Letter to the Editor 29.07.2024

Submission of the revised manuscript

Dear Editor,

We would like to thank you for the opportunity to revise the manuscript based on the reviewer's additional comments. We would also like to extend our gratitude to the reviewer's continued support and in-depth suggestions aimed at refining the manuscript's key message.

In response, we have added additional analysis and figures to the manuscript to address the reviewer's concern about the extrapolation of original 4.5 month comparison to climatological timescales. The manuscript text has been revised to clarify that the comparison with Miller et al 2015 simulations spans 4.5 months. Then, we used 100m ERA5 wind speed data from 2000 - 2020 to extrapolate the impact of Kinetic Energy (KE) removal to longer timescales. The results from this analysis have been included in the comparison between our estimates and those previously published by Lopez et al (2012) and Brown et al (2016) (Table 4); in the comparison of our estimates with results from other numerical modelling studies (Figure 5); and in the estimation of LCOE impacts (Figure 6). We find that the ERA-5 based KEBA estimates and their impacts are consistent with the WRF based estimates from Miller et al. 2015 and the previously published literature. The text of the manuscript has been also edited accordingly to ensure a coherent and logical structure.

Next, we acknowledge that KE removal effects depend on wind directions but show that the impacts of wind direction in our study are anticipated to be small. This is because almost 70% of the near - hub height wind speeds blow from the South (Appendix figure D1). This suggests that most of the KE is transported from the South and therefore so are KE removal impacts. Since the deployment that we evaluate is oriented along this axis it can be assumed that the impact in the predominant wind direction is accounted for. This expectation is consistent with other regional wind energy resource assessments in areas with a dominant wind direction which showed that KEBA estimates and their agreement with WRF were largely unaffected by wind direction. This may imply that the impact of wind direction in areas with a highly dominant wind direction maybe limited. However this needs to be analysed in more detail and could be the subject of future evaluations.

Lastly, we have refined the text to temper the message around the policy relevance of our analysis. In line with the reviewer's comments, the reference to policy applications in the title has been dropped. Additionally we have specified in the text that the technical potential estimates in the text are not meant for direct use in policy design applications. However, we do maintain the that the insights from our are relevant for energy policy design. This is because atmospheric response impacts in LCOE terms are non - trivial even for the lowest installed capacity density scenarios (Figure 6). Given scale of these impacts and the fact that they are not yet incorporated into regional wind resource analysis means that our message about policy relevance remains considerably relevant. Further more we test an approach which presents a viable option for practically incorporating these impacts into regional wind resource assessments within the constraints of energy system modelling. Thus, we have edited the text so as to mitigate over-interpretation of the result while maintaining the significance of analysis to policy applications.

We believe that we have adequately addressed the additional concerns raised by reviewers and hope that the manuscript is acceptable for publication. As the corresponding author, I confirm that the manuscript has been read and approved by all the co-authors.

Thank you for your consideration.
Sincerely

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Institute of Physics and Meteorology | Biospheric Theory and Modelling

Point by point response to reviewer

We are grateful to the reviewer for the detailed comments that aid us in further refining the manuscript's message. We have included additional analysis and further revised the manuscript to address the reviewer's comments. The details of the actions taken are specified below with the respective comment. The original reviewer comments are in black while the responses/actions are in blue.

"I read the response to the reviewers and am partly satisfied. The authors improved their Figures and I still agree that the general message is interesting (wind resource depletion matters when wind parks become huge) and this paper is new in the sense that it applies and compares an existing framework to a new location. However, I am still concerned that the authors are over-confident in their results and generalize in unjustified ways. I believe that the authors need to either improve their approach or tone down the real-world relevance of their findings."

Response:
We thank the reviewer for the comments. To address them we have included additional analysis to further substantiate our insights and ensure that our conclusions are justified (Details below). The text has been updated accordingly to ensure that the manuscript maintains a coherent and logical structure. Further, we have also edited the text to ensure that our intention to state the policy relevance of our results does not lead to unjustified generalisation. Lastly, as suggested by the reviewer, the complete manuscript has been reviewed by the senior co-author.

"In the reponse to the reviewers, the authors state that the manuscript "is mainly concerned with the approach used in energy scenario analyses to estimate technical wind energy potential". But the title still suggests that the paper produces insights for policy even though it is highly simplified and therefore comes with substantial uncertainties. To avoid the impression that reported numbers can be directly used for policy, I suggest that the authors rephrase their title (e.g., "A simple estimate of the technical wind energy potential of Kansas that incorporates the atmospheric response for energy system simulations with very high wind shares")."

Response:
We have clarified in the text that our manuscript deals mainly with the approach to technical potentials to avoid the impression that the numbers evaluated in the study are policy ready.

Action:

1. The title has been updated to " Estimating the technical wind energy potential of Kansas that incorporates the effect of regional wind resource depletion by wind turbines".

"I am not convinced that 3.5 months of input data can provide sufficient evidence for the conclusions that the authors draw from their analysis. Weather varies on the seasonal scale and beyond which will strongly impact the results. If the authors do not extend their analysis to capture a longer analysis period, this short period must be mroe prominently flagged in abstract and conclusions. The reasonsing provided by the authors (ENSO about neutral, GP LLJ and soil moisture average) provide an explanation why one might want to choose those particular 3.5 months over another 3.5 month window. But since the authors draw generaly conclusions about technical potentials from their modeling, they also have to argue why the 3.5 months are representative for the conditions over a few decades."

Response:
This is a valid point and the text has been revised to state that comparison with WRF is over 4.5 months (15 May to 30 September 2001). Additionally, to show that the role of KE removal remains relevant despite large variations in wind speeds over decadal timescales, we have extended the analysis using 100m wind speed product over Kansas from ERA5 for the 2000 to 2020 period (See below - Figures 1 and 2). Using this, we estimate KEBA and Standard capacity factors for the 2.5 and 5 MW km^{-2} deployment scenarios. This comparison has been included in Table 4. The difference between the ERA5 based Standard and KEBA technical potential estimates is similar to that between Standard and WRF estimates over the original 4.5 month period. WRF estimates of technical potential are ~ 53% and 33% lower than the Standard for the 5 and 2.5 MW km^{-2} cases, respectively. The ERA5 based 20 - year KEBA estimates are similarly lower by ~59% and 41%

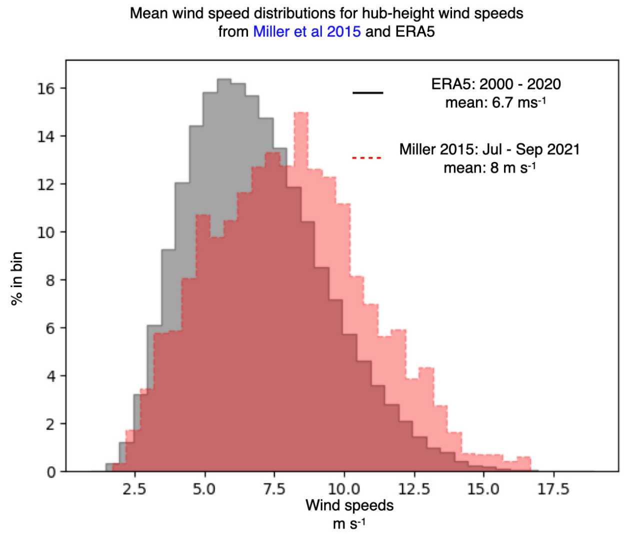

Figure 1

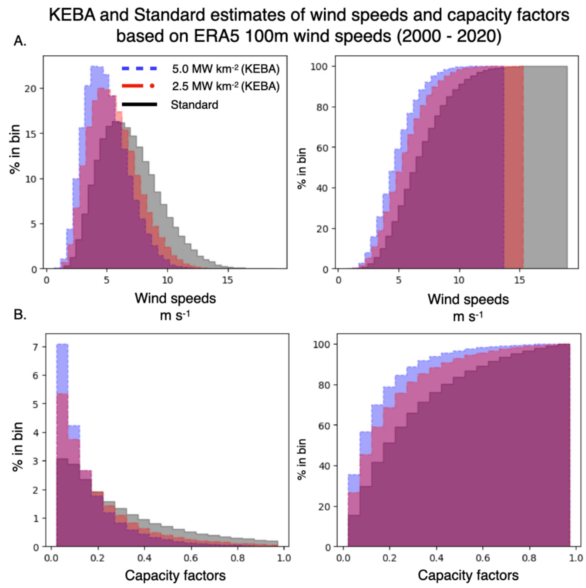

Figure 2

than the Standard estimate. The differences are similar because the impacts of KE removal scale with wind speeds. Wind turbine extract more energy when wind speeds are higher leading to a concomitant reduction in efficiency and vice versa. This is shown in Figure 3 (manuscript) where the reduction in capacity factors is higher at night than during the daytime. The ERA5 based estimates also remain consistent with other published simulation estimates (manuscript Fig. 5) and project similar impact on LCOE as the shorter duration WRF based evaluation (manuscript Fig 6). This supports the generalised insight from our analysis that KE removal effects are non-trivial over longer timescales and must be included in the technical potential estimation.

Action:

1. Calculated Standard and KEBA estimates with ERA5 100m wind speed data for 2000 - 2020. Figures 1 and 2, shown above, added to appendix.
2. Added ERA5 based standard and KEBA estimates to Figures 5 and 6.
3. Table 4 updated to include ERA5 based, model based and published estimates.
4. Updated abstract to specify that the WRF model based analysis and comparison covers 4.5 months (15 May to 30 September 2001) and highlight the extended analysis performed using the ERA5 data.

Authors argue that “Since the deployment scale is large, it can be assumed that most of the turbines will be affected by wind speed reductions regardless of the direction (Antonini & Caldera 2021).” But KEBA assumes a certain extent of the wind park along and accross flow direction. Wind direction therefore always matters unless your wind parks are infinetely large, even in the simplified logic of your approach. If wind direction changes by 45°, your wind park length becomes longer at the center and shorter at the edges…

Response:
This is a valid point. The changing directions will result in a variation in the the deployment geometry presented to the wind farms. This means the choice of length and breadth used in KEBA does vary with the wind directions. However, near hub - height winds in Kansas blow from the South about 70% of the time (see below - Fig. 3). This means that the bulk of the kinetic energy is transported from the South to the deployment. Therefore, we do not employ direction-dependent values of wind turbine deployment geometry (width and length) for the KEBA model setup. This may not be the case for other geographical regions and we recommend that KEBA implementation in other locations account for the direction dependency of the wind turbine deployment geometry.

Action:

1. Included Figure 3 into appendix.
2. Text updated to included additional clarification regarding the impacts of wind directions.

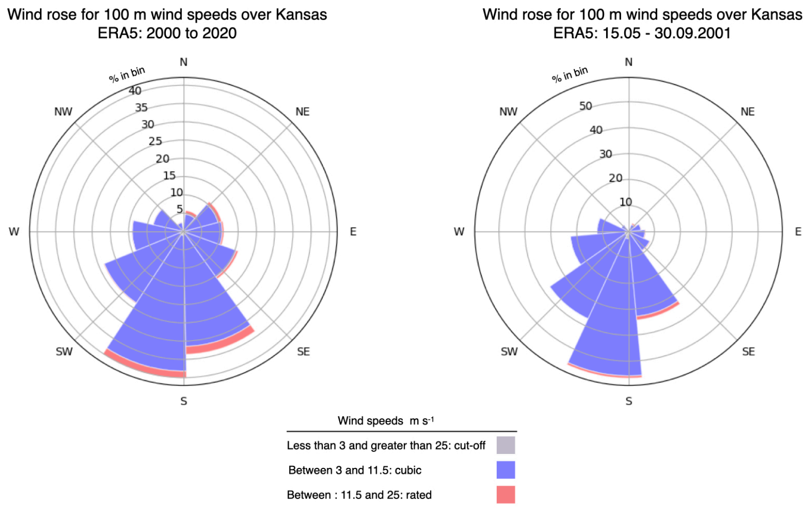

Figure 3

In the response, you argue that “KEBA makes no assumptions about the vertical variations in wind speeds”. The opposite is true. KEBA makes one of the strongest possible assumptions about vertical wind variations by assuming that horizontal winds are independent of height in the boundary layer.

Response:
The KEBA approach uses only the hub-height wind speed as input. These wind speeds are sourced either from atmospheric models, reanalysis datasets or observations. In our case, WRF model winds speed at 84 m or 100m wind speeds from ERA5 have been used. Therefore, the vertical variation of wind speeds would be implicitly imprinted in the value that we use to initiate the model. KEBA, itself, does not specify any constraints on wind speeds at other heights.

Action:
None

I would recommend that the senior author has a look at the revised version of the abstract. I appreciate that you shortened it but it would still benefit from some brushing (e.g., “fixes losses to 10%” --> who does that? also needlessly detailed. “reduces wind speeds or depletes wind resources”? --> “or” or “and”? etc.).

Response:
The abstract has been updated and refined further with inputs from the senior author. Fixed losses of 10 % (array losses) are, however, typical for technical wind energy potential estimation. All the studies which employ the Standard approach to estimate technical potential, which we refer to in the manuscript, use a fixed 10% array loss. This also remains the case for more recent evaluations as well (O’Callaghan et al 2023, Jung et al. 2022)

Action:

1. Removed the sentence “Fixed losses fixed to 10%” in the abstract.

References

1. O’Callaghan, B., Hu, E., Israel, J., Llewellyn Smith, C., Way, R. and Hepburn, C. (2023). Could Britain’s energy demand be met entirely by wind and solar? Smith School of

Enterprise and the Environment, Working Paper 23-02.

2. Jung, C., Schindler, D. Development of onshore wind turbine fleet counteracts climate change-induced reduction in global capacity factor. Nat Energy 7, 608–619 (2022). https://doi.org/10.1038/s41560-022-01056-z