

# Moreno-Montes et al 2026 Reviewer

## Comments

Title: Decadal predictions of wind, solar and compound power indicators to support the European renewable energy sector

Author(s): Sara Moreno-Montes et al.

MS No.: egusphere-2026-1205

MS type: Research article

### General Comments

This manuscript evaluates the ability of a multi-model hindcast of decadal predictions to forecast energy indices across Europe for forecast years 1-3. The authors report some skill for forecasts of solar PV potential, while more limited skill is present for wind capacity factors. They also construct a compound energy drought indicator and identify seasonally dependent prediction skill. While the approach presented here is novel, there are several issues with the methodological approach and interpretation of the results which may delay publication until further evaluation has been conducted.

The decision to regrid the decadal predictions (~1 degree resolution) to the higher resolution of ERA5 (0.25 degree) is questionable here, as this gives the impression that decadal predictions have the ability to capture small scale variability in specific regions. This makes it more difficult to interpret the skill maps presented, as much of the significant positive skill is demonstrated in scattered, small scale areas. It would be more appropriate to regrid ERA5 and the decadal prediction systems to the resolution of the coarsest model used (e.g., 1.25 degrees) to provide a fair comparison of skill on the appropriate spatial scales of the decadal predictions. Especially as much of the discussion is focused on slight changes in skill over small regions, a fairer comparison, like the coarse regridding suggested, would help to clarify signal from noise. This would help the interpretation of the findings, as the areas which do show significant skill should be visible as larger regions. This would further demonstrate the robustness of the findings presented here.

The density of figures and subplots described in the results section makes the findings of the paper hard to follow. For example, on page 7 the results of Figure 1 (10 panels) are described with reference to Figure S5 (15 panels) in the supplementary information. In order to make the findings of this paper more impactful, it might be appropriate to condense the findings described in the results section and reduce references to the

supplementary material to only where necessary. This would help the reader to better understand the main findings of the paper and improve the clarity of the results.

The authors report that the main contributing factor to the number of energy drought days (NED) during the winter (DJF) is the number of effective wind generation days (wind-Neff). However, in the supplementary material (Figure S13b, i) they demonstrate that solar-Neff values are missing across most of Europe (aside from southern Iberia). This means that the number of ineffective solar days component of the NED index is effectively constant. The authors repeatedly claim that the areas of NED skill during DJF primarily align with wind-Neff. While this is correct, this is also misleading, as for most of the area shown in Figure 5b and 5g, only the number of ineffective wind days contributes, so the NED shown is no longer a compound index. For clarity, the authors should consider presenting the results differently for DJF in Figure 5 or rephrasing the findings to be clear about what is shown.

Overall, this paper applies a novel methodology to demonstrate some notable skill for energy-sector metrics over Europe. However, methodological choices currently limit the robustness of the results presented. Until these issues are addressed, principally that of fair model comparison (coarse resolution), publication should be delayed. To allow time for these points to be addressed, I recommend that publication to ESD is declined until these issues can be addressed.

## Specific Comments

- Pg3, lines 65-66: “In this study, five different decadal forecast systems contributing to the Decadal Climate Prediction Project (DCPP; Boer et al., 2016) of the Coupled Model Intercomparison Project Phase 6 (CMIP6; Eyring et al., 2016), are used.”
  - Technically, only three different decadal forecast systems are used, as 3 of the 5 forecast system are different initialisations of EC-Earth3. Although the ensemble size is detailed in the SM, it would be useful to mention this here. Has the drift of each of the models been corrected here, as in Appendix E of Boer et al., 2016 (10.5194/gmd-9-3751-2016)?
- Pg 3, lines 67-68: “the indicators are computed for the three corresponding CMIP6 non-initialized historical forcing simulation models.”
  - It would also be useful to include the number of members here.
- Pg 3, line 83: “To ensure consistency across datasets, variables from all model simulations are first interpolated to the ERA5 grid (0.25°).”
  - Can you explain why you chose to regrid the model simulations to a higher resolution for comparison, rather than regridding the ERA5 data to a constant model resolution (e.g., all models and ERA5 data compared at 2.5 x 2.5 degrees)? No new information is gained from the models from

regridding to the higher resolution of ERA5. It would be useful to know how much the results of this study would change, if all analysis was done with models and ERA5 at a 1.25 degree (or 2.5 degree) resolution, to ensure a fair comparison.

- Pg 4, lines 90-91: “In addition, ERA5 wind speeds are adjusted using the Global Wind Atlas (GWA) to better represent long-term mean conditions at hub height.”
  - I am not clear here as to whether the 100m wind speeds are used from ERA5 or the 10m wind speeds passed through the power law transform. It would be useful to be clearer with this. In the SM, it appears that 10m wind speeds were used from ERA5 and then extrapolated using the power law up to 100m. How would the results change if 100m wind speeds from ERA5 were used instead?
- Pg 4, lines 91-92: “Figure S1 shows the climatological ratio between ERA5 and GWA mean 6-hWIND over 1961–2019, which is applied as a pointwise correction to the ERA5 data.”
  - With this pointwise bias correction of ERA5 to the global wind atlas, is this an additive (correcting the mean) or a multiplicative (correcting the spread) bias correction? It would be useful to have greater detail on how exactly this works.
- Pg 4, lines 95-96: “EQM maps the quantiles (percentiles) of the modelled data to the observed ones without assuming any underlying distribution and then it corrects the model biases.”
  - With the Empirical Quantile Mapping, it would be useful to clarify the moments of the distribution that this method acts to correct. This is currently performed for each forecast year and member, but how would it change the results if the EQM was performed across all ensemble members for each of the models in turn? With the signal to noise issue (Scaife and Smith, 2018, 10.1038/s41612-018-0038-4), we cannot assume that the individual members can be regarded as equivalent realisations of climate variability as in the observations. Will the EQM method applied to each member in turn artificially inflate the variance? How is the signal to noise issue accounted for in this study?
- Pg 4 lines 111-114: “PVpot is computed as a dimensionless metric that combines RSDS, TAS and SFCWIND to represent the performance of photovoltaic cells under ambient environment. PV power generation at a specific location is determined by multiplying PVpot with the nominal installed capacity of that location.”
  - How do you determine the nominal installed capacities at each location here?
- Pg 7, Figure 1:

- It would help the reader to have a title for the colorbar here (e.g., ACC). Also, it may help to have ylabels on the far left plots in each row, identifying “Full field” and “Residual” correlation skill. This goes for Figures 3, 5, and similar plots in the supplementary material as well (e.g., S5 onwards).
- Pg 7 lines 182-183: “This reduction is mainly due to northern Europe and parts of Iberia, where MAM skill is generally non-significant.”
  - Presumably due to the lack of skill in northern Europe here?
- Pg 8 lines 210-211: “On the other hand, DCPD skill (Figure 1b) is lower than HIST skill (Figure S7b) in areas such as southern Iberia or the Alps”
  - To my eye, the DCPD (Figure 1b) shows no skill over the Alps and marginal skill over eastern Spain. This marginal skill over Spain increases slightly in HIST, but the skill over the alps in Figure S7b appears too small to be significant. It might be helpful to remove the statement about the Alps here for clarity, therefore.
- Pg 9, Figure 2:
  - Can you explain how the trend of 0.00 in Figure 2b DJF Scandinavia can be statistically significant?
- Figures 2, 4, 6:
  - In all of these figures, it would be helpful to make sure the colorbars for trends (a and b) and correlations (c and d) are the same in each figure. It makes it more difficult to compare the trends particularly when the colorbar scales are slightly different.
- Pg 10, lines 248-249: “The ResCorr isolates the added value of initialization beyond the historical simulations, providing insight into the role of internally generated variability.”
  - The residual correlations show the added value of initialisation, but do not necessarily isolate the internal variability. Initialisation can improve the response to external forcing when initialisation corrects the model response to external factors (Smith et al., 2019, <https://doi.org/10.1038/s41612-019-0071-y>). Consider rephrasing this sentence accordingly.
- Figures 3-6:
  - It would be useful to include the captions here, rather than refer back to the previous Figure through “...Same as Figure X”. This would make it easier for the reader to interpret the findings. As mentioned above for previous figure, it would be very useful to include captions for the colorbar (e.g., ACC) and potentially ylabels identifying what is shown in each row.

- Page 12, lines 293-294: “For the annual mean and DJF (Figure 3f–g), initialization reduces WCF skill over Scandinavia and northern Europe, although HIST skill in these regions is already non-significant”
  - To my eye, there is no significant positive skill over Scandinavia and Northern Europe in either the DCPD or HIST results for WCF in Figure 3 or Figure S10. Suggesting that “initialisation reduced WCF skill over...” could be misleading here, as, although this statement is correct looking at Figure 3f-3g, it suggests the existence of predictable skill for WCF over Scandinavia and Northern Europe in the first place. I find it difficult to interpret the significance of initialisation reducing skill where there is none in the first place. Consider rephrasing or removing.
- Pg 12, line 295: “The skill reduction may be related to the high interannual variability of WCF in these regions (Figure S9f–g),”
  - In Figure S9, I find it very difficult to interpret whether the interannual standard deviation over Scandinavia and Northern Europe is higher than in other regions. To my eye, the magnitudes of interannual variability are similar in Scandinavia and Northern Europe to the rest of Europe, with the one exception to this being the Baltic region. I am not clear how the skill reduction here (i.e., less negative skill in HIST to more negative skill in DCPD) relates to interannual variability. Consider rephrasing or drop.
- Page 12, lines 302-304: “During JJA (Figure 3i), initialization increases skill over parts of eastern Europe and central Germany, while reducing it over Iberia, where HIST skill is already low (Figure S10d), resulting in a higher fraction of significant area for the DCPD than for the HIST”
  - In this case, the areas of significance over central Germany are very small so could be a result of noise. Additionally, as mentioned in the point above, I am not clear on the significance of negative skill (i.e., no skill) becoming more negative over Iberia. The implications of this to me are unclear.
- Page 12, lines 305-308: “Overall, model initialization affects WCF skill mainly in winter and summer: it leads to a marked reduction of skill in DJF, while it enhances skill in JJA.”
  - I am not fully convinced on the evidence provided to justify this statement. ‘Marked reduction of skill in DJF’, as mentioned above, suggests that there was some predictable skill present in the first place, which there is not in Figure 3b. Similarly, the enhancement of skill in JJA only appears to occur over Northern Europe. I would consider rephrasing this statement to be more precise.
- Pg 12-13, lines 321-323: “In addition, ResCorr is significantly positive in Central Europe in MAM, even though the correlation is not significant and trend

agreement is weak, indicating the increase of skill due to the initialization in this case.”

- Could the significance in this case potentially be by chance? As with 95% significance we would expect 1 in 20 samples to show significant correlations by chance. This result is not reflected in the spatial plot in Figure 3h.
- Pg 13, lines 323-324: “Other regions show non-significant but positive ResCorr values, suggesting local improvements due to initialization.”
  - I am not clear as to whether insignificant residual correlation values of 0.1-0.2 suggest local improvements due to initialisation. As these are such low values, they could be the result of noise. Additionally, where there is no correlation skill shown in the region in Figure 4c, skill does not improve, rather negative skill becomes less negative. Perhaps it would be more appropriate to focus the discussion on findings where there is both positive skill in Figure 4c and positive residual correlation skill in Figure 4d.
- Pg 13, lines 330-331: “in other cases providing additional skill independently of it.”
  - I am not clear on where initialisation provides additional skill independent of agreement in the trends. Is this referring to the example of MAM Central Europe in Figure 4d?
- Pg 13, lines 332-333: “Using climate projections, Pryor et al. (2005) have identified slight decreases in wind speed and wind energy density over northern Europe.”
  - Are CMIP3 class models a fair comparison for the results shown here? Have there been other, more recent, studies that support the findings of this research?
- Pg 13-14, lines 336-338: “Observational analyses by Vautard et al. (2010) and Torralba et al. (2017a) have revealed spatially heterogeneous near-surface wind trends across Europe, with predominantly negative signals in several regions.”
  - What level of agreement is there between the observed regional trends shown here and in the studies referenced? Do the areas of trend increase/decrease shown in Figure 4c line up with previous analysis in this area?
- Pg 14, lines 360-361: “In our case, the prior calibration of all climate variables against ERA5 ensures consistent magnitudes...”
  - Consistent magnitudes of what? Magnitudes of variability?
- Pg 14-15, lines 370-373: “By contrast, both-Neff exhibits more spatially fragmented skill, with isolated regions of significant values. In solar-Neff and both-Neff, some areas appear as missing values because correlations cannot be

computed when the indicator remains equal to zero for all years, and solar energy droughts are expectable when the amount of daily sun hours is low (Figure S13b, e).”

- I am not clear on the presentation of NED for DJF in Figure 5. Figure S13b, g, and i, suggest that for much of Europe, the solar indicator is equal to zero for all years, due to the low amount of daily sunshine hours. In this case, most of the area in Figure 5b and 5g is solely dependent on the number of ineffective days for the wind resource, as the number of ineffective days for solar is constant (i.e., all winter days are ineffective for solar in the greyed out region in Figure S13). Therefore, this would suggest that most of the area in Figure 5b and 5g does not show the combined NED index, as only one of the variables (e.g., wind speed) varies. It would be useful to clarify this and potentially modify the figure to show only the area where the NED index incorporates both solar and wind speed, as this may be misleading to the reader otherwise.
- Pg 15, lines 376-378: “By contrast, over parts of the Alps and southeastern region, NED exhibits significant skill despite wind-Neff being non-significant, indicating a dominant contribution from solar-Neff.”
  - In Figure 5a (annual NED correlations), I cannot clearly see a significant area of positive skill over the Alps. While there is some skill over eastern Europe, both solar-Neff and wind-Neff appear to show some significant positive skill over this region in Figure S13, although the magnitude of the positive skill is lower for wind-Neff.
- Pg 15: lines 378-379: “In DJF (Figure 5b), NED skill over Iberia and eastern Europe aligns mainly with wind-Neff skill (Figure S13g).”
  - As with the point described above for lines 370-373, I think this statement could be misleading, as only wind-neff skill varies over eastern Europe and much of Iberia. The solar-neff is effectively constant. In this case, surely the NED skill over eastern Europe, can only align with wind-neff skill, as solar-neff is constant?
- Pg 15, lines 381-383: “Finally, in SON (Figure 5e), NED skill is confined to parts of Poland and the Baltic region, closely matching the wind-Neff skill pattern (Figure S13j) and in points of southeastern Europe, which are skillful at solar-Neff (Figure S13e).”
  - Much of the NED skill shown in Figure 5e is non-significant here. Additionally, both solar-Neff and wind-Neff show skill over Poland, suggesting that both sources contribute, rather than only the wind-Neff mentioned in this statement here. Additionally, the area of significant skill in southeastern Europe is small, suggesting that it could be related to noise.

- Page 15, Lines 385-386: “During periods of high solar availability (JJA and, in the southern region, MAM), NED skill is largely driven by solar-Neff, whereas in low-radiation seasons (DJF and SON) wind-Neff becomes the main modulator.”
  - As with the comments above, I think this conclusion may be misleading for the DJF result, as, for much of the European area shown, only the number of ineffective wind days contributes, as the number of ineffective solar days is effectively constant. For much of the region in DJF therefore, wind-(in-)Neff is the only modulator, so it is misleading to suggest that it is the main modulator. This point is not clarified in enough detail in the main text. Additionally, the suggestion that wind-Neff is the main modulator for SON may also be misleading, as this is only true for the limited area of skill over southeastern Europe, while for Poland/Baltic area, both the wind-Neff and solar-Neff appear to contribute.
- Page 16, lines 393-395: “In MAM (Figure 5h), initialization enhances skill over parts of southern and eastern Europe while reducing it over Iberia, leading to a modest improvement of DCPD skill relative to HIST (Figure 5c vs Figure S14c).”
  - Much of the ResCorr skill in Figure 5h is marginal and insignificant, suggesting that these areas could be due to chance. The difference in significant area of ~4% is relatively small. Therefore, the improvements from initialisation are marginal and the DCPD skill is approximately the same as the HIST (i.e., initialisation does not significantly degrade the skill).
- Page 16: lines 398-400: “For this indicator, initialization generally increases predictability under conditions of higher sunshine duration (JJA and MAM) and reduces it under the opposite conditions (DJF and SON).”
  - Once again, any findings related to solar-Neff driving DJF results are misleading, as only the number of inefficient wind days contributes across most of Europe. The only season where initialisation appears to increase skill via ResCorr in Figure 5 is JJA, with MAM, DJF, and SON all showing few areas with any ResCorr skill. I would suggest this sentence is rephrased for clarity, as the MAM ResCorr result is not equivalent to the JJA ResCorr result.
- Page 16, lines 403-404: “In JJA, Iberia, Western Europe, Mediterranean, Central and Eastern Europe display significant negative trends in both datasets, which results in significantly positive correlations (Figure 6c).”
  - It would be useful here to understand the relative contributions of trends in the number of ineffective wind and solar days, in absolute terms. I suspect that the trends in solar radiation contribute more strongly than the trends in wind speed. It would be useful to include greater detail on this here.

- Page 16: lines 418-419: “NED variability is largely controlled by the energy source that dominates effective production in each season: wind in DJF and SON, and solar in JJA and, secondarily, in MAM.”
  - See the above points on the number of ineffective wind days in DJF being the only contributor to NED here.
- Figures 2, 4, 6
  - It would be useful to have subplot headings here, to convey the information more clearly to the reader, these could be: “a) ERA5 trends”, “b) DCPD trends”, “c) Corr”, “d) ResCorr”.
- Page 17, lines 429-430: “Additionally, DCPD trends show seasonal patterns that broadly resemble the future evolution of hybrid energy droughts projected by (Kapica et al., 2024), particularly in DJF and SON.”
  - It would be useful to have further clarification on how these resemble the results of Kapica et al., 2024, particularly regarding the similarities and differences of the NED metric used here compared to the hybrid metric from Kapica.
- Page 18, lines 445-446: “This strong seasonal dependence indicates that PVpot predictability is highest during periods of higher and persistent solar radiation, while such predictability is more limited under weaker and more variable radiative conditions.”
  - To what extent is the highest predictability during the summer driven by the fact that the trends are strongest during the summer (Figure 2)?
- Page 19: lines 455-457: “Significant skill is consistently found over parts of eastern Europe across seasons, while other regions show skill only seasonally: central Europe in JJA, southern Europe in DJF, and northern Europe in MAM and SON.”
  - I am not convinced that significant skill is found over southern Europe in DJF (Figure 3b). While there is some skill over eastern Europe, the remaining areas of significant positive skill are small and spatially heterogenous.
- Page 18, lines 459-460: “Initialization tends to enhance skill in summer and reduce it in winter, while its impact is generally small in the transition seasons.”
  - Most of the ResCorr in Figure 3i is over Eastern Europe, suggesting that the enhancement of skill in summer is more regionally constrained than suggested in this statement. The reduction of skill during the winter is mostly negative skill becoming more negative, suggesting that there wasn’t skill to reduce in the first place. Consider rephrasing.
- Page 19, lines 466-469: “Seasonal differences in NED skill reflect the dominant energy source. During high-radiation periods (JJA and partly MAM), NED

predictability is mainly driven by solar-Neff, whereas during low-radiation seasons (DJF and SON) wind-Neff becomes the main contributor.”

- See the above points on the number of inefficient wind days being the only contributing factor to skill during DJF. Consider rephrasing and/or changing narrative above.
- Page 19, line 480: “However, ERA5 has been identified as the best reanalysis for wind (Ramon et al., 2019)”
  - It would be useful for the reader to be more specific about the particular characteristics of ERA5 which make it preferable to other reanalysis products here.
- Page 19, lines 492-494: “In addition, hybrid approaches that combine percentile-based metrics with physically motivated thresholds may help balance climatological comparability and operational relevance when assessing compound renewable-energy risks.”
  - How can percentile-based metrics be combined with physical thresholds in a hybrid approach? Does this involve selecting the percentiles based on physical thresholds or vice versa?

## Technical corrections

- Pg 8 Line 216: “Figure 2 summarizes the regional behaviour of PVpot long-term trends across the seven European sub-regions (Figure S2)”
  - Figure S2 should be Figure S4
- Pg 12, line 301: “and the DCP (Figure 3c) has higher skill values than HIST (Figure S10h)”
  - Figure S10h should be Figure S10c
- Pg 14, line 353: “...almost all seasons closely resembles the patterns found for the WCF standard deviation (Figure S8f-j),”
  - Figure S8f-j should be Figure S9f-j
- Pg 15, line 373: “and solar energy droughts are expectable when...”
  - Replace ‘expectable’ with ‘to be expected’ or ‘expected’