Response to the reviewers

Extrapolation is not enough: Impacts of extreme land-use change on wind profiles and wind energy according to regional climate models

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Under review for ESD

Status: Publish subject to minor revisions after first round of revisions

We would like to thank both reviewers for their clear assessments of our submission. We are glad that reviewer #1 is satisfied with our first round of revisions and that they now recommend *publish as is*. We understand that reviewer #2 has not taken part in the first round of revisions and that they have additional good comments which we address below.

Throughout this document, we use *italics* to mark the reviewer comments, ~~strikethrough red~~ to mark text that we deleted from the manuscript and blue to mark text additions to the manuscript. When reporting line numbers, we refer to the clean manuscript in the first round of revisions (i.e., the most recent version before this revision).

Report #1

Wohland et al's efforts to fix various parts of this study are done to my satisfaction. I also appreciate they utilized this chance to expand upon a few technical steps as well as highlight why the research is interesting. Thank you.

Author response

Thanks for taking the time to evaluate our first response. We are glad that you consider our modifications to be satisfactory.

Report #2

The study presents the effect of extreme land use change on wind energy sources. Specifically, they evaluated the affect of afforestation/deforestation on wind energy. It uses model outputs from REMO-iMOVE run by GERICS and WRFaNoahMP run by IDL, which are products of LUCAS consortium. The authors have computed mean wind speeds for a time period of 1986 to end of 2015. It considers changes in annual mean as well as seasonal and daily cycle.

There are two major purposes of this study. First one is to evaluate the methodologies that are used for evaluating hub-height wind speeds in the field of wind energy: a) the conventional one that extrapolates using power law log-law from surface winds, or b) interpolating winds from model outputs, such as WRF and REMO-iMOVE. Second is to evaluate a possible effect of afforestation on future wind speed projection and hence, wind power. The results from the study can be helpful in better management and forecasting of wind energy resources in future.

The study is mostly well presented and thought. The authors have compared a conventional method of extrapolating wind speeds, with the output from dynamical models. They have found limitations in it from the perspective of climate change (afforestation).

Author response

We are glad that reviewer 2 considers our evaluation to be well presented and thoughtful.

Comment 2-1

I have a few questions from the author: I understand that the power law used here in this study are valid for a neutral atmospheric condition. In that case will it be more relevant to find only the instances from the model output where the atmosphere is neutral for an apple-to-apple comparison. Kindly address what can be the affect of atmospheric stability in using these techniques in section 3.1. Please check for log law as well.

Author response

Thanks for your comment. Yes, we agree that the power law can be a useful heuristic under certain atmospheric conditions. And we also acknowledge that the log law can even be formally derived from the atmospheric equations of motion as a special case (l. 156: "The log law can be formally derived as a special case of the general equations of motion under neutral stratification, while the power law is empirically motivated (e.g., Emeis, 2013)".

Our goal is not so much to verify that the power law is valid when it should be (i.e. in the special case of a neutral atmosphere), but instead to show that the typical use of the power and log laws to extrapolate future climate model output is inappropriate. Indeed, even though those heuristics are sometimes useful, they are used under all atmospheric conditions in the published literature. Since we are interested in contrasting explicit modeling with the state-of-the-art in climate-model based wind energy assessments, the proper apple-to-apple comparison to compare to the extrapolations applied at all times. Below you find a list of examples that documents that log or power laws with constant (!) coefficients are typically used to extrapolate near-surface winds to hub height. The list is copied from our first response to reviewers, assuming that you did not have access to it. Please apologize for the duplicate should you have had access to it already:

1. "**Hueging et al., 2013** : They use 2 regional climate models to analyze climate change impacts on wind energy in the 21st century. They use the power law with "power-law exponents a of 0.2 for onshore areas (IEC 2005a) and of 0.14 for offshore sites (IEC 2005b)" to extrapolate from 10m to hub height. That is, they use the same wind profile in the future.
2. **Tobin et al., 2016** : They use EURO-CORDEX regional climate models to analyze climate change impacts on wind energy in the 21st century. They extrapolate 10m wind speeds to hub height using the power law with a fixed coefficient of 1/7 (see their supplementary material, page 10). That is, they use the same wind profile in the future.
3. **Reyers et al., 2016** : They use CMIP5 simulations and statistical-dynamical downscaling to analyze climate change impacts on wind energy in the 21st century. They use the power law to extrapolate from 10m to 80m with a constant power law coefficient with the same values as in Hueging et al., 2013 (see Reyers

et al., 2015 for details about the method). That is, they use the same wind profile in the future.

4. **Karnauskas et al., 2018** : They use CMIP5 simulations to evaluate global wind energy potential and how it develops under climate change. Their method: "The 10-m wind speed fields are extrapolated to 100 m using a power law with coefficient 1/7". That is, they use the same wind profile in the future.
5. **Schlott et al., 2018**; They use EURO-CORDEX simulations together with PyPSA power system modeling to quantify the effect of climate change on the European power system. They use the log law with a roughness length "which is provided by the datasets as a static quantity" and a displacement height of zero to extrapolate from 10m to 90m hub height. That is, they use the same wind profile in the future.
6. **Soares et al., 2019**: They use regional climate model simulations (largely from CORDEX Africa) to evaluate the effect of climate change on wind energy resources in Northwestern Africa. For the CORDEX-Africa simulations which only provide 10m winds, they use the power law to extrapolate from 10m to 100m and 250m. That is, they use the same wind profile in the future (at least for parts of the analysis).
7. **Lima et al., 2021**: They use the same approach as in Soares et al., 2019 to study the present and future wind resource in South-Western Africa. That is, they use the same wind profile in the future (at least for parts of the analysis).
8. **Wohland et al., 2021**: They use EURO-CORDEX to study the effect of climate change on wind energy complementarity in Europe. They use the power law with a fixed coefficient of 1/7 to extrapolate from 10m to 80m hub height. That is, they use the same wind profile in the future.
9. **Bloomfield et al**., **2020:** They use reanalysis and selected EURO-CORDEX simulations (i.e., the ECEM dataset) to quantify the effect of climate change on different types of renewable generation, including wind energy. Their assessment is based on 10m winds and they extrapolate to 100m using the power law with a fixed 1/7 exponent. That is, they use the same wind profile in the future.

All studies listed above use the same wind profile in the future even though land-use is poised to change in the analyzed scenarios. We hope that this list helps to clarify the context of our paper and how we aim to contribute to better climate-model based wind energy assessments with it." (Copied from first Author response).

Comment 2-2

Title of the paper is quite strong: "Extrapolation is not enough". My question is: Is there any possibility that only extrapolation, may be along with calibration based on different scenarios can work?" I also request authors to kindly consider rephrasing the title.

Author response

Thanks for this comment. As we show throughout the paper, wind profiles vary, for example, with time of day, season, and lower boundary forcing. As you rightly point out in your first comment, these variations are partly related to atmospheric stability, and hence physically expected.

The fundamental problem with the log and power law (see eq. 2 & 3 in the manuscript) is that the profiles themselves are not temporally dependent (see lines 137 to 142 in manuscript) and the parameters *alpha* and *z0* and *h* are typically not even changed in climate projections with strong land-use change. That means that the log and power law are unable to reproduce the variations with time of day, season and lower boundary forcing that we report based on model-level output. So, to answer your question: it is not possible that extrapolation can "work" as long as it doesn't explicitly include the spatio-temporal complexity of wind profile changes. We therefore decided to keep the title as is after considering your suggestion.

Please note, however, that our suggested method (i.e., interpolation between model levels) takes the form of a power law with the essential modification that *alpha* is not longer a single number but a function of time and space, also varying per scenario. To compute this version of *alpha,* one needs knowledge about wind speed above and below the height of interest. We do not think that it is justified to label this version of the power law as an extrapolation because it is an interpolation.

Comment 2-3

Line 268, Section 3.2: Why in "summer regions there is a drop and then in winters there is an increase in wind profile change?"

Author response

Unfortunately, we could not find the sentence that you quote above in our manuscript.

The paragraph around line 268 in the clean version of the manuscript discusses Fig. 4 and states that "surface changes are very high in winter (ca. 3m/s), high in the annual mean and spring (ca. 2 m/s), medium in fall (ca. 1.75 m/s), and comparably low in summer (ca. 1.2m/s). By contrast, wind speed change is approximately 0.8 m/s at the highest displayed level (around 750m) in all seasons."

In the tracked changes version of the manuscript, we write in line 268: "The models also agree that wind speed changes decay monotonically with height although exceptions exist during the summer, where IDL projects a local minimum followed by increased change (Fig. 4b,c)."

Maybe you could clarify what your question exactly refers to and whether the text in quotation marks ("") is an actual quote or something else? Thanks.

Comment 2-4

Section 3.3: L289 The surface perturbation decays slowly around noon and decays quicker in night. There is more mixing in atmosphere around noon, yet the perturbation because of introduction of a forest decays slowly. I request authors to consider adding *justification.*

Author response

Thanks for this comment. If we understand correctly, you are challenging the results presented in Fig. 5 (2nd and 4th column) because you expect wind profile changes to decay more quickly around noon because there is more mixing. Actually, the opposite is true: more mixing means that the surface change also manifests further aloft (because the near-surface wind speed reduction is not constrained to the surface but “spreads” to upper levels via mixing).

Let’s take April as an example. Panels m and o in Fig. 5 show that low wind speed values do not only occur near the surface but also aloft in the GRASS simulations, and this effect is most pronounced at 12h. In other words, there is indeed more mixing at 12h in the GRASS simulations, as expected since downwelling solar radiation peaks around noon. At the same time, panels f and h show that the GRASS-FOREST difference has the largest vertical extent at noon as well. That is, the reduction near the surface also manifests at the models levels further aloft. Or in other words:

> “In particular, we find that the surface perturbation decays slowly around noon and decays relatively quickly at night during all months in IDL (Fig. 5, panels d, h, l, and p) and during all months but October in GERICS (Fig. 5, panels b, f, j, and n).” (Quotation from the manuscript, l. 275)

We hope that these explanations helped to resolve the confusion. We decided to add the following short explanation to the manuscript to avoid similar confusion among the readers.

Changes to the manuscript

l. 279:

> during all months but October in GERICS (Fig. 5, panels b, f, j, and n). This daily cycle is consistent with physical expectation: increased mixing around noon implies that the near-surface wind reductions impact higher level winds more strongly. Similarly, more stable nighttime conditions imply that the near-surface wind reductions are more constrained to the surface. While models…

Comment 2-5

Please recheck if all the supplementary figures are discussed in the main paper

Author response

Thanks for spotting this one. There was indeed a problem that was caused by an earlier reformatting of the SI which meant that SI Figures S2 – S9 were not properly referenced. We changed the text to fix this issue, see below. We made sure that all SI Figures are now referenced in the manuscript.

Changes to the manuscript

> To test the robustness of change in different parts of the year, we analyzed the changes per season (see Supplementary ~~section 4~~ Figs. S2 – S9).

Comment 2-6

Why are there two peaks in Figure 8 IDL CF?

Author response

We are not aware of any a priori reason to expect one, two, or more peaks in the distribution of capacity factors. The bimodal shape of the IDL distributions implies that CF of around 0.4 or around 0.55 in the GRASS simulation occur more often than other values. By contrast, the GERICS distributions feature a single peak. Because of this finding, and a few others, we flag repeatedly that there is considerable model uncertainty, for example:

- l. 381: "Since we also report substantial model uncertainty, the exact values provided here should be treated with caution"
- l. 264 "In other words, model uncertainty is high at individual locations."
- l. 212 "Additionally, disagreement can stem from the allocation of land surface surface parameters (e.g., roughness length and leaf area index) and whether and how those parameters evolve throughout the year."

Since other readers might also wonder why the distributions are qualitatively different, we add a brief discussion (see below). Thanks.

Changes to the manuscript

l.352:

> Moreover, while GERICS features a single distinct peak in both experiments, IDL is weakly bimodal. The single peak in the GERICS GRASS distribution means that values around 0.3 occur most frequently. By contrast, the bimodal shape of the IDL distributions implies that CF of around 0.4 or around 0.55 in the GRASS simulation occur more often than other values. While we are not aware of any a priori reason to expect one, two, or more peaks in the distribution of capacity factors, this qualitative difference in shape is another example of model uncertainty.