in the figure caption.

- The discussion of how prevailing wind direction influences sea breeze type and, hence, offshore wind speeds is qualitatively interesting (P22 L379-393). However, the connection between the wind direction analysis (Figures 6-8) and the capacity factor composites (Figure 5) could be made more quantitative. For example, could the authors condition the capacity factor analysis on prevailing wind direction categories to demonstrate this mechanism more directly?
  We believe that we have already discussed prevailing wind direction a lot in the manuscript, and so are reluctant to add more analysis on this in the main text. However, in the Supplementary Material of the revised manuscript, we now present SBF day frequency for four different prevailing wind regimes, as well as the afternoon capacity factor for each regime on SBF days and non SBF days (Section S5). This is briefly mentioned in the manuscript (line 317-321) and discussed in the Supplementary Material (Section S5).

- The detailed diurnal composites (Fig. 5) focus on the austral summer. While this is the season of peak sea breeze activity (Fig. 2), sea breezes do occur in shoulder seasons - particularly in lower-latitude sites like Newcastle, where occurrence peaks in October. Energy demand patterns also differ outside of summer. A brief discussion of what might be expected in other seasons - or a supplementary figure showing spring (SON) composites - would round out the analysis.
  We have now repeated Figure 5 for each of the other seasons of the year (MAM, JJA and SON), with results shown and discussed in the Supplementary Material (Section S3). We agree this is important to include, as the results show that some of the findings for DJF do not generalise to other times of the year. This is discussed in Section 3.2 of the revised manuscript (lines 287-294) and Discussion (line 404).

- Related to the point above. I would encourage the authors to more explicitly quantify the practical significance of their findings. For instance, what is the magnitude of the sea breeze contribution to total annual energy production at each site? The current framing focuses on the austral summer (DJF) and on relative differences between SBF and non-SBF days, but a reader interested in energy planning would benefit from understanding how often sea breezes occur and how much energy they account for in absolute terms across the full year.
  Thanks for this suggestion, we appreciate that this work could be made

potentially more impactful by presenting the results in this way. Although it would be useful to quantify the energy *only* from the sea breeze, we don't think this is possible in our current framework. This is because the sea breeze occurs at the same time as several other wind processes, both on larger and smaller scales. Therefore, we retain the method of quantifying the impact of sea breezes by considering SBF days and non-SBF days, but with the addition of the following (considering the reviewer comments):

- The difference in capacity factor and the corresponding energy generation between SBF and non-SBF days, assuming a 2.2 GW offshore wind farm (see Table 2 and lines 412-419 in the Discussion). This is intended to provide more relevant information for energy planning purposes. Table 2 is for December-February, in line with the results presented in most of the manuscript, with an equivalent table for the entire year presented in the Supplementary Material (Section S3).

- A table with the SBF day frequency for each coastal land area, including an annual total as well as for each individual season (Table 1). This is intended to give more concise information on how often sea breezes occur in each region (complementary to the spatial maps presented in Figures 2 and 3).

Minor and technical comments

- P3L85: The moisture frontogenesis threshold of 16.1 g/kg/100 km/3h is quite specific. While the authors refer to Brown et al. (2025b) for justification, a brief note on the sensitivity of results to this threshold would be helpful for readers of this paper.
  We have now added some more details on the threshold. This includes details on why we used the specific value of 16.1 (line 97), as well as some brief discussion of the sensitivity tests performed in our previous paper, Brown et al 2025b (lines 98-103).

- P7L162-175: The power curve is interpolated using cubic splines with a cut-in at 3 m/s and a cut-out at 25 m/s. Is there any hysteresis modelled for the cut-out behaviour? This is a minor point, but it could affect capacity factor estimates during high-wind events.
  There is no hysteresis behaviour modelled in the power curve, and this is now stated in the revised manuscript (line 186).

- P7L162-175: The paper focuses on capacity factors from a single turbine and does not discuss wake effects, which would reduce actual energy production in an operational wind farm. A brief note acknowledging this limitation would be appropriate, particularly since sea breeze wind directions may differ from the prevailing directions for which farm layouts are optimised.
  This is a good point and is now mentioned in the Data and Methods section (line 188-190) as well as in the Discussion (line 467). We think the reviewers point

that the sea breeze may modulate the wind direction and therefore the wake dynamics is very interesting, and have noted it as a potential topic of future work (line 469).

- The use of a single 10 MW reference turbine is acknowledged, but it would be helpful to briefly discuss sensitivity to turbine choice, particularly since some of the offshore wind areas with lower mean wind speeds may be better suited to turbines optimised for lower wind regimes (e.g., larger rotors relative to generator capacity).
  We have now discussed this as a limitation in the Discussion section (lines 465-467).

- P7, Section 2.4: For the demand data analysis, the authors should briefly note whether the demand data represents operational demand (which is increasingly depressed during daytime hours by behind-the-meter rooftop solar PV in Australia) or underlying demand. This distinction matters for interpreting the alignment between sea breeze capacity factors and demand peaks (P22L370-375).
  We have used operational demand data, and have now referred to this as such throughout the revised manuscript.

Reviewer 2

This well written manuscript deals with sea-breezes in connection with off-shore wind energy production. The analysis is based on the BARRA-C 4.4 km horizontal grid spacing reanalysis product.

A new method, based on "moisture frontogenesis" is used to determine the sea-breeze front. The results are interesting both from a theoretical and applied point of view.

Thank you for your comments, we have responded below in blue.

A few comments:

1. Line 82 "sufficiently", please be more specific.
   We have now specified in the revised manuscript that "sufficiently capture the sea breeze" refers to the fact that the 4.4 km BARRA-C is expected to broadly capture the mesoscale sea breeze structure, compared with coarser models (lines 85-87)

2. Line 83: Provide details on the Gaussian smoothing filter and how effective it is.
   We have provided some details on this in the revised manuscript, including more of a description as well as justifying the choice of smoothing and potential sensitivities, also in response to reviewer 1. See Section 2.1 (lines 88-94).

3. Line 87. Please discuss the sensitivity of the choice of the threshold value for the frontogenesis
   In the revised manuscript we have described where the value of 16.1 came from,

as well as a brief discussion of the sensitivity tests performed in our previous paper, Brown et al 2025b (lines 95-102).

4. Line 87: How does this value compare to similar threshold values in other studies.
   It is difficult to say because of the different grids/datasets used to compute the quantity (the magnitude of spatial gradients are sensitive to grid size), as well as the fact that moisture frontogenesis has not been used previously in the context of sea breeze object identification.

   Arnup and Reeder (2007) highlight regions order of magnitudes smaller (2.0 x 10^-10 g/kg/m/s) than our threshold (1.5 x 10^-8 g/kg/m/s), but this was done in the context of quantifying the large-scale northern Australia dry line, from coarser model data (0.375-degrees) than used here.

   In Kraus et al. (1990), an example sea breeze front is shown with a maximum around 3.0 x 10^-8 g/kg/m/s based on a scale of 1 km (their figure 3), similar to our threshold (1.5 x 10 ^ -8 g/kg/m/s) and horizontal scale (4 km). This suggests our thresholds are reasonable, although the exact thresholds to use are unclear, and model-dependant.

   Frontogenesis has also been applied to identify large-scale cold fronts, although these should probably not be compared directly given the different scale and intensity to the sea breeze (e.g. Thomas and Schultz 2019).

   Arnup, S. J., & Reeder, M. J. (2007). The diurnal and seasonal variation of the northern Australian dryline. Monthly Weather Review, 135(8), 2995–3008. https://doi.org/10.1175/MWR3455.1

   Kraus, H., Hacker, J. M., & Hartmann, J. (1990). An observational aircraft-based study of sea-breeze frontogenesis. Boundary-Layer Meteorology, 53(3), 223–265. https://doi.org/10.1007/BF00154443

   Thomas, C. M., & Schultz, D. M. (2019). What are the best thermodynamic quantity and function to define a front in gridded model output? Bulletin of the American Meteorological Society, 100(5), 873–896. https://doi.org/10.1175/BAMS-D-18-0137.1

5. Line 87: I suggest adding the theshold value (16.1) with standard scientific units.
   We have now added the threshold value in units of g/kg/m/s (1.5 x 10 ^ -8, line 97). Note that the units of g/kg/100 km/3h are often used for convenience and so this is retained as the primary units (e.g., as in Thomas and Schultz 2019).

   Thomas, C. M., & Schultz, D. M. (2019). What are the best thermodynamic quantity and function to define a front in gridded model output? Bulletin of the American Meteorological Society, 100(5), 873–896. https://doi.org/10.1175/BAMS-D-18-0137.1

6. Line 87-88. Please help the reader to better comprehend the text “before a series of filters are applied intended to remove non-sea-breeze fronts. These filters include conditions on the morphology of the objects, as well as constraints on object-averaged physical variables”

In our revised manuscript, we now include a more detailed description of these filters in Section 2.1 (line 103-110), as well as a more detailed table of filters in the Supplementary Material (Section S1). This is also in response to a comment from Reviewer 1.

7. Line 164: between which height was the interpolation done.
   The interpolation is done from the model levels, which in BARRA are on "hybrid-heights" that follow variations in terrain close to the surface and become increasingly homogenous with height (see an example of this here from ECMWF: https://confluence.ecmwf.int/display/UDOC/Model+level+definitions). The actual heights therefore depend on the terrain height at each grid point. We have mentioned this in the revised manuscript as well as the approximate number of model levels (line 180).

8. Line 164: Were stability effects considered in the interpolation?
   Stability effects were not included in this calculation, and this is now discussed in the revised manuscript (line 179). This interpolation was done by the modelling group that produced the BARRA-C2 dataset (the Bureau of Meteorology), with the original model level data deleted, so we cannot explore any other interpolation strategies in this dataset.

9. Line 175: which are the other reanalysis models – be specific.
   We have now specified that these previous studies compared with ERA5 and MERRA-2 (line 197)

10. Line 338: "robust assessment", I think you have mentioned in the text a number of examples where the method would fail, so I do not see how the word "robust" can be justified.
    Fair enough, I suppose we got a little ahead of ourselves! Have replaced with "an assessment" (line 379).

11. Line 375: I do not see the relevance of introducing Alpine meteorology in a paper on sea breezes in Australia
    Although it is a very different context, we believe that this paper is relevant because it demonstrates that mesoscale/local atmospheric processes are important for wind energy resource assessments, similar to what we have demonstrated in our manuscript. These processes (sea breezes, alpine winds) can both be forced to some extent by small-scale surface temperature gradients (land-sea or mountain-valley). The relevance of local scale circulations has implications for the scale of modelling needed for wind resource assessments, for example.