nature portfolio

Peer Review File

Projected impacts of future offshore wind farms on coastal precipitation over the Northwest European shelf

Corresponding Author: Dr Naveed Akhtar

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns

This study suggests that large-scale offshore wind farms (OWFs) planned for the North and Baltic Seas may significantly alter regional precipitation. Using the high-resolution COSMO-CLM 6.0 model, researchers simulated weather over 2008–2017 under present (2023), near-term (2030), and future (2050) OWF scenarios with turbines ranging from 5 to 15 MW. The results show that OWFs reduce near-surface wind speeds by 2–3 m/s, changing air–sea exchanges of heat and moisture. More importantly, they redistribute rainfall: precipitation increases by 10–15% over wind farms but decreases by 8–15% along adjacent coasts, particularly in Denmark, northern Germany, the Netherlands, and the UK. The drying is strongest in Jutland and during winter and autumn, when prevailing south-westerly winds dominate.
The issue of circulation changes in OWF is important and timely. The experimental design, analysis, and results are straightforward and easy to follow. However, there are several key issues that the authors should address: Regarding precipitation changes in future scenarios, the treatment appears to oversimplify the warming effects. The comparison with RCP8.5 may not be entirely appropriate for this context. Additionally, more validation against observational data in large-scale circulation is notably absent from the study. I recommend additional simulations and more comprehensive analysis to strengthen the findings. The main comments are listed below.

Main
1. In the abstract, the authors mention a reduction in precipitation by 10–17%, which contrasts with climate projections under RCP8.5 showing a 15% increase in winter precipitation across these regions. However, the comparison with RCP8.5 projections may be misleading, as it does not represent a directly comparable framework. RCP8.5 considers circulation changes driven by external forcing, incorporating both thermodynamic and dynamic effects. The experimental design in this study does not adequately account for these factors. I suggest conducting additional simulations using COSMO-CLM 6.0 with modified boundary conditions that properly incorporate both thermodynamic and dynamic changes consistent with the RCP8.5 scenario.
2. What is the extent of model bias in COSMO-CLM 6.0? To understand the potential future effects of OWF, it is essential to provide reliability information, including quantification of model bias uncertainty, to stakeholders. The authors provided a comparison with FINO1 observations; however, the spatial information is not adequately discussed. Please provide more comprehensive analysis with entire domain to demonstrate the model's simulation capabilities in NOWF simulation.

Minor

1. Significance testing is missing in Figures 2, 3, 4, 5, 6, S1, S2, S5-7, S9, and S10.
2. The authors consistently mention percentage changes throughout the manuscript. I suggest using a consistent approach in the figures: show percentage changes in color shading, while contours could represent the mean of the total field from the control simulation.
3. Figure S1c contains only one additional panel compared with Figure 2, making it redundant. Consider consolidating or removing the duplicate content. Same as Fig 3 and Fig S2.

Reviewer #2

(Remarks to the Author)
Review of COMMSENV-25-3601: “Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns”

The study examines the projected effects of large-scale offshore wind farms on coastal rainfall patterns in Northwest Europe by 2050. It is well written, structured, and organised. However, using unrealistic wind turbine scenarios and failing to test for statistical significance weakens the reliability of the findings. I expand on this below.

I have four main concerns regarding the methods:

1. It is unreasonable to consider scenarios with 5 MW wind turbines. Currently, large wind farms already utilise turbines rated at 8 MW or higher, making it uneconomical to deploy smaller units. Unless the authors provide other physical justifications, this scenario seems unnecessary.

2. The main impact of wind farms on wake effects and surface climate variables depends primarily on the spatial extent and capacity density of future wind farms, rather than on the capacity of individual turbines. I notice the capacity density values used in the simulations are missing from the manuscript (Table 1 appears incomplete). These data are essential to assess whether this somewhat unrealistic scenario aligns with the country's wind expansion targets. For example, studies on efficiency losses have led the German authorities to limit wind farm capacity densities to approximately 8 MW/km²; Denmark’s limits are around 4.5 MW/km². Why model these scenarios if they lack realism? If the authors have replaced a 5 MW turbine with a 10 MW or 15 MW turbine, this effectively doubles or triples the capacity density. This conflicts with development plans for the North Sea. Such “potent” wind farms are unlikely to be constructed because they are economically unviable. Of course, these simulations will show changes in precipitation, but this is unlikely to happen given the current development scenarios.

3. Given that weather modelling is a chaotic system, the results should be tested for statistical significance, especially outside wind farm areas, because precipitation has a substantial time variability.

4. The vertical grid spacing is relatively coarse compared to earlier studies. The authors do not demonstrate whether the results are sensitive to changes in either spatial or vertical grid spacing.

Some other minor corrections and questions:
1. L53-66. Please be more precise on whether changes are seen over land or offshore. Some of the impacts are different on these two surfaces.
2. Something is missing in Table 1. The last line is orange and not complete.

Reviewer #3

(Remarks to the Author)
Great manuscript overall, please see attached Word document for more detailed comments.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The author has addressed my question.

Reviewer #2

(Remarks to the Author)
I have detailed my review of the manuscript in the attached document.

Reviewer #3

(Remarks to the Author)
This manuscript is almost ready! The authors have done an excellent job at addressing the reviewers' comments. I just have a few small copy-edits and two minor suggestions to incorporate before this manuscript is published.

Line 14: Delete "Baltic Sea", or change to "and the Baltic Sea" if you choose to consider OWF development in this region as well. Also, there is a missing comma after the word "period".

Line 25: Change "the temperature rise" to "global average temperature increases above pre-Industrial levels". This is more precise wording.

Line 45: Change "wakes areas" to "wake areas".

Lines 52-54: Delete these two sentences. It's too short of a paragraph to stand alone and disrupts flow between the previous and next paragraphs, which have a more logical progression of topics.

Line 74: Delete "and minimal", you don't want to undervalue the impacts that OWFs can have on local climate, since that's what you are arguing here.

Lines 81-82: Missing commas after "installations" and "precipitation".

Line 99: Change "includes" to "include", since "data" is a plural word.

Lines 150-156: These details about why the COSMO-CLM model was used should be placed at the start of this section, not the end. This rationale would make more sense immediately after introducing the model itself, perhaps after Line 123 and before explaining your grid resolution.

Line 319-322: I think you should explicitly state that IPCC AR6's GCM simulations do not account for OWF development's effects on regional/local climate, hence the discrepancy that exists, meaning that an argument exists for the use of COSMO-CLM and other similar models for providing robust climate simulations at smaller scales that account for the effects of sustainable energy transitions. This would help make the point of your manuscript and its contribution to the broader climate literature stronger.

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
While the study's design tends to exaggerate the impact of large offshore wind farms on precipitation patterns, I appreciate that the authors have provided sufficient caveats in the abstract and the manuscript's conclusions. These acknowledgements help clarify the limitations and potential uncertainties associated with the findings. Given this consideration, I am willing to approve the manuscript for publication.

Reviewer #3

(Remarks to the Author)
This is my third time viewing this manuscript, and at this point I am more than content with its scientific content! I only have copy-edits to offer:

Lines 56-58: Add these two sentences into the next paragraph, such a short paragraph on its own like this looks a little odd.

Lines 62-63: You already defined TKE as an acronym on Line 54, no need to do it twice!

Lines 121-122: "In this study, we focus on the impact of OWFs on coastal 121
precipitation in the Northwest-European shelf." Delete this sentence, or delete the mention of the Baltic Sea in the next sentence. It's odd to state you are focusing on the Northwest-European shelf to then mention the Baltic Sea again immediately afterwards and again later in the Methods chapter.

Line 170: Same comment as above about acronyms, no need to define them again! Same again on Line 251.

It looks like the authors did a good job addressing Reviewer #2's comments too!

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The images or other third party material in this Peer Review File are included in the article's Creative Commons license, unless indicated otherwise in a credit line to the material. If material is not included in the article's Creative Commons license and your intended use is not permitted by statutory regulation or exceeds the permitted use, you will need to obtain permission directly from the copyright holder.
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Point to point reply

Dear Editor,

We sincerely thank all three reviewers for their valuable suggestions and constructive comments, which have greatly improved the quality of our manuscript "**Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns**". In response to the feedback, we have made substantial revisions throughout the paper, carefully addressing each comment and incorporating all recommendations into the revised version.

Due to these extensive modifications, the line numbers in the updated manuscript no longer correspond to those in the previous submission. In addition, some sections referenced in the reviewer comments have been rewritten, reorganized, or removed to enhance clarity and coherence.

Along with the revised manuscript, we submit a detailed point-by-point response, in which each reviewer comment is addressed individually and the corresponding changes are clearly explained.

We sincerely appreciate the time and effort invested by the reviewers and the editorial team. Their feedback has significantly strengthened the manuscript, and we hope that the revised version meets the requirements for publication.

Kind regards

Naveed Akhtar
31.01.2026

Reviewer #1

Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns

This study suggests that large-scale offshore wind farms (OWFs) planned for the North and Baltic Seas may significantly alter regional precipitation. Using the high-resolution COSMO-CLM 6.0 model, researchers simulated weather over 2008–2017 under present (2023), near-term (2030), and future (2050) OWF scenarios with turbines ranging from 5 to 15 MW. The results show that OWFs reduce near-surface wind speeds by 2–3 m/s, changing air–sea exchanges of heat and moisture. More importantly, they redistribute rainfall: precipitation increases by 10–15% over wind farms but decreases by 8–15% along adjacent coasts, particularly in Denmark, northern Germany, the Netherlands, and the UK. The drying is strongest in Jutland and during winter and autumn, when prevailing south-westerly winds dominate.
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Main

1. In the abstract, the authors mention a reduction in precipitation by 10–17%, which contrasts with climate projections under RCP8.5 showing a 15% increase in winter precipitation across these regions. However, the comparison with RCP8.5 projections may be misleading, as it does not represent a directly comparable framework. RCP8.5 considers circulation changes driven by external forcing, incorporating both thermodynamic and dynamic effects. The experimental design in this study does not adequately account for these factors. I suggest conducting additional simulations using COSMO-CLM 6.0 with modified boundary conditions that properly incorporate both thermodynamic and dynamic changes consistent with the RCP8.5 scenario.

The reviewer is right, reference to the RCP8.5 is too simplified and may not be entirely appropriate for this context. The mentioning in the abstract misguides the reader, since the RCP8.5 and climate change scenarios are not addressed in your paper and beyond the scope of our paper. We acknowledge that climate change scenarios would involve different boundary and thermodynamic conditions compared to our OWF simulations. Our intention was not to analyze an impact of wind energy usage under climate change.

The reference we made to RCP8.5 is solely intended to provide a “contextual comparison” of precipitation changes illustrating that OWF induced reductions (8–15%) are of similar magnitude to projected increases under RCP8.5 (~15%). Note that the numbers for

RCP8.5 scenarios are for the end of 2100 whereas OWFs number are based on 2050 technical scenario, when climate change signal will be still small. This comparison was done to assess the relative importance of OWF impacts vs regional climate change signals without implying physical consistency between the two sources of change. Performing simulations with RCP8.5 forcing would require a fundamentally different experimental design. A recent assessment of the downscaled CMIP6 data produced in our group (currently unpublished) reveals that simulated wind speeds vary systematically with the choice of forcing GCM, and none of the simulations captures the decadal variability present in ERA5 (Figure 1 and Figure 2).

It is no trivial task to select a suitable GCM and time slices in order to obtain generally valid statements about the probable effects of OWF on climate change. Figure 3 shows the differences in wind speed at 100 m height for the North Sea between GCMs and scenarios over the decades to come. The mean wind speeds of the different forcings differ by up to 1 m/s, meaning the response to wind farms depends strongly on the chosen GCM as a forcing. Additionally, to the necessary in depth analysis, the set of 10-years simulations for historical climate as control and future climate as control plus the technical scenarios for both is far beyond the scope of our study.

In this context we would like to mention that the computational costs for additional high resolution simulations would be extremely high. Up to now we invested 60000 CPU-Nodehours on the HPC system Levante of the DKRZ for our investigations: three future coverage scenarios (2023, 2030, 2050) and three turbine size scenarios (5 MW, 10 MW, 15 MW) over a 10-year period at 5 km resolution were done to significantly reduce the impact of internal variability and to capture key drivers of uncertainty.

We plan to conduct such an interesting study in the future. To avoid any confusion regarding our 'contextual comparison' with RCP8.5 scenarios. We have removed any discussion of climate change impact from results and abstract and improved the discussion in the conclusion section to contextualize the regional changes based on published literature. Removing this statement does not affect the scope of the paper. As a prove of validity of our statement concerning the RCP8.5 precipitation signal, please, find the analysis of the precipitation over Denmark for the recently finished CMIP6 downscaling experiments in the UDAG project (Figure 3; Früh, 2023)

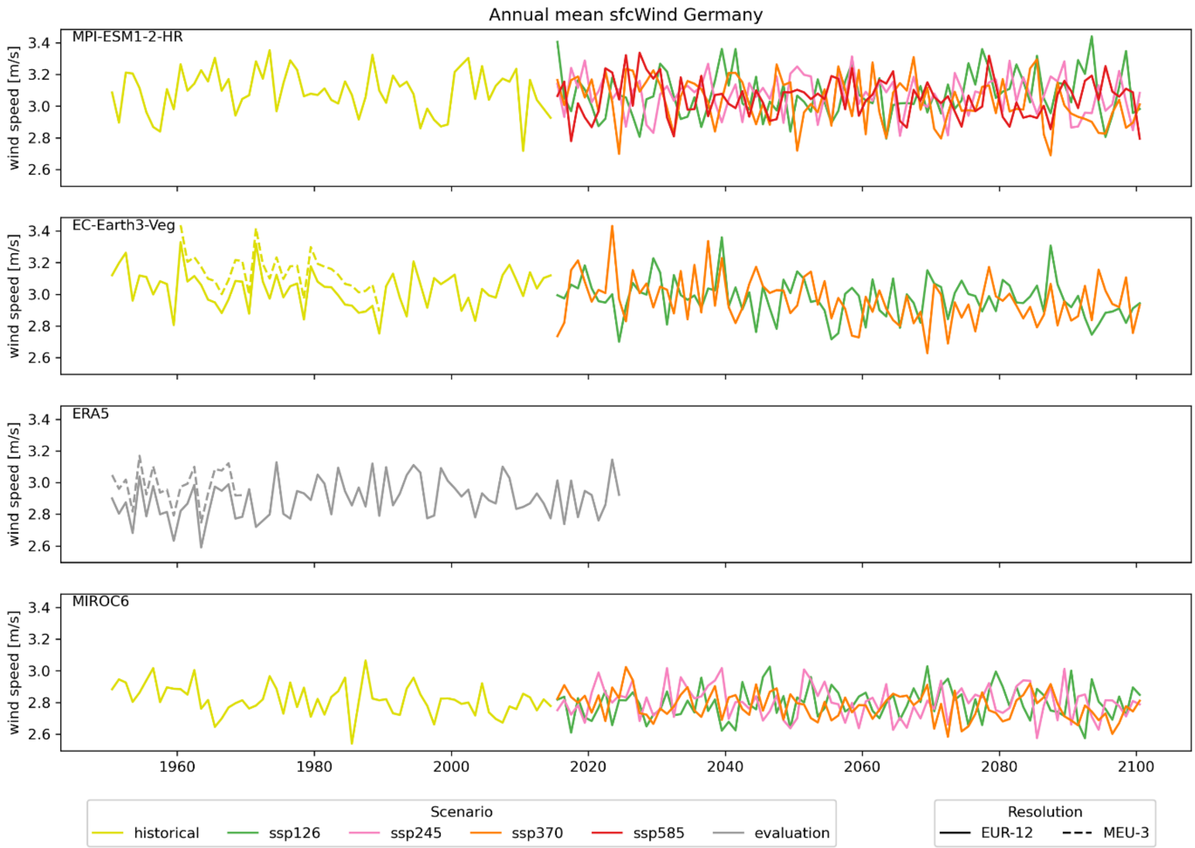


Figure 1: Example for yearly mean wind speeds in 10m height for Germany of CMIP6 downscalings with a resolution of 12 km (solid lines) and 3 km (dashed lines) for the GCMs CNRM-ESM2, EC-Earth-Veg, MIROC6 and MPI-ESM1 and for ERA5 for comparison. Historical simulations started in 1950 and 1960 and lasted until 2014; the associated scenarios run until 2100. The figure was prepared by P. Heinrich (Hereon) and shows ongoing work. Data are produced within the project UDAG and not yet published.

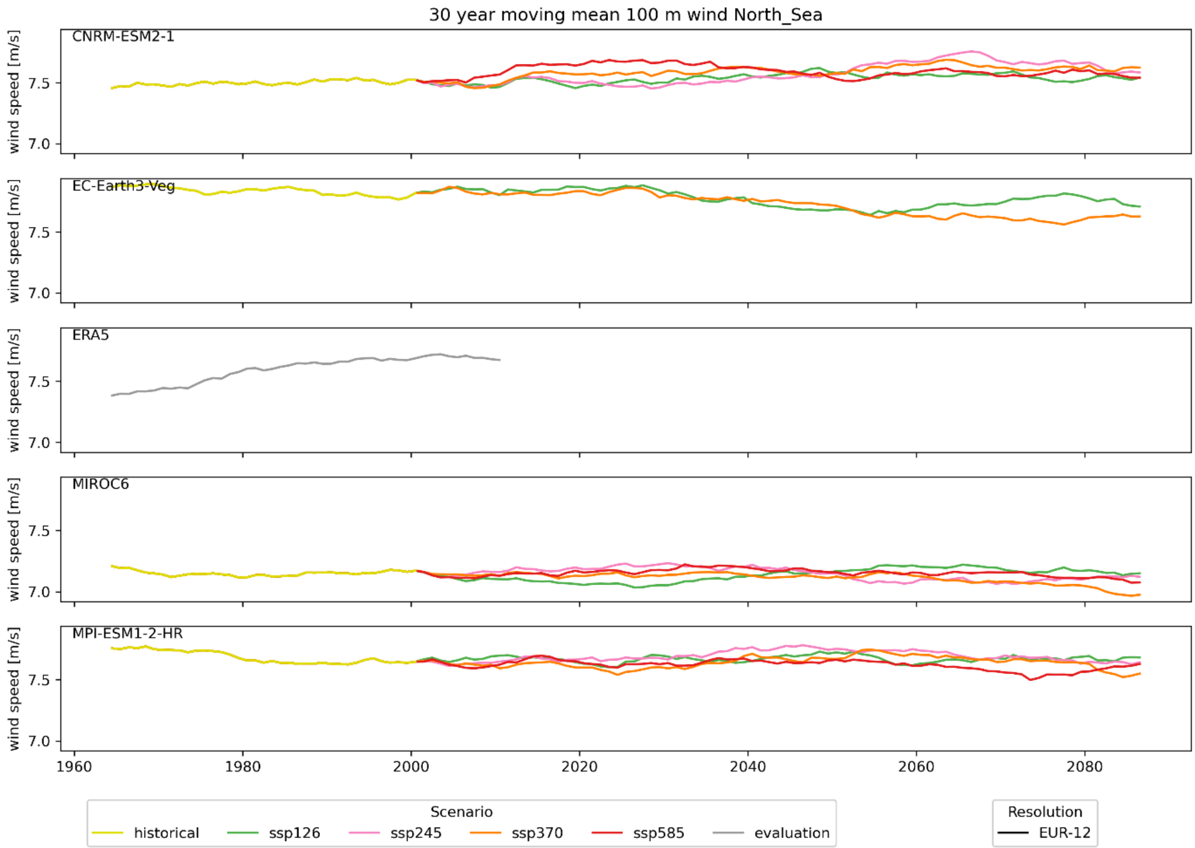


Figure 2: Example for 30 year running mean wind speeds in 100 m height for a selected North Sea region (0-8°E; 54-57°N) of CMIP6 downscalings with a resolution of 12 km for the GCMs CNRM-ESM2, EC-Earth-Veg, MIROC6 and MPI-ESM1 and for ERA5 for comparison. Historical simulations started in 1950 and 1960, respectivly and lasted until 2014; the associated scenarios run until 2100. The figure was prepared by P. Heinrich (Hereon) and shows ongoing work. Data are produced within the project UDAG and not yet published.

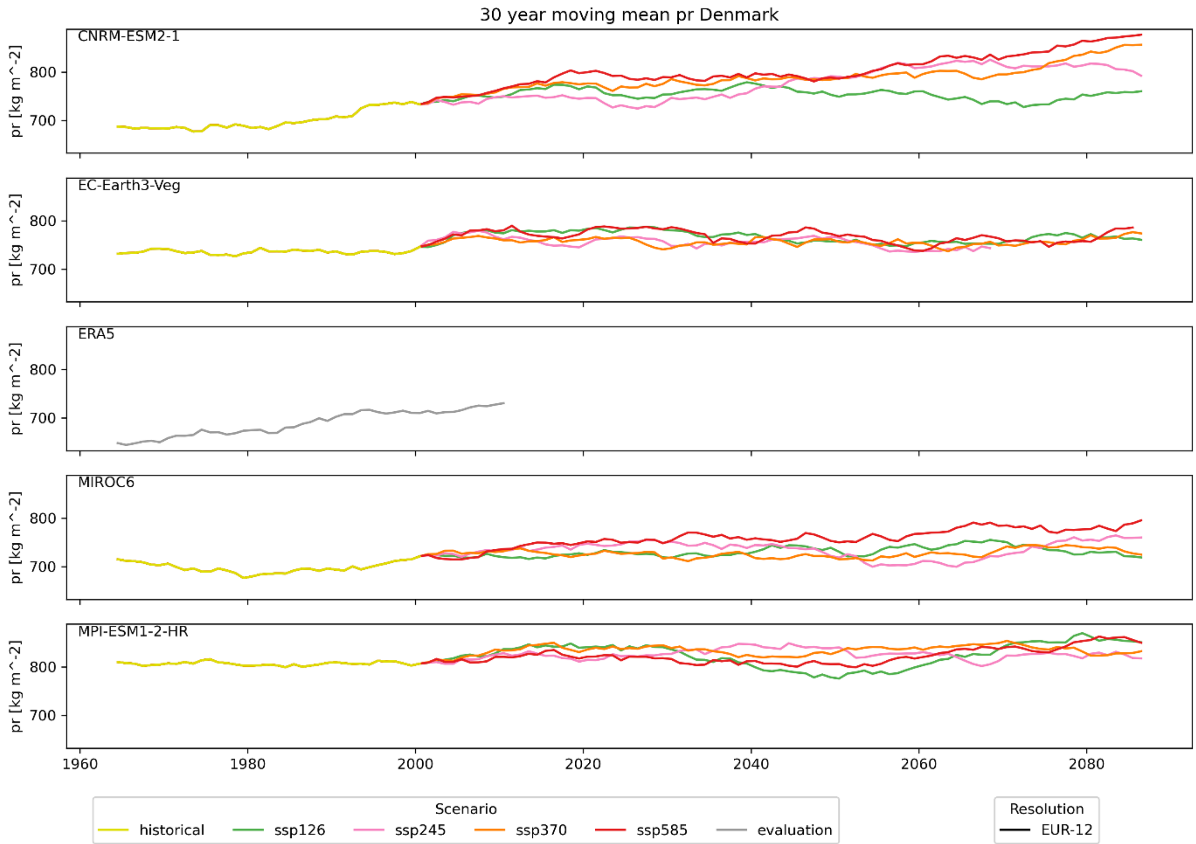


Figure 3: Example for precipitation amount as 30 year moving averages of CMIP6 downscalings with a resolution of 12 km for the GCMs CNRM-ESM2, EC-Earth-Veg, MIROC6 and MPI-ESM1 and for ERA5 for comparison. Historical simulations started in 1950 but were used from 1960 and lasted to 2014; the associated scenarios run until 2100. Figure was prepared by P. Heinrich, Hereon. Data are produced within the project UDAG and not yet published.

2. What is the extent of model bias in COSMO-CLM 6.0? To understand the potential future effects of OWF, it is essential to provide reliability information, including quantification of model bias uncertainty, to stakeholders. The authors provided a comparison with FINO1 observations; however, the spatial information is not adequately discussed. Please provide more comprehensive analysis with entire domain to demonstrate the model's simulation capabilities in NOWF simulation.

COSMO-CLM is a well-established regional climate model that has been validated against observational data in several high-impact studies (Rockel et al. 2008, Geyer et al. 2014). Our own previous work (Akhtar et al. 2021, Akhtar et al., 2022; Akhtar et al., 2024, Ruben et al., 2024) includes model validation and validation of wind speed changes associated with OWF parameterization. Furthermore, a recent manuscript from our group (Alberto Elizalde et al., 2025, Preprint), presents additional long-term validation and focuses on uncertainty quantification. We added the following text to manuscript (Line:150—156): "COSMO-CLM is a widely used regional climate model that has been extensively validated against observational datasets in numerous high-impact studies[31,33,35,37]. In addition to demonstrating the model's overall performance, earlier research [14–16,31,32,38] has specifically evaluated wind speed changes resulting from OWF parameterization, using both airborne measurements and mast data from FINO1. More recently, another study[39] has provided further long-term validation of existing OWFs and placed particular emphasis on quantifying uncertainties."

Minor

1. Significance testing is missing in Figures 2, 3, 4, 5, 6, S1, S2, S5-7, S9, and S10.

Thank you for recognizing this. In our study, the panels in Figures 2, 3, 4, 5, 6, S1, S2, S5–7, S9, and S10 present long-term mean fields that highlight systematic and persistent deviations between simulations with and without offshore wind farms. These figures are not intended to illustrate event-based or sample based variability, but rather climatological differences that emerge from multi-year averaging.

Because these results represent deterministic long-term means derived from the full simulation periods, rather than distributions of repeated samples, classical statistical significance testing is not necessary here. The purpose of these figures is to show structurally consistent, physically interpretable shifts in the mean climate state attributable to the presence of offshore wind farms, not to evaluate stochastic variability.

2. The authors consistently mention percentage changes throughout the manuscript. I suggest using a consistent approach in the figures: show percentage changes in color shading, while contours could represent the mean of the total field from the control simulation.

Thank you very much for the suggestions. We have added the contours as recommended. However, because the figure panels are already quite small, it is difficult to extract meaningful information from the contour lines (see below Fig.4). Therefore, we have included the percentage values of the absolute differences in Fig. SI 6 instead. We also added the absolute values in the main text alongside the percentage differences.

For wind speed the absolute values of wind speed are crucial for understanding how a change of 0.5 or 1 $m \cdot s^{-1}$ can affect the rated power of a wind turbine. Therefore, for constancy we added only absolute difference in the main plot.

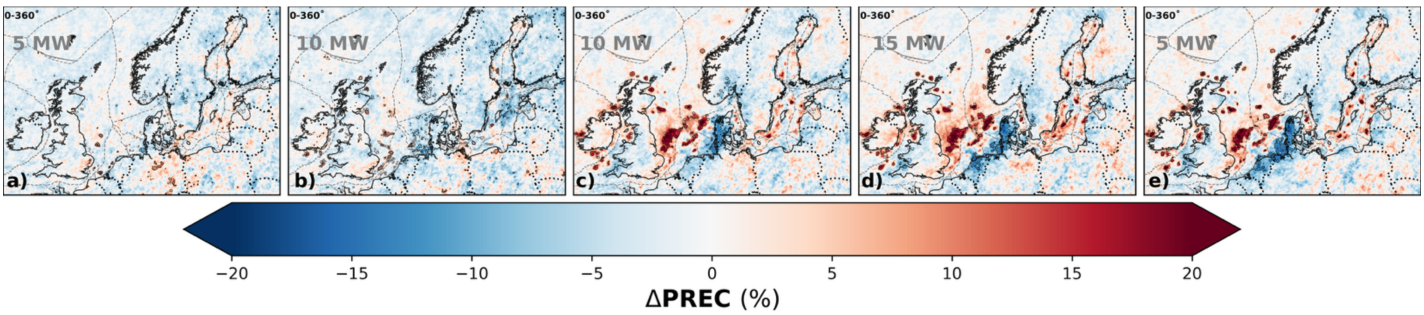


Figure 4 Annual mean percentage differences (colors) and absolute difference (black contours) in precipitation between simulations with wind farms a) WF2023_5MW, b) WF2030_10MW, c) WF2050_10MW, d) WF2050_15MW, and e) WF2050_5MW and the control NOWF simulation for all wind directions (0—360°) over the period of 2008—2017. This figure was created with Matplotlib (Hunter, J. D., Matplotlib: a 2D graphics environment. Computing in Science and Engineering 9, 2007) and Cartopy (Met office, Cartopy: a cartographic python library with a matplotlib interface. Exeter, Devon, https://scitools.org.uk/cartopy, 2015).

3. Figure S1c contains only one additional panel compared with Figure 2, making it redundant. Consider consolidating or removing the duplicate content. Same as Fig 3 and Fig S2.

Thank you very much for the suggestion. We have updated Figures SI 4 and SI 6 by moving the 5 MW panel to the end, ensuring consistency with Figures 2 and 3 of the main paper. Although the first four panels remain same to those in the main paper, we have retained them in the supplementary material to allow readers to easily compare all five cases within a single figure panel. The captions have also been updated accordingly.

Reference:

Akhtar, N., Geyer, B. & Schrum, C. Impacts of accelerating deployment of offshore windfarms on near-surface climate. *Sci. Rep.* **12**, 18307 (2022).

Akhtar, N., Geyer, B. & Schrum, C. Larger wind turbines as a solution to reduce environmental impacts. *Sci. Rep.* **14**, 1–12 (2024).

Akhtar, N., Geyer, B., Rockel, B., Sommer, P. & Schrum, C. Accelerating deployment of offshore wind energy alter wind climate and reduce future power generation potentials. In review. *Sci. Rep.* 2021 (2021) doi:10.1038/s41598-021-91283-3.

Borgers, R. *et al.* Mesoscale modelling of North Sea wind resources with COSMO-CLM: model evaluation and impact assessment of future wind farm characteristics on cluster-scale wake losses. *Wind Energy Science* **9**, 697–719 (2024).

Elizalde, A., Akhtar, N., Geyer, B. & Schrum, C. Uncertainty in Offshore Wind Power Forecasts: A Regional Climate Modeling Approach for the North Sea. Preprint at https://doi.org/10.5194/wes-2025-64 (2025).

Früh, B. (2023). *Aktualisierung der Datengrundlage für die Anpassung an den Klimawandel in Deutschland*. https://www.dwd.de/DE/forschung/projekte/udag/udag_node.htm

Geyer, B. High-resolution atmospheric reconstruction for Europe 1948–2012: coastDat2. *Earth Syst. Sci. Data* **6**, 147–164 (2014).

Rockel, B., Will, A. & Hense, A. The regional climate model COSMO-CLM (CCLM). *Meteorologische Zeitschrift* **17**, 347–348 (2008).

Reviewer #2

Review of COMMSENV-25-3601: "**Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns**"

The study examines the projected effects of large-scale offshore wind farms on coastal rainfall patterns in Northwest Europe by 2050. It is well written, structured, and organised. However, using unrealistic wind turbine scenarios and failing to test for statistical significance weakens the reliability of the findings. I expand on this below.

I have four main concerns regarding the methods:

1. It is unreasonable to consider scenarios with 5 MW wind turbines. Currently, large wind farms already utilize turbines rated at 8 MW or higher, making it uneconomical to deploy smaller units. Unless the authors provide other physical justifications, this scenario seems unnecessary.

Thank you for your valuable suggestion. We acknowledge that larger turbines (8 MW and above) are now being deployed. However, most operational wind farms currently use turbines rated between 3.6 MW and 6 MW, with an average hub height of about 90 m (see Table 1). While 5 MW turbines may not be installed in 2050, we included this scenario as a baseline for several reasons:

1. It provides a reference point for sensitivity analysis, allowing us to quantify how increasing turbine size to 10 MW or 15 MW influences local climate
2. Smaller turbines have different wake characteristics and turbulence patterns, which are important for understanding atmospheric responses in comparison to larger turbines
3. It reflects historical and current configurations
4. Including 5 MW ensures continuity with earlier research, as our previous studies have used this turbine size, making our results comparable (Akhtar et al. 2021, 2022, 2024, Borgers et al. 2024).

Table 1. Names, hub heights and diameters of wind farms installed in the German Bight by 2017 (thewindpower.net).

Wind farm	Hub height (m)	Diameter (m)	Turbine Capacity (MW)
AlphaVentus	90	116	5
Amrumbank West	90	120	3.6
Butendiek	90	107	3.6
Borkum Riffgrund 1	87	120	3.6
Borkum Riffgrund 2	105	154	8
DanTysk	88	120	3.6
Global Tech I and II	90	116	3.6
Godewind 1 and 2	110	154	6
Hohe See	105	154	7
Meerwind Süd and Ost	85	120	3.6
Nordsee one	100	126	6
Nordsee Ost	96.5	126	6
Riffgat	90	120	3.6

Sandbank	95	130	4
Veja Mate	103.3	154	6.25

We added a short explanation in the text (Line:106—110): "Although larger and more powerful turbines are expected to be deployed in the future, the 5 MW turbine is retained as a baseline to evaluate how scaling up to 10 MW and 15 MW influences local climate effects. Including this turbine ensures continuity with previous studies and allows for consistent comparison of results[15,16,31,32]."

2. The main impact of wind farms on wake effects and surface climate variables depends primarily on the spatial extent and capacity density of future wind farms, rather than on the capacity of individual turbines. I notice the capacity density values used in the simulations are missing from the manuscript (Table 1 appears incomplete). These data are essential to assess whether this somewhat unrealistic scenario aligns with the country's wind expansion targets. For example, studies on efficiency losses have led the German authorities to limit wind farm capacity densities to approximately 8 MW/km^2; Denmark's limits are around 4.5 MW/km^2. Why model these scenarios if they lack realism? If the authors have replaced a 5 MW turbine with a 10 MW or 15 MW turbine, this effectively doubles or triples the capacity density. This conflicts with development plans for the North Sea. Such "potent" wind farms are unlikely to be constructed because they are economically unviable. Of course, these simulations will show changes in precipitation, but this is unlikely to happen given the current development scenarios.

We used the same capacity density of 8 MW/km^2 for all experiments. The term 'turbine density' has been updated and replaced with 'capacity density,' in Table 1 and this change has also been incorporated into the main text (Line:110—111): "All experiments were conducted using the same capacity density of 8 MW/km^2."

3. Given that weather modelling is a chaotic system, the results should be tested for statistical significance, especially outside wind farm areas, because precipitation has a substantial time variability.

We appreciate the reviewer's comment regarding the chaotic nature of atmospheric modeling and the importance of evaluating statistical significance. Running a full ensemble would indeed be ideal; however, due to the computational cost of high resolution windfarm simulations, ensemble experiments were not possible within the scope of this study.

We computed grid point p-values from a paired t-test between the control and windfarm simulations and applied the Benjamini-Hochberg false discovery rate (FDR) correction to account for multiple spatial tests. Figure 1 presents the resulting significance map. Statistically significant differences ($p < 0.05$ after BHFDR correction) occur primarily over the windfarm regions, while additional significant clusters appear in some coastal areas, consistent with the mean precipitation patterns shown in the main article. The t-statistic map shows spatially coherent regions of positive and negative values across the offshore wind farm domains, with $|t|$ values frequently exceeding 5 and reaching up to ~10 in several areas. These coherent structures indicate a physically organized and nonrandom response of precipitation to the presence of offshore wind farms.

The corresponding p-value map highlights widespread areas with $p < 0.05–0.10$, demonstrating that the precipitation differences between CCLM_WF and CCLM are locally statistically significant across large parts of the domain. The agreement between high |t| regions and low p-value regions confirms that the precipitation signal is structured and statistically detectable.

These results confirm that the modeled precipitation changes over the wind-farm areas in certain downwind coastal regions are statistically significant.

We have included the figures in the Supplementary Information (see Fig. 3) and added clarifying text in the manuscript to describe these results (Line:245—249): "The statistical evaluation of precipitation differences shows spatially coherent regions of significant change, with |t| values exceeding 5 in several OWF areas. Corresponding p-value patterns ($p < 0.05$–0.10) confirm that these precipitation responses are locally significant. These results demonstrate that the modeled precipitation impact of OWFs is not random but represents a structured and detectable signal (see SI 3)."

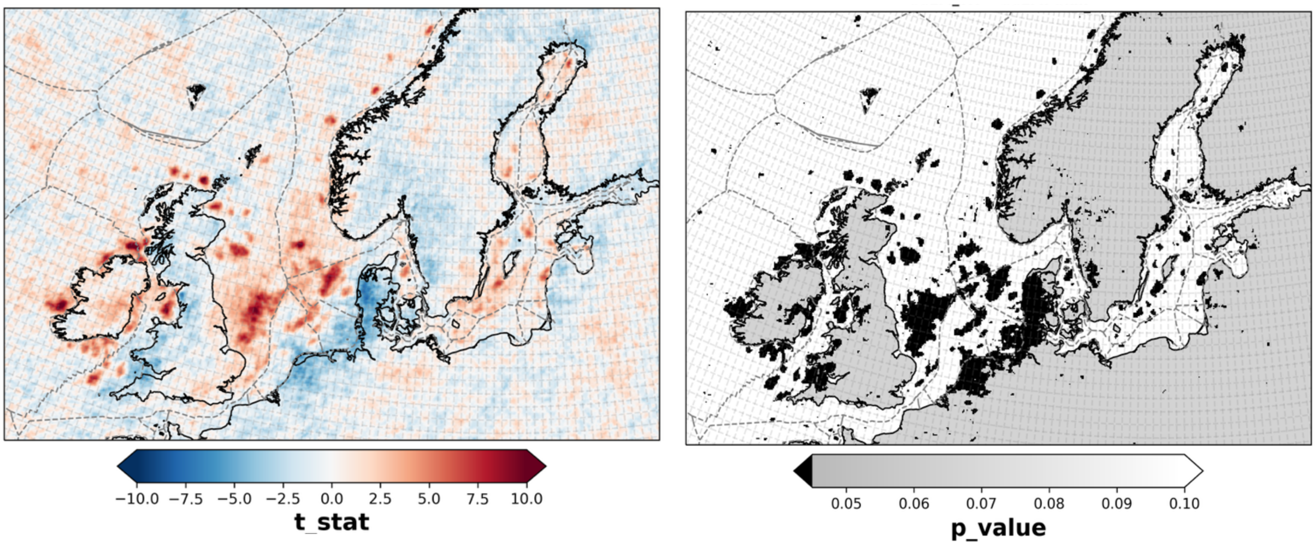


Figure 1 a) t-values and b) p values in precipitation difference between simulations with wind farms WF2050_15MW and control simulation (NOWF) for all wind directions (0—360˚) over the period of 2008—2017. This figure was created with Matplotlib (Hunter, J. D., Matplotlib: a 2D graphics environment. Computing in Science and Engineering 9, 2007) and Cartopy (Met office, Cartopy: a cartographic python library with a matplotlib interface. Exeter, Devon, https://scitools.org.uk/cartopy, 2015).

4. The vertical grid spacing is relatively coarse compared to earlier studies. The authors do not demonstrate whether the results are sensitive to changes in either spatial or vertical grid spacing.

Thank you very much for the suggestions. We have conducted several tests runs in previous years to evaluate both the horizontal and vertical resolution.

Horizontal Resolution:

Our previous publications (Akhtar et al., 2020, 2022, 2024) were based on a ~2 km horizontal grid resolution using a smaller domain that only covered the North Sea. In this study, we selected a larger domain that includes the North Sea, the Baltic Sea, as well as the UK and Irish Sea. The reason for choosing a larger domain is to simulate a broader region, including

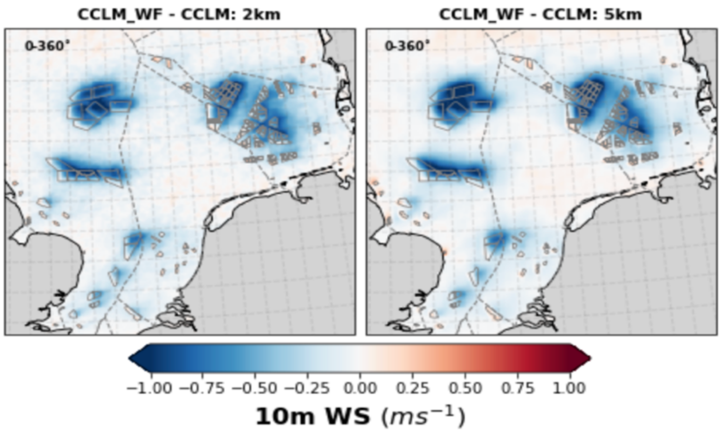
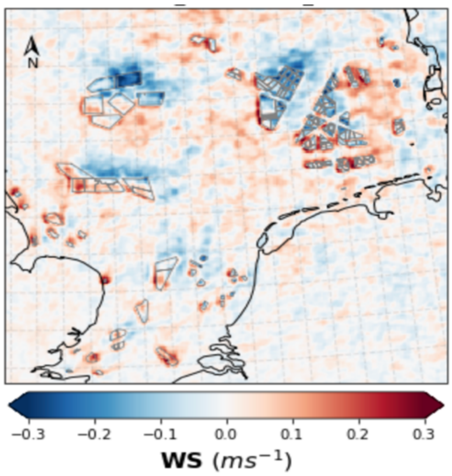


Figure 2a. 10 m wind speed difference (a) CCLM_WF and CCLM with 2 km resolution and b) CCLM_WF and CCLM with 5km resolution.

especially the Baltic Sea, and analyze the potential impact of future OWF development. To reduce computational costs, we opted for a slightly coarser horizontal resolution. Prior to running the simulations, we conducted test studies and found that the differences in wind speed between 2 km and 5 km resolution over wind farms are minor (see Figure 2a, b)



Vertical resolution:

Figure 2b. Difference in 10 m wind speed between COSMO-CLM simulations with wind farms at 5 km resolution (CCLM_WF05) and COSMO-CLM simulations with wind farms at 2 km resolution (CCLM_WF02).

We also carried out several test simulations to identify the optimal vertical resolution: standard setup with 61 levels, two additional layers below the rotor and in the rotor area (63 levels), and standard redistributed levels for 32, 64, and 128 level (see Figure 3). The results indicate that differences between the various vertical resolutions are minimal over the sea (Figure 4 and Table 22) in the range of intrinsic variability of the model. However, we selected 61 vertical model levels because the wakes were better resolved over the wind farm areas, consistent with findings from an earlier study (Akhtar et al. 2021).

We have added text in this context in Method section (see Line:127—135) and in more details in Supplementary information.

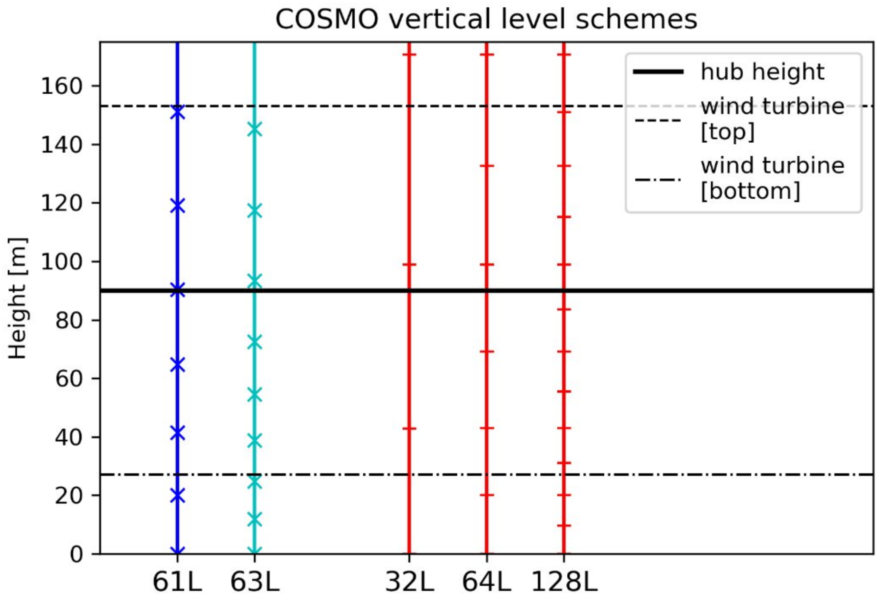


Figure 3: Illustration of the location of the model layers near the Earth's surface in the test experiments: standard setup with 61 levels, two additional layers below the rotor area (63 levels), and standard redistributed levels for 32, 64, and 128 level. Crosses and ticks mark the layer boundaries; horizontal lines indicate hypothetical hub height and rotor area of a turbine. Figure was produced by Dmitrii Kovalevskii, Hereon and is unpublished up to now.

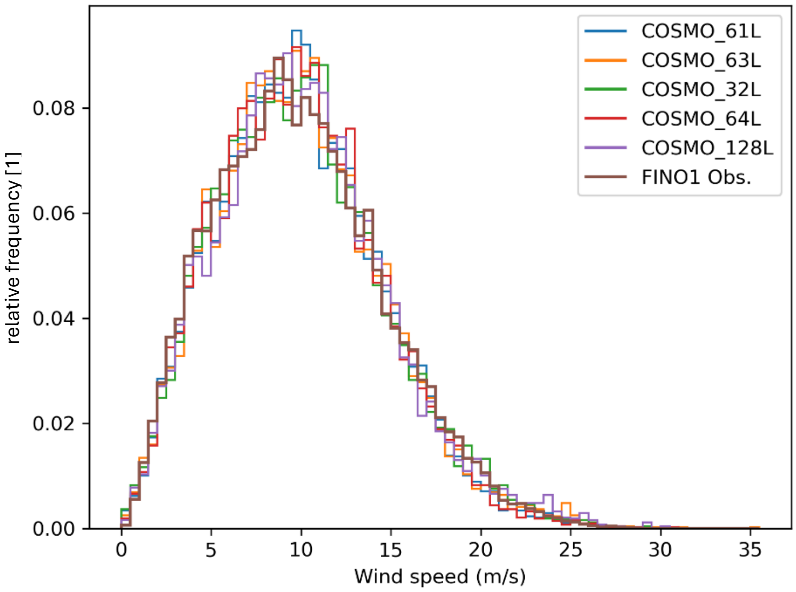


Figure 4. Wind speed relative frequency on hourly basis (bin width 0.5 m/s) for comparison between COSMO-CLM simulations with different vertical resolutions and FINO1 observational data for 2008 at a height of 90.3 m. Figure was produced by Dmitrii Kovalevskii, Hereon and is unpublished up to now.

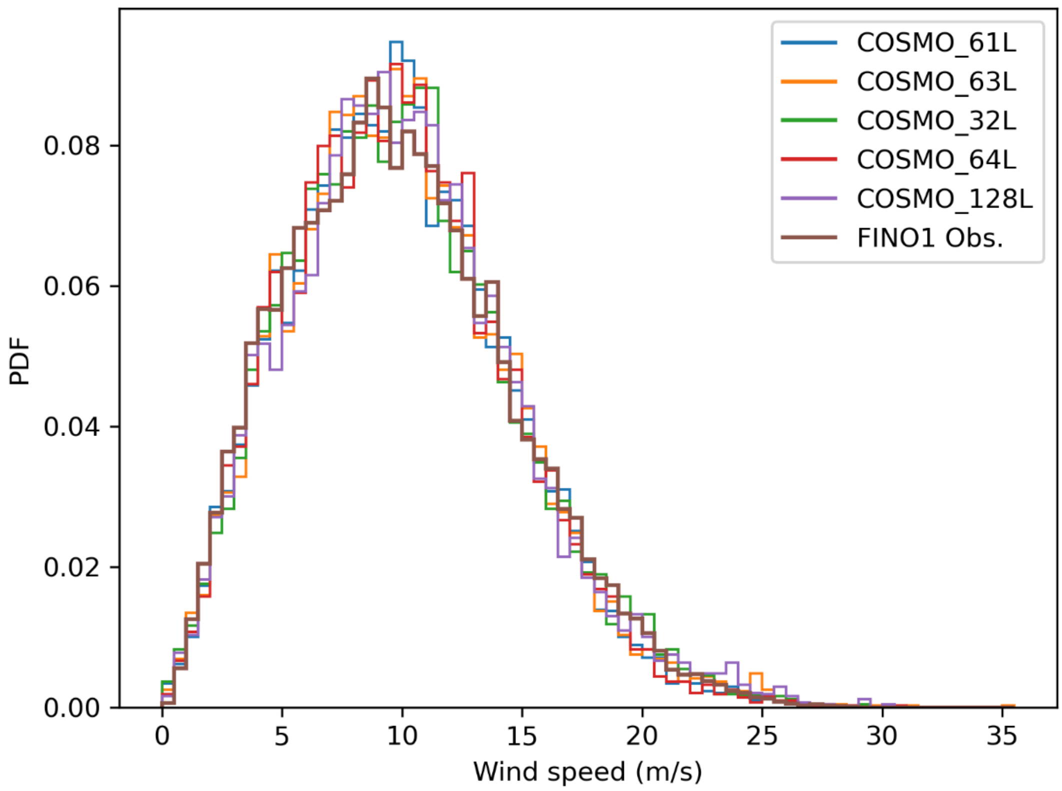
Table 22: Statistical measures of observed (at 90.3 m height) and simulated wind speed data (interpolated to 90.3 m) with different setup for vertical levels at FINO1: Mean value and standard deviation [m/s], Perkin skill score, linear correlation and RMSE for the simulations compared to observational values for 2008. The standard setup with 61 levels is marked in bold. Values delivered by Dmitrii Kovalevskii, Hereon.

	Observation	32 Level	**61 Level**	63 Level	64 Level	128 Level
Mean [m/s]	9.96	10.01	**9.91**	10.00	9.88	10.14
Std. [m/s]	4.67	4.66	**4.49**	4.64	4.47	4.74
PSS		0.96	**0.95**	0.95	0.96	0.95
Lin. correlation		0.77	**0.76**	0.75	0.75	0.75
RMSE		3.19	**3.20**	3.28	3.23	3.33

Some other minor corrections and questions:

1. L53-66. Please be more precise on whether changes are seen over land or offshore. Some of the impacts are different on these two surfaces.

Thank you for the suggestion. The studies mentioned in this paper focus on offshore wind farms. We have updated the text to clarify this point. As suggested by another reviewer, we have rearranged the text and incorporated your recommendation (Lines: 52–96) "Wind farms in the North Sea can substantially modify … *What changes in coastal precipitation patterns can be expected as a result of local climate modifications induced by extensive OWF installations in Northwest Europe?* "



2. Something is missing in Table 1. The last line is orange and not complete.

Thank you for pointing out the error. We have updated Table 1 to include the missing capacity density.

Reviewer #3

Manuscript Title: Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns
Journal: Communications Earth & Environment
General Comments:
This is a highly scientifically relevant and interesting manuscript to read. The core subject matter of the manuscript is projecting changes in coastal precipitation patterns in response to the construction of commercial offshore wind farms in the Baltic, North, and Irish Sea, as mandated by the European Commission. The authors do a very good job of articulating exactly how offshore wind farms can produce localized climate change and why these impacts extend beyond the location of these future wind farm projects. Compelling evidence is provided that explains how precipitation over Denmark, Germany, and the western half of the UK is projected to decline because of changes to moisture advection caused by offshore wind farm construction, with the paper's conclusion establishing a path forward for developing these findings in the context of hydroclimatic extremes and ecosystem sustainability. My concerns for this manuscript are mostly in its organization and some interpretations that could be more precise. Specifically, the argument constructed in the Introduction for the localized climate change induced by wind farms requires some reorganization, a few details in the model setup are missing (rationale for vertical levels and simulation period), and the explanation for the UK's projected rainfall decline needs to be explained more carefully. None of these issues are fatal, however. The manuscript is of high-quality and worthy of publication once all of my recommended changes have been made. Thus, I consider this manuscript to require only **minor revisions**

Comments on Grammar, Style, and Construction:

- There are few minor concerns with grammar throughout the manuscript, most of which I have addressed in the Line-By-Line Comments, though none of them are major.

- The content from Lines 53 to Line 86 is very relevant, but it feels disorganized. The first paragraph (Lines 53-66) starts a discussion about atmospheric dynamic response to OWFs and relates these changes to meteorological variables (temperature and specific humidity), but then returns to discussing atmospheric dynamic response in the next paragraph (Lines 67-77), followed by a glib three lines about general environmental impacts from precipitation changes (Lines 78-80) and a short paragraph focused on OWF impacts on precipitation specifically (Lines 81-86). Also, the mention of a "substantial impact on the North Sea ecosystem" in the middle of all of this feels random coming right off a discussion of atmospheric dynamic response. My advice is to restructure these paragraphs as follows:
    - Start with the general statement about local environmental impacts of OWFs and relate this to evidence for ecosystem consequences.
    - Explain how TKE, momentum, and all other atmospheric dynamic variables have been shown to respond to OWF installation in previous work.

- Then move into how meteorological variables respond to these changes in dynamics, starting with temperature and humidity and ending with precipitation, since the latter is the focus of your study.

Thank you very much for the suggestions. The text is re-structed following your suggestions to improve the readability and flow (Line 52—96).

- The manuscript switches between the phrase “onshore precipitation” and “coastal precipitation” to describe this study’s context. I recommend selecting one phrase for consistency, perhaps “coastal” rather than “onshore” since the latter can imply areas much further inland.

  Thank you for the suggestions. ‘Onshore’ is now replaced with “coastal”.

- The paragraph from Lines 207 to 212 is written in past tense (e.g., “the number of precipitation days over land *has remined* constant), even though you’re writing about projected future change in precipitation. You should change “has remained” to “is projected to remain” and do the same thing for “has increased” later in the paragraph.

  Thank you very much for the suggestions. We corrected the tenses. See Line 251 & 254.

Comments on Scientific Substance:

- The Abstract does an excellent job of highlighting the scientific contribution of this manuscript, being a topic that is not frequently studied: the potential for commercial wind energy development to induce localized climate change. The contrast with the projections of General Circulation Models, which of course do not factor infrastructural changes as a climate driver, effectively and succinctly makes the research gap that this manuscript fills very clear, pointing to a need for GCMs to account for the climatological impacts of such projected land/ocean use change.
- The opening paragraphs of the Introduction effectively set up the broad scope of this research, starting with the context of the European Commission’s targets for wind energy deployment and segueing smoothly into the challenges of micrositing wind farms in the North Sea to meet these targets, and the consequent impacts on local climate.
- The final paragraph of the Introduction starts with the phrase “To address this important question”, despite an explicit question not being asked earlier in the Introduction chapter. Based on the literature you have covered, perhaps that question would be something like: *“What are the projected impacts on coastal precipitation patterns due to the potential local climate change caused by installations of OWFs in Northwest Europe.?”* Adding that question to the end of the previous paragraph might help to explicitly state the scientific intent of your work and the question that you are seeking to answer.

Thank you. We added text according to your suggestions (Line 94—96): "This raises a critical question: *What changes in coastal precipitation patterns can be expected as a result of local climate modifications induced by extensive OWF installations in Northwest Europe?*"

- I recommend being consistent in terms of how you describe the geography of your area of interest. The manuscript title mentions "the Northwest-European Shelf", the phrase "northern European waters" is used in the Methods chapter, and mentions of the Baltic Sea stop after your Introduction chapter despite being a consistent feature of your figures. Select one phrase that describes your study area and use that phrase consistently throughout the manuscript.

Thank you very much we changed North Sea to Northwestern European Shelf.

- Lines 115-117 state that different numbers of vertical levels of the COSMO-CLM model encompass the rotor area of each of the three turbine capacities. It might be worth stating the total turbine heights (in meters) associated with these three capacities as well, just to clarify how much of the boundary layer/lower troposphere is being represented in your work. I see that information in Table 1, but I think it's worth stating it at this point of the main text too.

Thank you very much. More information is included in the text (Line:138—140): "The total heights of 5 MW, 10 MW, and 15 MW turbines are approximately 153 m, 218 m, and 270 m, respectively (see Table 1 for key parameters of the turbines used in this experiments)."

- There are a few errors and concerns regarding Tables 1 and 2:
  - Table 1 contains a row titled "Installed power", but the data entries for this row are blank.
  - Please correct the spelling mistakes in the caption for Table 2, and in Table 2 itself (e.g., "opertional", "underconstruction").

Thank you very much. Tables and captions are updated.

- The Methods section states that baseline atmospheric conditions were simulated between the years 2008 and 2017, but a reason for this specific 10-year period is not given. Some sort of scientific rationale for constructing a climatology of offshore wind speed specifically for these 10 years needs to be given.

Thank you very much for your suggestions. Added following text (Line:175—177): "We simulated atmospheric conditions for 10 years to capture a broad spectrum of weather patterns and assess the potential influence of large-scale OWF development on coastal precipitation. We selected the period 2008–2017 to be consistent with earlier studies where comparisons with different forcing data were made."

- The first sentence of the Results and Discussion chapter does not fit with the rest of the paragraph, with the first sentence discussing local climatological effects of OWFs above and below hub height and the following sentences focusing on projected changes in 10-meter wind speed. I recommend moving that first sentence to the Introduction chapter, since it would strongly support the Introduction's argument for OWFs causing localized climate change.

  Thank you. The sentence is moved to introduction (Line:55—57).

- Your description of projected changes in precipitation over the UK needs to be written more carefully. Since your analysis (as you stated in the Introduction chapter) is focused primarily on the North Sea, you should state explicitly that the projected reduction of precipitation over the western half of the UK (Great Britain specifically) is a response to OWF installation in the Irish Sea, not the North Sea. This result could be attributed to the prevailing south-westerly winds that are characteristic of Western Europe; hence, these reductions are not experienced over Ireland or the Eastern half of the UK despite their proximity to future OWF installation. If you are going to mention projected rainfall reductions over the UK, you do need to mention OWF installations in the Irish Sea as well.

  Thank you. We considered your suggestions and use some of your text for explanation (Line: 229—235): "OWF installations in the Irish Sea lead to a reduction in precipitation along the west coast of the UK, estimated at approximately 5–8% (Fig. 4b). This reduction is likely driven by the prevailing south-westerly winds characteristic of Western Europe; consequently, similar decreases are not projected over Ireland or the eastern half of the UK, despite their proximity to future OWF sites. Additionally, the western half of the UK receives more precipitation due to orographic uplift, whereas the eastern half experiences less precipitation because of the rain-shadow effect."

- The sentence on Line 204-206 feels obvious. I would consider deleting it and replacing it instead with a sentence that suggests a reason for why the 5MW wind turbine scenario produces the greatest projected changes in precipitation. Perhaps because having a larger number of smaller turbines causes an overall greater increase in TKE and advection of moisture?

  Thank you for your suggestion. We have revised the text accordingly (Line: 242–244): "This is due to the higher turbine density of 5 MW units, which results in a greater overall increase in turbulence kinetic energy (TKE) and enhanced advection of moisture."

- The paragraph in which you discuss prevailing south-westerly winds (Lines 213-222) is perhaps where you should mention how OWF installation in the Irish Sea influences rainfall over the western half of the UK. It might also be worth mentioning that the orography of the UK causes a rain-shadow effect, preventing this increase in precipitation from affecting the eastern half of the country.

Thank you for your suggestion. We added a sentence on that as already mentioned above (Line:231—235).

- Vertical wind speed changes are expected to be small, meaning that a projected change of around 0.003 m/s over the OWFs does require some context. I highly recommend conducting some form of statistical test (besides just total mean difference) that determines whether these projected changes of vertical wind speed are statistically significant.

We performed grid-wise t-tests comparing the wind-farm and control configurations (CCLM_WF – CCLM) and mapped both t-statistics and p-values (see Fig. 1a, b)**.** The results demonstrate spatially coherent regions of significant change, with extensive areas showing $p < 0.05$–0.10 and large $|t|$ values. These findings confirm that, beyond the overall mean difference (~0.003 m s^{-1}), the projected changes in vertical wind speeds are statistically significant across substantial portions of the OWF domains.

We have included the figures in the Supplementary Information (Fig. SI 12) and added clarifying text in the manuscript to describe these results (Line:296—302): "The t-statistic map exhibits spatially coherent bands of positive and negative values aligned with OWF footprints and coastal features, with $|t|$ frequently exceeding 5 and reaching ~20 in several corridors. The p-value map shows extensive areas with $p < 0.05$–0.10, indicating that differences between CCLM_WF and CCLM are locally significant across large portions of the OWF domains. The consistency between high $|t|$ regions and low p-values confirm a structured, statistically detectable signal (see Fig. SI 12)."

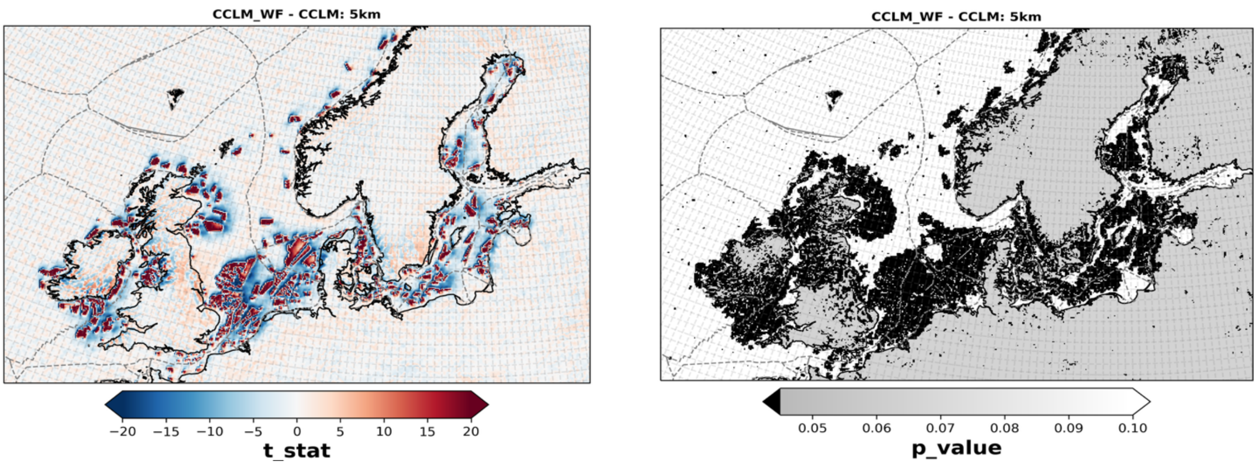


Figure 5. Significance analysis of vertical wind speed at 150 m height: a) t-values of difference of CCLM_WF and CCLM b) p values of difference of CCLM_WF and CCLM.

Point to point reply

Dear Editor,

We sincerely thank all three reviewers for their valuable suggestions and constructive comments, which have greatly improved the quality of our manuscript entitled *"Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns."*

In response to the reviewers #2 and #3 feedback, we have revised the abstract and conclusions to specifically address the concerns raised by Reviewer #2. We have also carefully addressed the points highlighted by the Editor in relation to Reviewer #2's questions.

Along with the revised manuscript, we submit a tracked-changes version of the paper and a detailed point-by-point response document, in which each reviewer comment is addressed individually and the corresponding revisions are clearly explained.

We sincerely appreciate the time and effort invested by the reviewers and the editorial team. Their insightful feedback has significantly strengthened the manuscript, and we hope that the revised version now meets the requirements for publication.

Yours sincerely,

Kind regards

Naveed Akhtar

Reviewer #2 (Remarks to the Author):

1. The scenarios (WF50_5MW, WF50_10MW, and WF50_15MW) used in the simulations grossly exaggerate the future wind turbine scenarios in the North Sea. For example, for the Danish North Sea, the area considered is about 10.000 km2. But this area is reserved for development, not meaning that the full area will be used for wind farms. If it were, at an installed capacity density of 8 MW km^{-2}, it would result in 80 GW in 2050. The current very ambitious Danish target is 35 GW, as detailed in Nordic Oceans 2050. Thus, the simulations in the manuscript more than double the projected installed capacity in the Danish North Sea in a single wind farm or nearly match the UK target of 95 GW. It is therefore not surprising that the impacts of wind farms are exaggerated in the simulations presented here. The key point is that the study does not emphasise, how unlikely these scenarios are and how unlikely it is that intense effects on precipitation will be seen under the scenarios currently considered by the North Sea countries.

We thank the reviewer for highlighting the importance of placing the wind-farm scenarios in a clear policy and planning context. We acknowledge that the scenarios used imply high installed capacity densities and exceed currently adopted national planning targets when interpreted at the country scale. However, recent political discussions indicate substantial uncertainty in national future offshore wind deployment pathways. Notably, during the 3rd North Sea Summit in 2026 it was agreed that 100 GW of the 300 GW North Sea target would be pursued through cross-border cooperation, partly in response to economic and spatial constraints on national targets such as the German 70 GW goal. Subsequent political communication has emphasized closer cooperation with Denmark and the Netherlands, indicating that large-scale transnational deployment scenarios remain under discussion.

Within this context, the scenarios used in this study represent technical scenarios designed to explore upper limits of offshore wind-farm deployment within designated areas including the development zones for 2050, rather than policy-based projections or predictions. While they exceed current national plans, they allow investigation of how atmospheric responses scale with wind-farm area coverage and installed capacity density. To our knowledge, no studies have examined atmospheric impacts at scales approaching the basin-wide plans with most existing work focusing on individual wind farms, single sites, or limited temporal periods.

We further clarify that changes in precipitation depend strongly on wind-farm area coverage, turbine density, deployment configuration, and atmospheric conditions. These dependencies are addressed through multi-year simulations that sample a wide range of meteorological situations and seasons. The revised manuscript will more clearly state that further studies using different regional climate models, wind-farm scenarios, and turbine densities are required to provide more realistic estimates of potential precipitation changes under policy-relevant North Sea development pathways.

NOTE: To clarify this, we added some text in abstract (L14:24) and conclusions

2. Previous studies have found very limited credible mesoscale-model evidence that wind farms can influence precipitation, but only under certain conditions, particularly near and downwind of very large wind-farm clusters during favourable atmospheric regimes (Pryor et al. 2018; Fischereit et al. 2021). However, the results are not consistently positive or negative and are highly sensitive to model choices, weather regimes, and the scale of analysis. Reliable attribution requires multi-parameterisation ensembles, high-resolution coupling with microscale models, and targeted observational validation. The study cannot, of course, address all these aspects, but the uncertainty must be emphasised in the manuscript.

Based on this, the manuscript should be very clear that such an intense response to wind farms is observed in a single set of simulations using a single mesoscale model, with a single setup and a single wind farm parameterisation. Similar studies using other models have not shown this. I could not find a single study using the WRF model that reports any systematic changes in precipitation; two report case studies, one with hurricanes.

We thank the reviewer for this important comment and for emphasizing the need to properly contextualize our findings within the existing observational and mesoscale-modeling literature. The reviewer stated that wind-farm impacts on precipitation are generally limited, highly condition-dependent, and sensitive to model choice, atmospheric regime, and spatial scale, consistent with previous studies (e.g., Pryor et al., 2018; Fischereit et al., 2021). BUT: In our view, contrary to that of the reviewer, these studies indicate that wind farms can be associated with increased precipitation over the wind-farm area and reduced precipitation in downstream regions, highlighting a redistribution of precipitation rather than a uniform response. Pryor et al. (2018) likewise report small but statistically significant precipitation changes in summer, while Fischereit et al. (2021) conclude that results are non-conclusive overall but document precipitation responses in specific regimes. The study by Pryor is based on onshore analysis, while Fischereit gives an overview over inshore and offshore findings. As the impact differs, it is important to distinguish the impact of offshore and onshore wind farms.

Further regional and mesoscale modeling studies support this conditional response. For example, Fiedler and Bukovsky (2011) and Boettcher et al. (2015) show that sufficiently large wind-farm deployments can modify regional precipitation or cloud-related processes under certain conditions. Event-based WRF studies, such as Pan et al. (2018), which is already cited and discussed in the main text, further demonstrate that wind-farm-induced redistribution of precipitation can occur during extreme weather events.

Not only modeling studies but also observational studies demonstrate the physical plausibility of wind-farm influences on atmospheric processes. Observational evidence shows that offshore wind farms measurably modify boundary-layer flow through wake-induced wind-speed deficits and enhanced turbulence (Barthelmie et al., 2003; Jensen et al., 2004). More recent observational work by Al Fahel and Archer (2020) further indicates statistically significant changes in downstream precipitation following offshore wind-farm installation, even for comparatively small developments, depending on wind direction and atmospheric conditions.

We fully acknowledge that the present study is based on a single regional model, one setup, and one wind-farm parameterization, and thus cannot provide robust attribution across models. However, with our multi-year simulation we resolve the impact of various different weather situations, in contrast to other studies which only focus on single specific weather situations. As mentioned in the response to the first comment the revised manuscript will emphasize that the simulated precipitation response is the first study resolving multi-year scales, and that further works with different models or parametrization setups, wind-farm scenarios, and deployment densities are desirable to obtain a broader view on estimates of potential precipitation changes, considering model- and scenario uncertainty.

We have added the text in Abstract (L 21-23) and Conclusions (L346-357).

3. The authors respond that their results are not significantly affected by spatial or vertical resolution. However, the figures they included in their response to the review are for 10-m wind speed, not precipitation. Thus, this question remains unanswered. Additionally, there is no mention of the evaluation of the precipitation simulated by this model over, for example, Denmark.

Horizontal resolution

We thank the reviewer for pointing out that our previous response did not explicitly state that a precipitation comparison between the 5 km and 2 km simulations had not been conducted. To address the reviewer's question, we have now carried out an additional set of one-year-5 km simulations with the same windfarm setup as our 'old' 2 km simulations to compare the precipitation amounts between the 5 km and 2 km configurations. Figure 1 (below) shows the differences between windfarm and control simulations a) for our 'old' 2 km 10-years-simulation described in detail by us (Akhtar et al., 2022), b) for the same simulation but for the year 2008 and c) for the new 2008-5km-simulation. The results show that the pattern of precipitation changes for one year, here 2008, is due to the nature of the system much noisier than for 10 years. The systematic changes over wind farms are overlayed by year specific patterns in the 2008 case. The results of the 5 km simulations show the changes over the wind farms in the same order of magnitude. Additionally, we see year specific deviations between the 2 km and the 5 km, which might be due to the disturbance introduced by the resolution change.

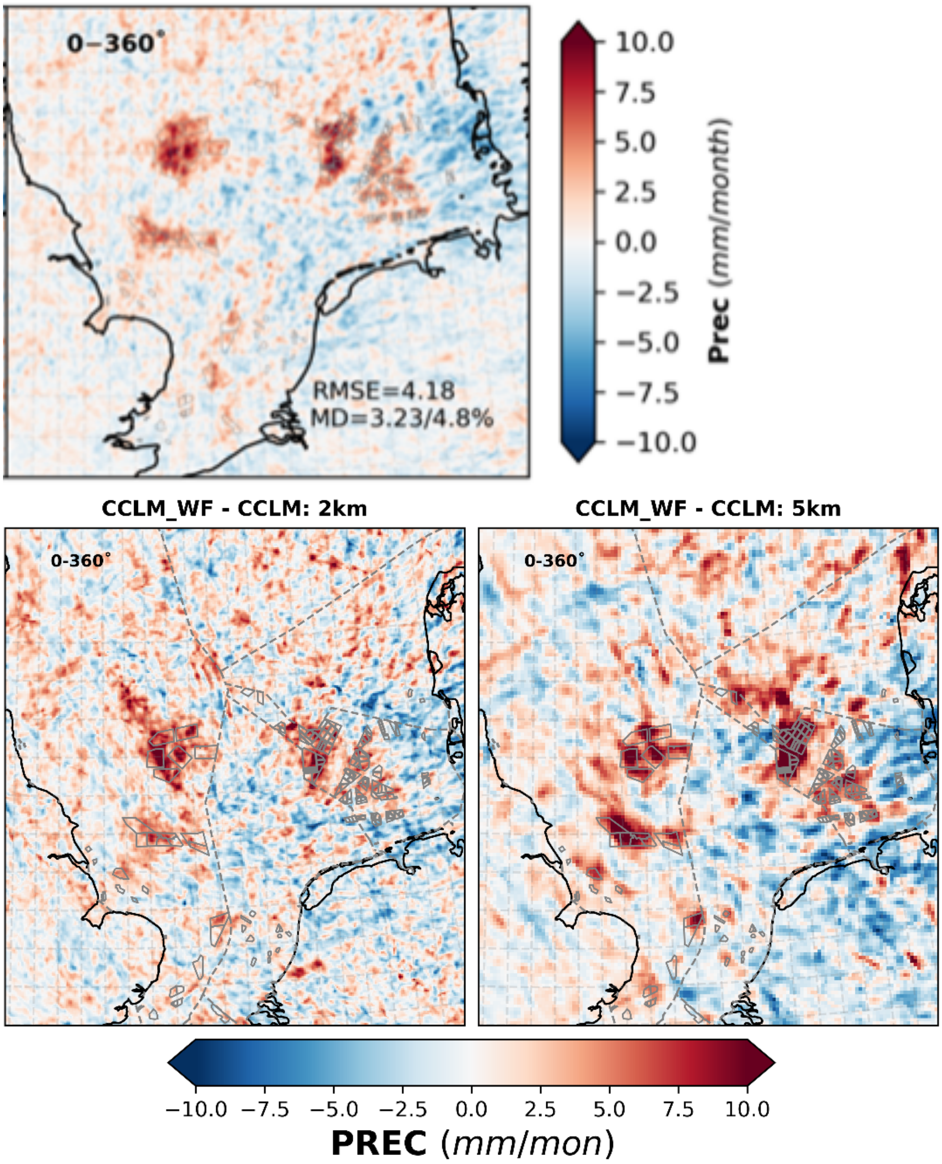


Figure 1: Total precipitation difference [mm/month] between CCLM_WF and CCLM with (a) 2 km resolution for 2008 – 2017 and b) 2 km resolution for 2008 and c) 5 km resolution for 2008.

Vertical resolution

The sensitivity experiments addressing vertical resolution were conducted to analyze the impact on wind speed, as wind speed is the primary variable directly affected by the wind-farm parameterization. Repeating the same vertical-resolution sensitivity experiments while additionally outputting precipitation would require a substantial number of further long simulations and is therefore not feasible due to the high computational cost. As stated before we did not find improvements on the simulated wind speeds compared to observations.

Previous studies by other authors have shown that changes in vertical resolution primarily affect boundary-layer processes and near-surface wind fields, while their influence on precipitation—especially over marine and offshore regions is comparatively smaller. When increasing the vertical resolution the effects on total precipitation are often weak or compensating due to offsetting changes in convective and large-scale precipitation components (Lee et al., 2019). Over ocean and offshore environments, precipitation is predominantly controlled by large-scale circulation and moisture advection, for which sensitivity to vertical resolution is limited when compared to near-surface wind fields (Iles et al., 2020; Sørland et al., 2021). Consistent with these findings, COSMO-CLM and other regional climate models show that improvements in vertical resolution mainly enhance the representation of the atmospheric boundary layer and wind profiles, while precipitation characteristics—particularly over low-relief and marine regions—remain largely unaffected (Heinemann et al., 2023; Sørland et al., 2021). For this reason, we focused our vertical-resolution analysis on wind speed, which is the most relevant variable for assessing wind-farm impacts.

Evaluation of the precipitation

COSMO-CLM is one of the most used models in the European region. It is well tuned model for the Euro-cordex region. Previous studies indicate that COSMO-CLM reproduces the mean and seasonal characteristics of precipitation over Denmark reasonably well, while exhibiting a wet-day bias and an underestimation of convective extremes, consistent with findings over Northern Europe (Seaby et al., 2019; Sørland et al., 2021).

4. The authors write in the response to Reviewer # 1: "The simulations are not deterministic". But this statement is incorrect. A good example of this is provided in the work of Lorenz et al (2016). The introduction of wind farms into numerical simulations is not unlike simulating the effects of tropical deforestation: surface properties change, and we use the model to study how these changes propagate to other atmospheric phenomena. As demonstrated by Lorenz's study, changes in a single grid point can propagate throughout the domain in a non-physical manner. Thus, testing the significance of the results against the background variability is a must. Additionally, atmospheric phenomena are heavily autocorrelated; thus, significance testing is not as straightforward as the authors make it sound. See, e.g., the textbook by Wilks (2019), Chapter 5.

We thank the reviewer for this clarification and would like to emphasize that we **did not** state that the simulations themselves are non-deterministic**.** We fully agree that the numerical simulations are deterministic in the strict sense, as they are governed by deterministic model equations given fixed initial conditions, boundary conditions, and model configurations.

Our intention in the earlier response was not to question the deterministic nature of the simulations, but to describe the interpretation of the results, which are embedded in an atmospheric system characterized by strong internal variability and chaotic behavior. The panels shown in Figures 2–6, S1, S2, S5–S7, S9, and S10 present multi-year mean differences between paired simulations with and without offshore wind farms. These diagnostics are designed to highlight systematic and persistent climatological deviations that emerge after averaging over extended time periods, rather than to depict event-scale variability or distributions of independent samples.

By employing multi-year averaging, we aim to reduce the influence of short-term chaotic fluctuations and emphasize structurally coherent and physically interpretable shifts in the mean climate state associated with the imposed wind-farm forcing. We do not claim that such averaging removes internal variability entirely, nor that it eliminates the need for statistical inference in general. We also acknowledge that atmospheric variables exhibit strong temporal and spatial autocorrelation, making formal significance testing non-trivial, as discussed in the literature (e.g., Wilks, 2019).

5. The authors added the installed capacity density to Table 1. However, installed capacity density is a measure of the scenario (i.e., how many turbines are in an area based on their capacity), not the turbine itself. Additionally, the installed capacity for each country should be reported in the manuscript so it can be compared with public scenarios from the Danish Energy Agency and similar authorities in other countries. This is the value that energy planners rely on for their future planning. Abstract and Plain text summary. The abstract does not include the caveats of their study, including that the study used a regional single model and unrealistically high installed capacity densities.

We acknowledge that installed capacity density is a characteristic of the deployment scenario, rather than of the individual turbine. Our intention in adding this metric to Table 1 was to transparently describe the overall intensity of the modeled wind-farm deployment.

We are aware that each North Sea country follows country-specific offshore wind planning frameworks, including different assumptions regarding capacity density and spatial deployment. Reporting installed capacity for each country is beyond the scope of the present study, as installed capacity can vary substantially from one wind farm to another, and comprehensive, consistent data are not available for many planned or reserved wind-farm areas. Nevertheless, we explicitly state in the manuscript that installed capacity differs between countries and between individual wind farms. (L122-114)

As stated in our response to Comment #1, the scenarios used in this study represent technical upper-limit scenarios designed to explore potential atmospheric responses under high deployment intensities, rather than country-specific or policy-based planning scenarios. This clarification will be emphasized consistently throughout the manuscript to avoid misinterpretation of the modeled scenarios relative to national offshore wind targets.

References

The manuscript is very heavily self-cited. Of the about 15 references about wind farm wakes, 8 are from the author's research groups. Work from several other important groups in Europe (e.g., Fraunhofer IWES in DE, DTU Wind and DMI in DK, KNMI in NL) and the USA (e.g., Cornell Univ, NREL, NCAR) is not cited at all.

Thank you for the reviewer's comments regarding the citation balance. We would like to clarify that we made a concerted effort to cite studies that are directly relevant to the objectives and methodology of this work, resulting in more than 40 references overall.

A considerable fraction of the self-citations relates specifically to the COSMO-CLM model configuration, the wind-farm parameterization implemented in COSMO-CLM, and associated model development steps. These aspects are central to the present study and, given that these developments were led by our research group, references to our previous work are necessary to ensure transparency, reproducibility, and proper documentation of the modeling framework. In addition, some of our earlier studies are directly built upon in this manuscript and therefore need to be cited for methodological continuity. In the introduction and the conclusions, the citations are well balanced but not focusing on case studies. We don't check for a proper institutional coverage but concentrate on the paper contents.

References:

Fischereit, J., Brown, R., Larsn, X., Badger, J., & Hawkes, G. (2021). Review of Mesoscale Wind-Farm Parametrizations and Their Applications. Boundary-Layer Meteorology, 182(2), 175-224. https://doi.org/10.1007/s10546-021-00652-y

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Reviewer #3 (Remarks to the Author):

This manuscript is almost ready! The authors have done an excellent job at addressing the reviewers' comments. I just have a few small copy-edits and two minor suggestions to incorporate before this manuscript is published.

Line 14: Delete "Baltic Sea" or change to "and the Baltic Sea" if you choose to consider OWF development in this region as well. Also, there is a missing comma after the word "period".

Thank you for suggestions. 'Baltic Sea is changed to "and the Baltic Sea"

Line 25: Change "the temperature rise" to "global average temperature increases above pre-Industrial levels". This is more precise wording.
Thank you for suggestions. Accpeted

Line 45: Change "wakes areas" to "wake areas".
Thank you for suggestions. Accpeted

Lines 52-54: Delete these two sentences. It's too short of a paragraph to stand alone and disrupts flow between the previous and next paragraphs, which have a more logical progression of topics.
Thank you for suggestions. Accpeted

Line 74: Delete "and minimal", you don't want to undervalue the impacts that OWFs can have on local climate, since that's what you are arguing here.

Thank you for suggestions. Accpeted.

Lines 81-82: Missing commas after "installations" and "precipitation".

Thank you for suggestions. Accpeted.

Line 99: Change "includes" to "include", since "data" is a plural word.

Thank you for suggestions. Accpeted.

Lines 150-156: These details about why the COSMO-CLM model was used should be placed at the start of this section, not the end. This rationale would make more sense immediately after introducing the model itself, perhaps after Line 123 and before explaining your grid resolution.

Thank you for suggestions. The text is moved in the begning of the section.

Line 319-322: I think you should explicitly state that IPCC AR6's GCM simulations do not account for OWF development's effects on regional/local climate, hence the discrepancy that exists, meaning that an argument exists for the use of COSMO-CLM and other similar models for providing robust climate simulations at smaller scales that account for the effects of sustainable energy transitions. This would help make the point of your manuscript and its contribution to the broader climate literature stronger.
Thank you very much for the suggestion. We have added the following text (L:336-342)

“The results highlight an important limitation of IPCC AR6 GCM simulations, which do not explicitly represent the regional and local climatic effects of offshore wind farm (OWF) development. This helps explain discrepancies between large-scale projections and the findings of this study. In contrast, high-resolution regional climate models such as COSMO-CLM are better suited to capturing OWF-induced mesoscale processes, underscoring their value for robust climate assessments at smaller spatial scales in the context of sustainable energy transitions."

Point to point reply

Dear Editor,

We would like to express our sincere gratitude to all three reviewers for their insightful suggestions and constructive feedback, which have greatly improved the quality of our manuscript entitled *'Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns'.* Renamed as per journal suggestions *to 'Projected impacts of future offshore wind farms on coastal precipitation over the Northwest European shelf '*

We are pleased to note that all reviewers support the improvement and publication of the manuscript. We are particularly appreciative that Reviewer #2 has acknowledged our responses and the improvements made. Reviewer #3 has suggested only minor typesetting revisions. In response to the feedback from Reviewers #3, we have revised the manuscript.

Along with the revised manuscript, we submit a tracked-changes version and a detailed point-by-point response document, in which each reviewer comment is addressed individually, and the corresponding revisions are clearly explained.

We sincerely appreciate the time and effort devoted by the reviewers and the editorial team. Their insightful feedback has greatly strengthened the manuscript, and we hope that the revised version now meets the requirements for publication

Yours sincerely,

Naveed Akhtar

Reviewer #2 (Remarks to the Author):

While the study's design tends to exaggerate the impact of large offshore wind farms on precipitation patterns, I appreciate that the authors have provided sufficient caveats in the abstract and the manuscript's conclusions. These acknowledgements help clarify the limitations and potential uncertainties associated with the findings. Given this consideration, I am willing to approve the manuscript for publication.

We sincerely thank the reviewer for the careful evaluation of our manuscript and for recognizing both the strengths and limitations of our study. We also appreciate your helpful comment about the possibility that our study design may overestimate the impact of large offshore wind farms on precipitation patterns. Your feedback and suggestions were very helpful in improving the manuscript.

Reviewer #3 (Remarks to the Author):

This is my third time viewing this manuscript, and at this point I am more than content with its scientific content! I only have copy-edits to offer:

Lines 56-58: Add these two sentences into the next paragraph, such a short paragraph on its own like this looks a little odd.

Thank you for your valuable suggestion. We have accepted and incorporated it into the manuscript

Lines 62-63: You already defined TKE as an acronym on Line 54, no need to do it twice!

Thank you for your valuable suggestion. We have accepted and incorporated it into the manuscript

Lines 121-122: "In this study, we focus on the impact of OWFs on coastal 121 precipitation in the Northwest-European shelf." Delete this sentence, or delete the mention of the Baltic Sea in the next sentence. It's odd to state you are focusing on the Northwest-European shelf to then mention the Baltic Sea again immediately afterwards and again later in the Methods chapter.

Thank you for your suggestions. We have rephrased the sentence to make it clearer.

Line 170: Same comment as above about acronyms, no need to define them again! Same again on Line 251.

Thank you for the suggestion. Accepted and revised accordingly

It looks like the authors did a good job addressing Reviewer #2's comments too!

Thank you for appreciation

Reviewer Comments:

Manuscript Title: Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns

Journal: Communications Earth & Environment

General Comments:

This is a highly scientifically relevant and interesting manuscript to read. The core subject matter of the manuscript is projecting changes in coastal precipitation patterns in response to the construction of commercial offshore wind farms in the Baltic, North, and Irish Sea, as mandated by the European Commission. The authors do a very good job of articulating exactly how offshore wind farms can produce localized climate change and why these impacts extend beyond the location of these future wind farm projects. Compelling evidence is provided that explains how precipitation over Denmark, Germany, and the western half of the UK is projected to decline because of changes to moisture advection caused by offshore wind farm construction, with the paper's conclusion establishing a path forward for developing these findings in the context of hydroclimatic extremes and ecosystem sustainability. My concerns for this manuscript are mostly in its organization and some interpretations that could be more precise. Specifically, the argument constructed in the Introduction for the localized climate change induced by wind farms requires some reorganization, a few details in the model setup are missing (rationale for vertical levels and simulation period), and the explanation for the UK's projected rainfall decline needs to be explained more carefully. None of these issues are fatal, however. The manuscript is of high-quality and worthy of publication once all of my recommended changes have been made. Thus, I consider this manuscript to require only **minor revisions**.

Comments on Grammar, Style, and Construction:

- There are few minor concerns with grammar throughout the manuscript, most of which I have addressed in the Line-By-Line Comments, though none of them are major.
- The content from Lines 53 to Line 86 is very relevant, but it feels disorganized. The first paragraph (Lines 53-66) starts a discussion about atmospheric dynamic response to OWFs and relates these changes to meteorological variables (temperature and specific humidity), but then returns to discussing atmospheric dynamic response in the next paragraph (Lines 67-77), followed by a glib three lines about general environmental impacts from precipitation chaneges (Lines 78-80) and a short paragraph focused on OWF impacts on precipitation specifically (Lines 81-86). Also, the mention of a "substantial impact on the North Sea ecosystem" in the middle of all of this feels random coming right off a discussion of atmospheric dynamic response. My advice is to restructure these paragraphs as follows:
    - Start with the general statement about local environmental impacts of OWFs and relate this to evidence for ecosystem consequences.

    - Explain how TKE, momentum, and all other atmospheric dynamic variables have been shown to respond to OWF installation in previous work.
    - Then move into how meteorological variables respond to these changes in dynamics, starting with temperature and humidity and ending with precipitation, since the latter is the focus of your study.
- The manuscript switches between the phrase “onshore precipitation” and “coastal precipitation” to describe this study’s context. I recommend selecting one phrase for consistency, perhaps “coastal” rather than “onshore” since the latter can imply areas much further inland.
- The paragraph from Lines 207 to 212 is written in past tense (e.g., “the number of precipitation days over land *has remained* constant), even though you’re writing about projected future change in precipitation. You should change “has remained” to “is projected to remain”, and do the same thing for “has increased” later in the paragraph.

Comments on Scientific Substance:

- The Abstract does an excellent job of highlighting the scientific contribution of this manuscript, being a topic that is not frequently studied: the potential for commercial wind energy development to induce localized climate change. The contrast with the projections of General Circulation Models, which of course do not factor infrastructural changes as a climate driver, effectively and succinctly makes the research gap that this manuscript fills very clear, pointing to a need for GCMs to account for the climatological impacts of such projected land/ocean use change.
- The opening paragraphs of the Introduction effectively set up the broad scope of this research, starting with the context of the European Commission’s targets for wind energy deployment and segueing smoothly into the challenges of micrositing wind farms in the North Sea to meet these targets, and the consequent impacts on local climate.
- The final paragraph of the Introduction starts with the phrase “To address this important question”, despite an explicit question not being asked earlier in the Introduction chapter. Based on the literature you have covered, perhaps that question would be something like: *“What are the projected impacts on coastal precipitation patterns due to the potential local climate change caused by installation of OWFs in Northwest Europe?”* Adding that question to the end of the previous paragraph might help to explicitly state the scientific intent of your work and the question that you are seeking to answer.
- I recommend being consistent in terms of how you describe the geography of your area of interest. The manuscript title mentions “the Northwest-European Shelf”, the phrase “northern European waters” is used in the Methods chapter, and mentions of the Baltic Sea stop after your Introduction chapter despite being a consistent feature of your figures. Select one phrase that describes your study area and use that phrase consistently throughout the manuscript.
- Lines 115-117 state that different numbers of vertical levels of the COSMO-CLM model encompass the rotor area of each of the three turbine capacities. It might be worth stating

the total turbine heights (in meters) associated with these three capacities as well, just to clarify how much of the boundary layer/lower troposphere is being represented in your work. I see that information in Table 1, but I think it's worth stating it at this point of the main text too.

- There are a few errors and concerns regarding Tables 1 and 2:
  - Table 1 contains a row titled "Installed power", but the data entries for this row are blank.
  - Please correct the spelling mistakes in the caption for Table 2, and in Table 2 itself (e.g., "opertional", "underconstruction").
- The Methods section states that baseline atmospheric conditions were simulated between the years 2008 and 2017, but a reason for this specific 10-year period is not given. Some sort of scientific rationale for constructing a climatology of offshore wind speed specifically for these 10 years needs to be given.
- The first sentence of the Results and Discussion chapter does not fit with the rest of the paragraph, with the first sentence discussing local climatological effects of OWFs above and below hub height and the following sentences focusing on projected changes in 10-meter wind speed. I recommend moving that first sentence to the Introduction chapter, since it would strongly support the Introduction's argument for OWFs causing localized climate change.
- Your description of projected changes in precipitation over the UK needs to be written more carefully. Since your analysis (as you stated in the Introduction chapter) is focused primarily on the North Sea, you should state explicitly that the projected reduction of precipitation over the western half of the UK (Great Britain specifically) is a response to OWF installation in the Irish Sea, not the North Sea. This result could be attributed to the prevailing south-westerly winds that are characteristic of Western Europe; hence, these reductions are not experienced over Ireland or the Eastern half of the UK despite their proximity to future OWF installation. If you are going to mention projected rainfall reductions over the UK, you do need to mention OWF installations in the Irish Sea as well.
- The sentence on Line 204-206 feels obvious. I would consider deleting it and replacing it instead with a sentence that suggests a reason for why the 5MW wind turbine scenario produces the greatest projected changes in precipitation. Perhaps because having a larger number of smaller turbines causes an overall greater increase in TKE and advection of moisture?
- The paragraph in which you discuss prevailing south-westerly winds (Lines 213-222) is perhaps where you should mention how OWF installation in the Irish Sea influences rainfall over the western half of the UK. It might also be worth mentioning that the orography of the UK causes a rain-shadow effect, preventing this increase in precipitation from affecting the eastern half of the country.
- Vertical wind speed changes are expected to be small, meaning that a projected change of around 0.003 m/s over the OWFs does require some context. I highly recommend conducting some form of statistical test (besides just total mean difference) that

determines whether these projected changes of vertical wind speed are statistically significant.
- Fantastic Conclusion chapter, it ties the whole paper together well and opens the doors to clear direction for future work that build directly off of this manuscript.

Line-By-Line Comments:

- Line 26: Change “limit the temperature rise” to “limit global average temperature change beyond pre-Industrial levels”, just for the sake of precision.
- Line 39: Change “This indicates” to “These properties mean”.
- Line 43: The phrase “allowing 2-3 times longer power generation” reads a little strangely, because that phrase implies power generation lasting longer throughout the day, whereas what I think you mean is that offshore wind energy “allows for 2-3 times greater total power generation”. Consider this rephrase, and if that’s not what you meant, please phrase this more carefully.
- Line 68: Delete the word “minimal”, you don’t want to diminish the importance of your research contribution by suggesting that wind farm impacts on local climate do not matter!
- Line 132: You have already defined TKE as an acronym in the Introduction chapter, there is no need to reiterate the acronym here.
- Line 146: Replace “installation” with “installed”.
- Line 147: Add a comma after “In total”.
- Line 171: Change “values as strong” to “reductions as great”.
- Line 175-176: As with the comment about TKE on Line 132, you do not need to reiterate an acronym that you have already defined, in this case being OWFs.
- Line 248: Replace “though” with “thus”.
- Line 257-258: The sentence “The transport of the drier air includes even far areas and is reaching the coasts leading to reductions in precipitation there” doesn’t read quite right, I’m not sure what you are trying to say here. I think you are trying to say that enhanced states of vertical mixing over OWFs produce drier conditions that extend further inland, if so, rewrite the sentence to reflect this.
- Line 287: Change “support” to “support for”.

Review of: Future Offshore Wind Farms on the Northwest-European Shelf: Projected Impacts on Coastal Precipitation Patterns

March 24, 2026

Recommendation

I recommend the manuscript be **rejected** for Communications Earth & Environment. The reasons for my decision are listed below.

Scientific rigour

After the last set of revisions, the manuscript still lacks

1. The scenarios (WF50_5MW, WF50_10MW, and WF50_15MW) used in the simulations grossly exaggerate the future wind turbine scenarios in the North Sea. For example, for the Danish North Sea, the area considered is about 10.000 km^2. But this area is reserved for development, not meaning that the full area will be used for wind farms. If it were, at an installed capacity density of 8 $MW\,km^{-2}$, it would result in 80 GW in 2050. The current very ambishous Danish target is 35 GW, as detailed in Nordic Oceans 2050. Thus, the simulations in the manuscript **more than double the projected installed capacity in the Danish North Sea** in a single wind farm, or nearly match the UK target of 95 GW.

   It is therefore not surprising that the impacts of wind farms are exaggerated in the simulations presented here. The key point is that **the study does not emphasise how unlikely these scenarios are** and how unlikely it is that intense effects on precipitation will be seen under the scenarios currently considered by the North Sea countries.

2. Previous studies have found very limited credible mesoscale-model evidence that wind farms can influence precipitation, but only under certain conditions, particularly near and downwind of very large wind-farm clusters during favourable atmospheric regimes (Pryor et al. 2018; Fischereit et al. 2021). However, the results are not consistently positive or negative and are highly sensitive to model choices, weather regimes, and the scale of analysis. Reliable attribution requires multi-parameterisation ensembles, high-resolution coupling with microscale models, and targeted observational validation. The study cannot, of course, address all these aspects, but the uncertainty must be emphasised in the manuscript.

Based on this, the manuscript should be very clear that such an intense response to wind farms is observed in a single set of simulations using a single mesoscale model, with a single setup and a single wind farm parameterisation. Similar studies using other models have not shown this. I could not find a single study using the WRF model that reports any systematic changes in precipitation; two report case studies, one with hurricanes.

3. The authors respond that their results are not significantly affected by spatial or vertical resolution. However, the figures they included in their response to the review are for 10-m wind speed, not precipitation. Thus, this question remains unanswered. Additionally, there is no mention of the evaluation of the precipitation simulated by this model over, for example, Denmark.

4. The authors write in the response to Reviewer # 1: “The simulations are not deterministic”. But this statement is incorrect. A good example of this is provided in the work of Lorenz et al (2016). The introduction of wind farms into numerical simulations is not unlike simulating the effects of tropical deforestation: surface properties change, and we use the model to study how these changes propagate to other atmospheric phenomena. As demonstrated by Lorenz’s study, changes in a single grid point can propagate throughout the domain in a non-physical manner. Thus, testing the significance of the results against the background variability is a must. Additionally, atmospheric phenomena are heavily autocorrelated; thus, significance testing is not as straightforward as the authors make it sound. See, e.g., the textbook by Wilks (2019), Chapter 5.

5. The authors added the installed capacity density to Table 1. However, installed capacity density is a measure of the scenario (i.e., how many turbines are in an area based on their capacity), not the turbine itself. Additionally, the installed capacity for each country should be reported in the manuscript so it can be compared with public scenarios from the Danish Energy Agency and similar authorities in other countries. This is the value that energy planners rely on for their future planning.

Abstract and Plain text summary

The abstract does not include the caveats of their study, including that the study used a regional single model and unrealistically high installed capacity densities.

References

The manuscript is very heavily self-cited. Of the about 15 references about wind farm wakes, 8 are from the author’s research groups. Work from several other important groups in Europe (e.g., Fraunhofer IWES in DE, DTU Wind and DMI in DK, KNMI in NL) and the USA (e.g., Cornell Univ, NREL, NCAR) is not cited at all.

References:

Pryor, S., Barthelmie, R., & Shepherd, T. (2018). The Influence of Real?World Wind Turbine Deployments on Local to Mesoscale Climate. Journal of Geophysical Research

Atmospheres, 123(11), 5804-5826. https://doi.org/10.1029/2017jd028114

Fischereit, J., Brown, R., Lars□n, X., Badger, J., & Hawkes, G. (2021). Review of Mesoscale Wind-Farm Parametrizations and Their Applications. Boundary-Layer Meteorology, 182(2), 175-224. https://doi.org/10.1007/s10546-021-00652-y

Wilks, 2019: Statistical Methods in the Atmospheric Sciences.

Lorenz, R., A. J. Pitman & S. A. Sisson (2016), Does Amazonian deforestation cause global e?ects; can we be sure?, J. Geophys. Res. Atmos., 121, 5567□5584, doi:10.1002/2015JD024357.