Final response Reviewer 1 07.04.2024

We would like to thank you for taking the time for a thorough review and detailed comments on our manuscript. Below we provide a point-by-point response to your comments and the action taken to address them, where necessary. The responses and actions are in red text while the original reviews are in black. Line numbers under the “Action category” indicate updates in the revised manuscript.

Review of "Estimating the technical wind energy potential of Kansas that incorporates the atmospheric response for policy applications" by Jonathan Minz et al. under consideration for Wind Energy Science
The authors investigate technical wind energy potentials under different wind park scenarios. They contrast the "standard" approach, which ignores depletion of atmospheric kinetic energy by wind parks, with explicit WRF modeling presented in a different study and a physics-based simple model called KEBA. The study focuses on Kansas. The authors find that the standard approach is not justified when huge wind parks are build and they argue that KEBA is a computationally tractable alternative to running highly resolved fluid dynamical simulations.

Overall, the study appears as a fine model intercomparison study. However, since the authors repeatedly stress the policy relevance of their work, a more balanced perspective is needed to contextualize the results. This is because the suggested wind park cluster in Kansas is huge, even exceeding the current global installed wind park capacity by about 30%.

Moreover, according to the SI of the manuscript, this paper has been submitted to Environmental Research Letters in 2021 (see KK2021_Readme.pdf available at https://edmond.mpg.de/dataset.xhtml?persistentId=doi:10.17617/3.78). I do not think that prior rejection elsewhere necessarily implies that the paper is not worthy of publication. However, I ask for an explanation of how the current version of the manuscript relates to the older one and how the earlier reviewer comments have been taken into account.

Responses to the points about the size of deployments being evaluated and, the ERL rejection and subsequent updates are detailed within the Major comments sections below.

I provide a list of additional major and minor comments below.

Major

1. According to the Global Wind Energy Report 2023 (https://gwec.net/globalwindreport2023/), a total of 900 GW is currently installed on the entire planet. According to your Table 2, the studied wind park has an area of 112 000 km2. Using the upper end of the capacity density range (10 MW / km2), you suggest to install >1.1 TW in Kansas alone. That is, you suggest to install more wind turbines in a single US state than we currently have on the whole planet. Given how extreme this scenarios is, I am surprised that you do not discuss this at all.

1. Response:

Our manuscript is mainly concerned with the approach used in energy scenario analyses to estimate technical wind energy potential. These estimates represent the maximum amount of energy that could be technically generated through the deployment of wind turbines over all the area that is actually available at a regional, national or global scales. Thus, to calculate it one must assume that all the land available for wind energy development is covered with wind turbines. The maximum can then be estimated by increasing the number of turbines deployed.

Within this context, the range of scenarios (35 GW, 70 GW, 140 GW, 280 GW, 560 GW, ~1.1 TW) and the deployment area (112, 000 km^2) that we evaluate are consistent with existing analyses that estimate the technical potential of Kansas. Lopez et al 2012 and Brown et al 2016, with whom we compare our results, evaluate an installed capacity of ~1 TW and ~0.5 TW and deployment areas of ~190,000 km^2 and ~157,000 km^2 in Kansas, respectively. Elsewhere, Enevoldsen et al 2019 have estimated the technical potential for onshore Europe assuming a deployment of ~52 TW over ~4,900,000 km^2 (40 % of European land area). We show that the kinetic energy removal effects become relevant (Fig. 5) from the 140 GW or 1.25 MW km^{-2} case. Ignoring these effects (Standard approach), lead to an overestimation of technical potential by 20 - 30% relative to KEBA/WRF.

Therefore, we emphasise that our analysis does not suggest the installation of 1.1 TW over Kansas, which is an extreme scenario, but makes the point that effects of kinetic energy (KE) removal cannot be discounted while making estimates of technical wind energy potential at the deployment scales that are typical in energy scenario analyses.

We will include specific text which highlights that the larger scenarios evaluated in the study can be considered extreme for Kansas. This will be in addition to the text in the manuscript which highlights the regional scale focus of our analysis. For example, lines 23 - 33 provide the context for the application and calculation of technical potential. Lines 34 - 39 highlight the difference between technical potential estimation and resource estimation for wind park development, and that our manuscript focuses on the former.

However,

1. Action:

- Added lines 28 - 33: It should be noted that the capacity deployments assumed here for estimating technical potentials can range from realistic to extreme i.e. from a few tens of Giga-Watts (GW) to a couple of Tera-Watts (TW). . These are reasonable assumptions since the aim is to quantify the maximum technically feasible future generation derived by covering the whole area available for generation with wind turbines (Adams and Keith, 2013; Jacobson and Archer, 2012; Eurek et al., 2017; Enevoldsen et al., 2019; Lopez et al., 2012; Hoogwijk et al., 2004; Brown et al., 2016; Volker et al., 2017; Lütkehus et al., 2013).
- Added lines 157 - 159 : It should be noted that the capacity deployments assumed here for estimating technical potentials can range from realistic to extreme i.e. from a few tens of Giga-Watts (GW) to a couple of Tera-Watts (TW).

2. Your calculation of LCOEs suggests direct real-world relevance. However, I am sceptical whether the results can be used in the real world because the scenarios are very extreme and the highly simplified (one small turbine only, a single massive park instead of multiple ones that allow for flow recovery, single hub height, rectangular shape, no directional dependence). Please provide strong justification or consider removing. I think that the paper would benefit from being framed as a model comparison without any immediate policy relevance other than "if you build very huge wind parks, think about modelling wind resource depletion".

2. Response:

The purpose of our Levelised cost of energy (LCOE) calculation is to highlight the conceptual link between the KE removal effect, installed capacity density, capacity factors (CF) and LCOE within the context of regional wind energy resource potential (Fig. 6). It serves to underscore the relevance of KE removal effects for energy scenario analysis and modelling by quantifying its relative impact on LCOE. Despite its simplified nature, the relationships between Technical Potential, CF and LCOE highlighted in Fig 6 are likely to be relevant for realistic scenarios. Therefore, we would like to keep Fig. 6 in the manuscript based on the following reasons.

1. Fig.6 provides a strong motivation for the impacts of atmospheric response to be included in energy scenario analyses because it conceptually links atmospheric response effects to CF and LCOE. Of all input variables, LCOE is most sensitive to changes in CF (Cory and Schwabe 2009). Since the figure shows that atmospheric response strongly shapes the CF, it encourages a detailed techno - economic evaluation of regional wind energy resource that includes the impact of atmospheric response to large scale KE removal.
2. The impact of the atmospheric response effects are not yet incorporated into energy scenario analyses. In fact it is usually assumed that improvements in turbine technology will lead to higher CF's in the future (Wiser et al 2016, IRENA 2019). However, as Fig. 6 shows the impacts of the atmospheric response tend to suppress CF. As a result, energy scenario analyses need to evaluate the positive impact of improving turbine technology within the context of atmospheric response impacts on CF. Resolving this juxtaposition is a critical component of making robust wind energy policies.
3. Fig 6. shows that energy scenarios need to balance the increase in generation from larger deployments against the erosion in CF due to atmospheric response. Installed capacity densities used in energy scenario analyses generally range from 3 to 11 MW km^{-2}. Since the

standard approach assumes that CF is independent of regional wind resource depletion, the primary constraint on technical potential is thought to be land availability. Therefore, technical potential can be maximised by increasing the installed capacity density. However, since the availability of KE within the boundary layer constrains generation from a regional deployment, it means that, beyond a certain installed capacity density additional generation results in lower CF. Therefore, the relationship of installed capacity density with technical potential and CF can be used as an additional constraint on installed capacity densities. In discounting the atmospheric constraint on technical wind energy potential, energy scenarios overestimate technical potential and underestimate the cost. Therefore, Fig 6. provides a simple conceptual framework of the physical constraints on technical potential that need to be incorporated in energy scenario analyses.

We will include the justification for the LCOE calculation in the Discussion section. This will be in addition to the description of the aim, the underlying assumptions and the methodology of the LCOE calculation already included in the manuscript (3.8 Implications for technical wind energy potential estimation lines : 356 - 376, and Appendix D2).

2. Action:

- Fig. 6 retained.
- Added justification and importance of Fig 6 (lines 411 - 427): Although the variation of technical potential, CF and LCOE with increasing capacity densities shown in Fig. 6 is idealised, the trend does have implications for realistic scenarios. The relationships in Fig.6 provide a conceptual frame work that quantitatively links the increase in generation from additional turbines with the degeneration of efficiency (CF) and cost (LCOE) arising from physical constraints imposed by the atmosphere. Despite its idealised nature, Fig 6 trends are consistent with real - world analyses that show that CF is the most important physical control on LCOE (Cory and Schwabe, 2009). Currently energy scenario analyses anticipate only an improvement in LCOE driven largely by improvements in CF due to better turbine technology(Wiser et al., 2016; Prakash et al., 2019; Blanco, 2009). Fig. 6 then motivates the evaluation of this expectation within the context of atmospheric limitations on KE availability for an improved estimate of LCOE. Further, the trade-off between increased technical potential and, CF and LCOE provides a strong physical constraint on installed capacity densities which, at present, range from 3 to 24 MW km^{-2} thought mainly to be constrained by land availability (Hoogwijk et al., 2004; Lopez et al., 2012; Brown et al., 2016; Eurek et al., 2017; Enevoldsen et al., 2019; Lütkehus et al., 2013). The physical constraint indicates that there is a likely region specific optimum installed capacity density which balances technical potential, CF and LCOE. Thus, even though Fig 6 represents idealised relationships, it still provides a physically consistent conceptual framework that encapsulates the non-trivial impacts of the atmospheric response to large scale wind energy generation for application in energy scenario analyses. As we have shown, these impacts can be incorporated in energy scenario analyses almost completely by accounting for the KE removal effect."

3. Lee Miller is listed as a co-author in the SI. Why is he not on the author list? What happened with the ERL submission? What has changed since then?

3. Response:

Dr. Miller requested to be removed from the manuscript prior to our revised submission to Wind Energy Science. He was unable to continue as a co-author on account of ongoing professional commitments. He has, however, provided active support and strong insights in the the preparation and finalisation of the manuscript and is duly credited in the acknowledgements section.

3. Action:

- Dr. Miller's name will be removed as a co-author in the Supplementary Material. His affiliation will also be removed from the title.

3a. Response to the question "What was the issue with the ERL submission?":

The original submission to the ERL focused primarily on evaluating the extent to which the counter-intuitive simulation results from Miller et al 2015 could be explained just by accounting for the depletion of the KE budget of the boundary layer. This was accompanied by a short discussion of implication of the results for estimation of technical wind energy potential. The article was around 4500 words and included 4 figures.

Our submission was rejected on the basis of a reviewer's contention that the Standard approach, then referred to as the Common approach, remains valid since Jacobson et al 2012 and Marvel et al 2012 had highlighted that increasing turbine density leads to a linear generation phase followed by a saturation phase. Since our ERL submission did not include explicit references to these papers it was implied that the motivation for our analysis, that the standard approach overestimates technical wind energy potentials, was incorrect. Secondly, it was stated that errors highlighted by Archer et al. 2020 in the Fitch Wind Farm Parameterisation scheme meant that the Miller et al 2015 simulations needed to be rerun. Lastly, the reviewer stated that the hypothetical scenarios evaluated in our study and the simplified nature of the KEBA model meant that it and our results were of limited use in case of real wind farms.

3b. Response to the question "What has changed since then?":
We have addressed the specific criticisms raised in the ERL review to show that these were either incorrect or had no impact on the results in our analysis, and also added more context to aid the interpretation of our results. Below is a description of the major changes made to the manuscript.

1. To address the issue around Jacobson et al 2012 and Marvel et al 2012 we added Fig 5 to show that our results are consistent with these analyses. It includes estimates from our analysis and from other studies that have made similar evaluations at regional or global scale. It shows that estimates from our study are consistent with the broader literature. Although there is a somewhat linear trend in power generation with increasing capacity density, it is limited to deployment densities less than 2 MW km^{-2}. Analyses that estimate technical wind energy potential usually assume a deployment density of 3 MW km^{-2} or more (Lopez et al 2012*,* Brown et al 2016, Eurek et al 2017). Marvel et al 2012, estimate a maximum power generation of 429 TW from a uniform global surface level wind turbine deployment. This implies a technical potential of about 0.8 W m^{-2}, which is consistent with our evaluation and the relevant literature. Therefore, incorporating Jacobson et al 2012 and Marvel et al 2012 in the discussion strengthens the motivation for our analysis rather than diminishing it.
2. An explanation about why Miller et al's 2015 simulations were not affected by the bug identified by Archer et al 2020 was added to the Appendix. Fischerheit et al 2022 showed that the issue was only found to affect WRF versions after 3.5 and before 4.2.1 while Miller et al 2015 used version 3.3.1. Further an initial analysis by Larsen & Fischerheit 2021 showed that bug corrected and bug prone versions of WRF produced similar results. This means that Miller et al's simulation results are reliable and can be used for analysis.
3. It has been clarified that the KEBA approach and our results are not aimed at wind farm developers or individual wind park planning and design. It has been stated that the aim of our manuscript has been to test the extent to which KE removal effects can explain Miller et al's counter-intuitive simulations, and quantifying the role of KE removal in shaping technical wind energy potential (lines: 126 - 128). Further, we explicitly state that the analysis of real wind farms is not within the scope of our analysis and neither are our insights aimed at wind farm developers (lines 34 - 44). We highlight that because KEBA is simple to implement means that it is a physically representative alternative for estimating robust technical wind energy potentials for applications in energy scenario analysis (lines 383 - 389).
4. Fig. 6, which charts the relationship between installed capacity density and generation, CF and LCOE, was added to highlight the non-trivial impact of KE removal effects on CF and costs. A description of the calculation methodology and interpretation was also included in the appendix (D2) and discussion sections, respectively.
5. Fig. 2, the conceptual diagram which explains the differences in the boundary layer between day and night, was included to provide a conceptual explanation of Miller et al's (2015) counter-intuitive simulation results.

Overall, the size of the manuscript has been almost doubled in terms of the number of words and figures for a deeper explanation of the motivation and context surrounding the analysis, a clearer definition of the scope, and a better illustration of the non-trivial impacts of atmospheric response effects on generation, CF and LCOE. Special emphasis has been made on ensuring that the issues raised by the ERL review were adequately addressed.

3a. , 3b. Action:
None

4. Why is it justified to ignore wind direction and wind park orientation? Those have a strong effect on how important the impact of wakes are.

4. Response:
Our manuscript focuses on potential improvements to the approach for estimating technical wind energy potentials. Technical wind energy potentials by definition pertain to the aggregate generation from hypothetical regional scale deployments. 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). Although, it is true that the strength of these reductions and their impact varies by direction our interest lies only in the aggregate, deployment scale impact on generation and CF. Explicitly accounting for wind direction and wind park orientation in KEBA would be relevant, had our focus been to quantify the variation in impacts with wind direction rather than just the cumulative impacts.

Further, a study that evaluated the German Bight's regional wind energy resource potential showed that the difference between incorporating and discounting wind directions and deployment orientation on KEBA estimates of technical potential was relatively small (Agora Energiewende, 2020). In it two sets of KEBA estimates were compared with WRF. One set of KEBA estimates accounted for wind directions and wind park orientation, and the other did not. It was found that the two sets of KEBA estimates were similar in their respective estimates. Put another way, the incorporation of additional details did not lead to a large impact on KEBA's estimates. It should be kept in mind that this is only applicable to regional analyses that focus on aggregate, deployment level impacts.

4. Action:
None

5. l. 146: "using atmospheric conditions from May 15 to September 30, 2001. This period is considered to be climatologically representative for this region (Trier et al., 2010)" --> This is a very strong statement and I doubt that it is correct. You are saying that 4.5 months are representative for average wind conditions over 20-30 years (which is the timespan that is normally used to define climatologies). Please provide quantitative evidence as such a limited input sample might severely impact the validity of your results.

5. Response:
This statement needs to be amended as the intent is not to suggest that the 2001 summer season (May 15 to September 30) is representative of the long - term climatology of Kansas. Rather, the point is to state that the simulated time period, itself, represents a typical summer over Kansas. This is because during this period large scale meteorological features, that are typically observed over the broader continental United States were found to be at their average locations and strengths. This was highlighted by the presence of near-neutral El Niño southern oscillation phase, a typical Great Plains low-level jet, and an average summer soil moisture content (Miller et al 2015). Therefore, Miller et al 2015 simulate a typical summer season over Kansas.

Since our aim is to evaluate the impact of atmospheric response on generation from a range of hypothetical wind turbine deployments, the most important variable for our analysis are the simulated wind speeds. Miller at al 2015, show that their model estimate adequately captures the observed horizontal and vertical variation during the typical summer season over the region of interest. This means that the analysis was conducted during a typical summer period over Kansas using WRF model outputs that adequately captured the variations in the wind speeds.

Thus, the highlighted statement is a miscommunication and will be revised accordingly.

5. Action:

- Removed:“This period is considered to be climatologically representative for this region (Trier et al., 2010)”
- Updated lines 150 - 153: The time period is representative of the typical summer season over Kansas typified by a near-neutral El Niño southern oscillation (ENSO) phase and an average Great Plains low-level jet and summer soil moisture content(Miller et al.,2015). The WRF model adequately captures the horizontal and vertical variations in wind speeds over this period(Miller et al. 2015).

6. In Fig. 3 how is it possible that the wind speeds and the capacity factors both decline linearly with W/m2 (which is installed capacity I believe...)? Are you sure that you are using the same x-axis for both? Since the relationship between them is non-linear, I don't see how both can be linear. Also this Figure is a good example that you need clearer axis labels.

6. Response:

In Figure 3b and 3d the reduced or effective mean wind speeds and mean capacity factors are plotted against the deployment’s generation, not the installed capacity density . This means the plots 3b and 3d represent the changes in mean wind speeds and capacity factors with KE extracted by the turbines. This means that, in 3b, the slope is given by the ratio of the change in effective wind speeds and the deployment’s yield $(m \cdot s^{-1})\ (W \cdot m^{-2})^{-1}$ while in 3d it is the ratio of the change in capacity factors and the deployment’s yields $(W \cdot m^{-2})^{-1}$. The relationship between wind speeds and the KE extracted is not linear but the relationship between capacity factors and KE extracted is linear.

We utilise linear fits in both cases as our interest is mainly to emphasise first order effects. The linear fit makes it easier to highlight the key first order effects i.e that reductions wind speed and capacity factors scale with the amount of KE extracted and that reductions are steeper at night than during the daytime. Thus, the choice to use linear fits is made mainly to emphasise key first order effects and enable an easy interpretation of the results.

We will add the axes labels to the relevant figures.

6. Action:
- Axes names will be added to the figure.
- The following clarifications have been added to the text:
  - Lines 245 - 250: The KE extracted by the wind turbines is represented by the total yield of the deployment. Although the reduction in mean wind speeds with KE removed is not strictly linear, we utilise linear fits. The linear fit makes it easier to highlight key first order effects i.e that reductions in mean wind speeds are higher when more KE is extracted and that reductions are steeper at night than during the daytime. Thus, the choice of linear fits emphasise the first-order effects and eases the comparison between WRF, KEBA and the Standard approach.
  - Lines 273 - 275: Like Fig.3(b), capacity factors are plotted against the KE extracted by the turbines. The relation-ship between capacity factors and extracted KE is linear and therefore the slope (1 /W · m^{-2}) shows that the generation efficiency reduces as more KE is extracted from the atmosphere.
  - Slope units for Figure 3 added in text: line 253, line 274 and in the Figure captions.

7. "This is likely because KEBA assumes a well-mixed boundary layer volume that is characterized by one effective wind speed, veff." --> I do not quite follow this argument. I think there are two elements that need unpacking here: 1) why is it justified to assume the same wind speed at all heights in the boundary layer? 2) Why is it justified to assume the same wind speeds at the 1st and the 1000th wind turbine in the wind park? In reality, winds strengthen with height and will weaken as air travels through the wind park. Please add an explanation why your approach is justified despite these concerns.

7. Response:

The statement “ This is likely because KEBA assumes a well-mixed boundary layer volume that is characterised by one effective wind speed, veff.” is only meant to be conceptual scaffolding intended to aid the interpretation of the results. Since KEBA only budgets the KE within the boundary layer, it implicitly assumes that KE anywhere within the boundary layer is instantaneously available to the turbine. However, since the real atmosphere transports KE via air masses, the availability of KE at the turbine can be quick or slow depending on the stability conditions. Then KEBA can be thought of as being closer to the highly unstable condition than to the highly stable condition. This thought process is useful for interpreting the differences between WRF and KEBA during day and night times.

KEBA makes no assumptions about the vertical variations in wind speeds. In terms of input wind speeds it only needs the hub-height wind speeds, which can be either observed or modelled(Kleidon & Miller, 2020). The hub-height wind speeds are enough for estimating turbine yields since the turbine yield is a function of the hub - height wind speeds. A more representative description of the vertical structure of wind speeds is not needed since the aim is simply to estimate the mean wind speed reductions, generation, and CF at the aggregate level of the regional deployment while accounting for KE removal effects. KEBA captures the impact of KE removal through the reduction factor, which is a function of the number of turbines deployed, the dimensions of the turbine deployment and the height of the boundary layer. The multiplication of this factor with the incoming wind speeds results in the effective wind speed. This effective wind speed can then be thought of as the wind speed that all the wind turbines operate at if KE removal is accounted for. An accurate description of variations in wind speeds and generation within the deployment is not within the scope of our analysis. As our results show, the KEBA approach is leads to a significant improvement in technical potential estimates, relative to the Standard approach.

7. Action:

- Added text lines 204 - 217: It should be noted that KEBA budgets the KE fluxes in the boundary layer over the entire wind turbine deployment with the aim of estimating atmospheric response impacts on energy yield and wind speeds at the scale of a regional deployment. It does not attempt to model the horizontal or vertical variation of wind speeds or energy yield within the deployment. Therefore, the only forcing input needed are the wind speeds at the turbine's hub height, v_{in}. This suffices because the the turbine yields are a function of the hub-height wind speeds (Fig 1b). Wind speed data used here is sourced from Miller et al. (2015) but observed wind speeds can also be used. The budget constraints on the boundary layer KE fluxes allow for wind speed reduction over the whole deployment or effective wind speeds (v_{eff}) to be estimated. The reduced wind speeds can be thought of as that which the deployment effectively operates at when the KE flux budget constraints are accounted for. This approach is fit for our study despite being a simplified representation of the boundary layer and the atmosphere - turbine interactions. This is because we are only interested in evaluating the impacts of atmospheric response on energy yield and wind speeds at the aggregate scale of the deployment. The evaluation of the finer variation within the deployment is not within the scope of our study. Further, it is also important to keep in mind that KEBA is simple in its formulation only compared to WRF. It is significantly more sophisticated inits representation of atmospheric physics relative to the Standard approach.

8. How are your results impacted by the choice of a wind turbine with relatively low hub height? Since mean wind speeds would be higher at, say 120m, wind speed reductions due to resource depletion might be less important if the turbines operate more often in the rated regime. I suggest to add technology uncertainty to your discussion of the limitations of the approach.

8. Response:

KEBA's performance relative to WRF and Standard approach when modern wind turbines with larger capacities and higher-hub heights are used, remains similar. Although we do not test KEBA's sensitivity to turbine choice here, KEBA has been used to evaluate the regional wind energy resource potential with larger capacity, higher hub-height turbines (Agora Energiewende 2020). In this evaluation, 12 MW wind turbines with ~150m hub-height within a higher wind speed offshore environment were tested. Similar to our results, this comparison also showed that KEBA estimates of technical potential were in closer agreement with WRF than the Standard approach, much like the current study. KEBA estimates of capacity factors were found to be within 15% of the WRF over the entire range of simulated deployment scenarios. This result is expected as higher wind speeds lead to a greater proportion of the installed wind turbines operating at higher efficiencies. This means that the kinetic energy budget is depleted to a larger extent. This means that reductions in wind speeds are likely to remain relevant even when taller wind turbines with larger capacities are considered under higher wind speed conditions.

We will technology uncertainty to the limitations section.

8. Action:

- Added text to limitations lines 326 - 331:Additionally, the impact of improving wind turbine technologies i.e higher turbines with larger capacities, on KEBA estimates has not been explicitly evaluated in our study. However, it is expected that our results remain largely similar in spite of improvements in turbine technology. An analysis of German Bight potentials (Badger et al., 2020) showed that KEBA's estimates of capacity factor were within 15% of WRF even when taller and larger turbines were assumed (150m, 15 MW). That said more analyses in different geographical regions with a range of turbine types need to be performed.

9. Conclusion: "We conclude that the KE removal effect is the predominant physical influence that shapes technical wind resource potentials at the regional scale." I do not think that you have shown that. The dominant phyical effect is wind speed. You have shown that the KE removal effect becomes sizeable when capacity density and park are are both very large and that KEBA can be used to estimate it with some level of confidence (although the deviation from WRF remains sizeable as well and one could also questions whether WRF is the best ground truth)

9. Response:

We will revise the conclusion in line with the reviewer's comments.

9. Action:

- Updated lines 429 - 430: We conclude that the impact of the KE removal effect on the technical wind energy potential of dense, regional scale wind turbine deployments is significant

Minor

1. All slopes are missing units! For example, in lines 226 and 227 but also elsewhere.

1. Action:
   - Slope units included in lines 253 and 274, and Figure 3. caption.
   - Slope units have been added on Figure 3.

2. Figure axis labels: Please add the variable name in addition to the units. The units themselves are not clear. For example, in Fig. 4, both axis have the same units (except a factor 10^6) but they have different meaning. This comment applies to almost all Figures.

2. Action:
   - variable names added to the Figure axes where applicable (Figures 1, 3, 4, 5, 6)

3. In the abstract, I suggest to cut down the introductory sentences to increase legibility (and make the paper more attractive to readers). Basically, I recommend to shorten lines 1-10 to maybe 4 lines or so.

In the abstract you write: "However, the depletion of wind resource or the reduction in wind speed scales with the total capacity installed within the deployment." I see two problems with this statement. First, it is unclear whether this is a result from the current analysis or a general statement. Second, I don't believe that it holds in general. For example, a wind park with 1GW capacity spread out over area A would not see the same depletion as 1GW wind park spread out over 100*A.

l. 15 ff: not clear what the percentages refer to. Relative to what?

3. Response:
   - The abstract has been shortened in line with the suggestions of the reviewer.
   - The percentages are relative to WRF and will be specified in the abstract.
   - The line in question has been revised to indicate that this has been previously reported in the literature. It has been updated to state that wind resource depletion increases with the amount of kinetic energy removed instead of the capacity of turbines.

3. Action:
   - Updated Abstract

4. The Introduction is generally of good quality. As a reviewer, I have nothing to critize. However, as a reader I would have prefered more conciseness.

4. Response: We opted to include a more detailed introduction in order to provide a more rounded description of our approach, and where it fits within the larger context of wind energy research based on the previous ERL review and the editors comments prior to submission for review.

4. Action:
   - None

5. l. 41: you are missing a verb in this sentence

"This effect is borne out in observation data" --> unclear what this means.

5. Action:
   - Updated sentence 41.

- Removed the sentence.

6. "The winds of the large-scale circulation and KE associated with their mean flow are predominantly generated in the free atmosphere by differences in potential energy due to differential solar radiative heating (Peixoto and Oort, 1992; Kleidon, 2021)." --> suggest to define free atmosphere. And do you mean atmosphere or troposphere?

6. Action:
   - Defined free atmosphere as the part of the Earth's atmosphere that is above the planetary boundary layer and is impacted negligibly by the impacts of surface friction (glossary.ametsoc.org) - lines 86 - 87

7. Fig. 2: I like the idea of a conceptual figure. I noted a few things in this figure that you might want to change:

   - I would not use arrows to depict the boundary layer height because you use arrows to depict momentum fluxes.

   - The circular arrow behind the wind parks seems to suggest that a circulation cell forms during the day. I don't think that this is what you suggest

7. Response:
The conceptual figure will be updated according to the suggestions of the reviewer. The circular arrow highlights that the boundary layer is well-mixed during the day given the generally unstable stability conditions.

7. Action:
- Figure updated to indicate boundary layer height with lines without arrows.

8. Which GCM are you talking about in Sec. 3.8?

8. Response:
- Miller et al 2016, PNAS used the Planet Simulator GCM
- Jacobson et al 2016, PNAS GATOR-GCMOM

8. Action:
None

9. Figure 5 needs a legend that explains the different markers.
9. Action:
- Legend added to Figure 5.

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3. Lopez, A., Roberts, B., Heimiller, D., Blair, N., and Porro, G.: Tech. rep., National Renewable Energy Laboratory, https://doi.org/https://doi.org/10.2172/1047328, 2012.
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5. Agora Energiewende, Agora Verkehrswende, Technical University of Denmark and Max-Planck-Institute for Biogeochemistry (2020): Making the Most of Offshore Wind: Re-Evaluating the Potential of Offshore Wind in the German North Sea.

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