nature portfolio

Peer Review File

Detecting and Calibrating Large Biases in Global Onshore Wind Power Assessment Across Temporal Scales

Corresponding Author: Professor Zhiwei Xu

This manuscript has been previously reviewed at another journal. This document only contains information relating to versions considered at Nature Communications.

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

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The paper “Large bias in global wind power assessment across temporal scales and the calibration method” examines the effect of temporal aggregation of wind speed on the calculation of wind power density (WPD), a commonly used metric for assessing wind power potential, in observations, reanalysis data, and climate model output. The authors propose that the use of longer (daily) averaging periods significantly underestimates the resulting WPD compared to the results of using higher-frequency (hourly) wind speed data, and that this underestimation can have large effects on the long-term climate change mitigation strategies. The study also points out that this underestimation is stronger in weak-wind environments. The study gives a detailed insight into the regional differences of wind speed distributions and best-fitting parameters to approximate WPD, and develops a robust statistical relationship between averaging period and WPD. The authors then develop a calibration factor which is region-specific and can be used to convert a WPD that was derived from low-frequency wind speed records to one much closer to actually observed values. They demonstrate that the correction of WPD with the calibration factor reduces the relative bias of daily-based WPD estimates considerably and represents a more cost-effective alternative to relying on high-frequency wind speed data, which – especially in climate models – is not always available.
The methodology is sound, well described, and generally reproducible. However, some of the data sources described in the method section should be moved to the reference list and provided with a last access date.
All in all, I feel the study constitutes an important contribution to the research community and is likely to spark further research towards a more accurate and comparable assessment of current and future wind power potential.
Overall, I strongly suggest publication of this study in Nature Communications after addressing minor comments and suggestions, which I will elaborate on in the section below.

General: I already like the discussion of the results and also the description of limitations and challenges that arise with WPD assessment and calibration. Regarding the application to future WPD estimates, I would like to see a little more differentiation between assessing changes in WPD and assessing future absolute WPD. While assessing future relative changes in WPD does not critically depend on the calibration as long as the climate model and the historical analysis are “equally biased”, having an accurate calibration factor is essential to the estimation of future absolute WPD. While reading I felt that this core implication and strength of the paper is a slight bit undersold, and should be moved from the supplement to the main discussion section.

L1-2: This really comes down to personal taste, but I believe the second part of the current title sounds a bit clunky. I would suggest renaming the paper to something like “Detecting and calibrating large biases in global wind power assessment across temporal scales”.

L92: Is there a specific reason why you chose this location in rural Canada as an example?

L149-156: Here, you jump from globally valid results (L111-148) to results that are valid for the exemplary location in Canada only, without mentioning that you do so. During the first reading, I was a tad confused as it sounds like the Weibull shape and scale parameters you mention in this section are valid for the whole globe, which of course would not make

sense. I suggest adding that you now focus on the example from earlier on again.

L261-275: I personally don't see the added value in repeating the analysis for the six WPD levels. The information that is contained in Fig. 3d) can also be extracted from Fig. 3c) (same for Fig. S9/S11 b,d,f compared to S9/S11 a,c,e). Furthermore, your gridded 2x2° K-value dataset which you present earlier in the manuscript (Fig. S12) already provides an excellent lookup table for the practical application of WPD calibration, arguably even with much higher accuracy and spatial resolution.

L283: I believe this should refer to Fig. 1d) instead of 1c).

L317-321: I would argue that wind gustiness is definitely a factor in the decay of WPD with increasing aggregation period length, however not at the timescales investigated in this study. While gusts occur at a second to sub-second scale, the biggest point here in my opinion is that the characteristic timescale of synoptic-scale high-wind features (e.g., the strongest part of an extratropical cyclone) lies somewhere between an hour and a day. You demonstrate very nicely that increasing the temporal resolution can resolve most of the biases and strong underestimation of WPD arising from daily-mean wind speed data. Gusts, however, are already averaged out, both in hourly and daily-resolved time series.

L345-347: I would call this a similarity instead of a coincidence, as the high correlation between 10-minute and 1-hour wind speed averages is not coincidental, but rather caused by variability on timescales longer than 1 hour that is present in both time series.

L367-368: This sentence reads like a repetition of L349-350.

L390-391: The link to the dataset should appear in the reference list and/or in a data availability statement at the end of the manuscript, along with the date of last access. The main body of the text should just contain the short citation. Same suggestion for L408.

L404: You specify it in the next sentence, but I still would not use the plural form "models" here if you only took data from EC-Earth3. It is perfectly valid to use one model only as a representative for the larger cohort, but I would not recommend introducing this paragraph by creating the expectation of a multi-model study.

L408: The link should be in the reference list, see my comment to L390-391.

L446: Please indicate the meaning of the gamma function in Eq. (4) in the text.

Figs. 2, S8, possibly also S5/S6: Could you mark the locations of the selected stations on any of the maps?

Figs. 2a & S2: The color of the curves in Fig. 2a) suggest a WPD level of somewhere between 100 and 200 W/m² for daily data, using the colorbar in Fig. 2g). However, in Fig. S2a), the observed WPD(daily) is 310 W/m² for the same location. Could you please explain the disparity between the two plots? Maybe I am missing something here. For WPD(hourly), both figures seem to agree on >400 W/m².

Fig. 3c: On first glance, it appears counterintuitive why the seemingly sparse scatter points in the lower left part of the plot are colored yellow, indicating a high point density. I assume this is because you calculate the density in linear WPD space, not in log(WPD) space, and the logarithmic x-axis distorts the densities, correct?

Fig. 4: The uppercase roman numerals in subplots 4f) and 4g) might be confused with the WPD levels (I through VI) that you introduced in Figure 3. I suggest to consider choosing another label here, for example lowercase roman numerals (i, ii, iii) or uppercase letters.

Supplementary Text L7-10: Please specify that study by Li et al. (2022) focuses on China only, contrary to Vautard et al. (2010). Thus, the statement on declining WPD trends in most regions should also be constrained to China.

Supplementary Figures: Would it be possible to order the supplementary figures by their appearance in the main text? For instance, currently Figure S7 is mentioned before S5 and directly after S2.

Reviewer #2

(Remarks to the Author)
This paper proposes a calibration method to reduce the biases in global wind power assessment across different temporal and spatial scales. As such, it could be a highly relevant and important contribution to the literature on this topic, potentially provides insight into the common underestimation of wind potential in large-scale studies and fits well in the scope of Nature Communications. The methodology is straightforward and the obtained result provides a good overview of the magnitudes of biases in wind power assessment.
But in its current form, the manuscript falls short in many respects, especially for Nature Communications, as outlined in detail below. Our main criticism is that the paper is written rather like a standard research paper, it contains a lot of repetition and does not deliver a concise, clear and powerful analysis that is expected from this journal. The ultimate problem that is addressed, that of the bias due to low/different data resolutions, is the result of a trade-off between accuracy and efforts. But the authors do not raise this important trade-off in their paper, instead only focusing on the point of the bias. If a method is

20% off but uses 50% fewer resources, then for some applications it may suffice, whereas others may need the extra effort to attain 10% or lower errors. This means that the method is not systematic in deriving a bias for a specific resolution, which is generalizable and thus useful to decide what resolution is required based on the above considerations.
The style contains a lot of long, nested sentences, which does not aid readability, for example on p.4 and p. 16. More importantly, the “so what?” question is not answered by the majority of the paper, in other words it is written in a rather conventional style that does not push the main implications / takeaways to the forefront and present them in a convincing way. So the relevance, importance and wider context for the main outcomes of the paper is missing or not well communicated.
Overall, then we consider this topic interesting for analysis but this current version of the manuscript cannot be considered for publication in the current form. A complete rewrite of the paper is required that goes beyond major revisions. We leave it at the editor’s discretion which overall decision this results in.
Below are some more detailed comments on the paper:
1. The introduction is too long, waffling and contains much repetition of the same content. Instead the authors are advised to introduce a clear structure to break their article down into digestible and logical chunks.
2. A proper definition of wind power density (WPD) is not given. WPD can be easily confused with wind farm power density, which depends on turbines’ technical characteristics, as explored by other studies such as: https://doi.org/10.1016/j.enpol.2018.08.023.
3. The dependence of WPD calculations on Δt: Figure 1 shows clearly the underestimation of WPD at lower temporal resolutions, but Figure 2 (b) and (c) are unclear due to missing color legend, (e) and (f) have unclear message: are they to show a weak or intermediate correlation?
4. Calibration of WPD across temporal scales: When $R < 0.7$, the correlation is moderate instead of “pronounced”, although it passed the statistical significance test.
5. The m in Equation (1) should be called the shear exponent in the power law. The power law is in general less accurate than logarithmic wind profile equation. Since there is already available global land cover data (WorldCover: https://esa-worldcover.org/en), it would be valuable to use logarithmic law based on the land cover (e.g. https://www.researchgate.net/publication/228474930), which also aligns with the goal of the paper to improve the accuracy of WPD calculation.
6. Implications for future wind power assessment: findings are clearly presented but figures can be improved to aid readability and interpretation, both in the figure itself and in the caption.
7. Discussion and summary: In Figure S14, the numbers at the top are not clearly defined.
8. Supplementary text, figs and tables: whilst reference is made to some of these figures in the article, it is not clear what purpose the whole collection of supplementary material serves and this should be clarified in the main article.

Reviewer #3

(Remarks to the Author)
While it is well-known that there is an influence of temporal resolution on WPD, there has been no systematic global quantification of this effect to date. This article closes this research gap. In addition, the calibration factors presented are also very useful for use in future studies. However, some aspects of this investigation need to be clarified. I hope the comments will help the authors to optimize the manuscript.

P1L22: Please cite the latest Global Wind Report 2024.
P3L82: Does the data set also include data from buoys? If not, what about the transferability of the study to offshore regions? Since the article deals with onshore wind energy, the term “onshore“ should be mentioned in the title.
Figure 1: What is the white bar for the ERA5 wind speed? Is it the prime meridian?
P1L20-25: Do the values in the first section refer to the combined offshore and onshore wind energy?
P17L312: “asΔt“ --> Please correct the typo.
P19L357: However, wind turbines are also sometimes installed in other forms of land use. It would be interesting if the authors could also subdivide their analysis into different types of land use.
P20L379: It should already be mentioned in the first section (Main) that the time issue is only one of many uncertainty factors. The article partly gives the impression that this is the main problem of wind resource assessment.
P20L393: What is the rationale behind the 80% threshold?
P21L406: The general uncertainty of climate models should be mentioned even more clearly.
P21L421: The surface roughness coefficient depends on the land-use. It is also variable in terms of time. The authors should consider the use of more accurate methods for extrapolating wind speed. Otherwise, it is essential to quantify the possible error magnitudes.
P22L434: Instead of equation 2, it would make sense to use a real wind turbine power curve and calculate the actual power output.
P22L436: Air density is not a constant. Especially in regions with high elevation, it is usually significantly lower than the specified 1.225 kg/m3. Although this fact has no influence on the research question of this study, it should nevertheless be mentioned.
P22L441: Although the Weibull distribution is indeed used in most cases, it does not appear to be suitable for all wind regimes in the world. The use of other, more flexible distributions should be examined.

Reviewer #4

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career

Researchers who co-review manuscripts.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I thank the author for the revised version of their manuscript “Detecting and Calibrating Large Biases in Global Onshore Wind Power Assessment Across Temporal Scales”, along with their response to my comments, which was detailed and concise. My previously raised comments and issues were addressed and answered in a satisfactory manner. The changes made to the manuscript, including those in response to the comments of the other referees, have improved the quality, conciseness, and scientific impact considerably. I can therefore recommend the publication of the manuscript in Nature Communications.

Reviewer #2

(Remarks to the Author)
Thank you for carefully addressing the comments and implementing the suggested changes. The updated manuscript clearly describes the methodology and interprets the results. We are satisfied with this version and suggest that it be accepted for publication.
It is nice to see the surface roughness lengths based on WorldCover are considered for using logarithmic law. This could provide nice inputs for any large-scale onshore wind resource assessment.
Supplementary Figure 7: Interesting finding on overestimation cases of using daily resolution. The explanation was clear, but why is it only a “likely” cause? It should be easy to check whether the RB changes to negative if you remove the cutout to “confirm” your explanation.

Reviewer #4

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Response to the Reviews (Point-by-point Replies)

Dear Reviewers,

We sincerely thank you for your thoughtful and constructive comments on our manuscript. Your detailed feedback has significantly helped us enhance the quality, clarity, and impact of our work. We have revised the manuscript accordingly, and below we provide point-by-point responses to each of your comments, outlining the specific changes made to address your concerns.

To facilitate readability, the reviewers' original comments are in blue, and our responses are in dark color.

Reviewer #1 (Remarks to the Author):

The paper "Large bias in global wind power assessment across temporal scales and the calibration method" examines the effect of temporal aggregation of wind speed on the calculation of wind power density (WPD), a commonly used metric for assessing wind power potential, in observations, reanalysis data, and climate model output. The authors propose that the use of longer (daily) averaging periods significantly underestimates the resulting WPD compared to the results of using higher-frequency (hourly) wind speed data, and that this underestimation can have large effects on the long-term climate change mitigation strategies. The study also points out that this underestimation is stronger in weak-wind environments. The study gives a detailed insight into the regional differences of wind speed distributions and best-fitting parameters to approximate WPD, and develops a robust statistical relationship between averaging period and WPD. The authors then develop a calibration factor which is region-specific and can be used to convert a WPD that was derived from low-frequency wind speed records to one much closer to actually observed values. They demonstrate that the correction of WPD with the calibration factor reduces the relative bias of daily-based WPD estimates considerably and represents a more cost-effective alternative to relying on high-frequency wind speed data, which – especially in climate models – is not always available.

The methodology is sound, well described, and generally reproducible. However, some of the data sources described in the method section should be moved to the reference list and provided with a last access date.

All in all, I feel the study constitutes an important contribution to the research community and is likely to spark further research towards a more accurate and comparable assessment of current and future wind power potential.

Overall, I strongly suggest publication of this study in Nature Communications after addressing minor comments and suggestions, which I will elaborate on in the section below.

Reply: Thank you very much for recognizing the significance and value of our work. We sincerely thank you for the thoughtful comments and constructive feedback. Your suggestions have been instrumental in improving the clarity and robustness of our manuscript. Below, we address each of your comments point by point:

General: I already like the discussion of the results and also the description of limitations and challenges that arise with WPD assessment and calibration. Regarding the application to future WPD estimates, I would like to see a little more differentiation between assessing changes in WPD and assessing future absolute WPD. While assessing future relative changes in WPD does not critically depend on the calibration as long as the climate model and the historical analysis are "equally biased", having an accurate calibration factor is essential to the estimation of future absolute WPD. While reading I felt that this core implication and strength of the paper is a slight bit undersold, and should be moved from the supplement to the main discussion section.

Reply: Thank you for this valuable suggestion. We have revised the Results and Discussion sections to more clearly differentiate between assessing relative changes in WPD and estimating absolute future WPD. Specifically, we have emphasized the critical role of accurate calibration in projecting absolute WPD values under future climate change scenarios, which is essential for wind power planning and policymaking.

We also clarified that the study focuses on the impact of Δt on absolute future global onshore WPD (as shown in Fig. 4) rather than relative changes to the present. Importantly, relative changes in WPD remain largely unaffected by applying the calibration coefficient K across different greenhouse gas emission scenarios (Supplementary Fig. 12). These revisions are detailed in the updated Results section (Pages 13-14, Lines 236-243).

1. L1-2: This really comes down to personal taste, but I believe the second part of the current title sounds a bit clunky. I would suggest renaming the paper to something like "Detecting and calibrating large biases in global wind power assessment across temporal scales".

Reply: Thank you for this great suggestion. We have revised the title to: "*Detecting and Calibrating Large Biases in Global Onshore Wind Power Assessment Across Temporal Scales*". The addition of "onshore" aligns with Reviewer #3's suggestion to clarify the study's focus on land-based wind energy.

2. L92: Is there a specific reason why you chose this location in rural Canada as an example?

Reply: The location was chosen from stations with high-resolution wind speed observational data, ensuring representation of diverse wind conditions. This site serves as an illustrative example to demonstrate the effects of temporal resolutions on WPD calculations. We have clarified this rationale in the revised manuscript (Page 3, Lines 72-74).

3. L149-156: Here, you jump from globally valid results (L111-148) to results that are valid for the exemplary location in Canada only, without mentioning that you do so. During the first reading, I was a tad confused as it sounds like the Weibull shape and scale parameters you mention in this section are valid for the whole globe, which of course would not make sense. I suggest adding that you now focus on the example from earlier on again.

Reply: Thank you for identifying this issue. We have revised the text to explicitly state the transition to the example in rural Canada. The updated section clarifies that the analysis now focuses on a specific site to explore statistical characteristics of wind speed distributions. Please refer to Page 6, Lines 110-111 in the revised manuscript.

4. L261-275: I personally don't see the added value in repeating the analysis for the six WPD levels. The information that is contained in Fig. 3d) can also be extracted from Fig. 3c) (same for Fig. S9/S11 b,d,f compared to S9/S11 a,c,e).

Furthermore, your gridded 2x2° K-value dataset which you present earlier in the manuscript (Fig. S12) already provides an excellent lookup table for the practical application of WPD calibration, arguably even with much higher accuracy and spatial resolution.

Reply: Thank you for this insightful observation. We have carefully considered this suggestion, and we recognize that the necessity of analyzing the six WPD levels might not have been fully explained in the original submission. While Fig. 3c serves as the foundation for Fig. 3d, the latter provides a simplified classification scheme for applying K-values in different wind energy environments. This approach is particularly useful in scenarios where future wind energy conditions may change significantly due to climate change. For example, regions transitioning from one wind power class to another, as defined by the National Renewable Energy Laboratory (Elliott, 1986), can refer to Fig. 3d for appropriate K-values across WPD levels.

Additionally, we would like to clarify the complementary roles of Fig. 3d and the gridded 1°x1° *K*-value dataset (Fig. S12, now Supplementary Fig. 8 in the revision). Supplementary Fig. 8 offers a gridded 1°x1° K-value dataset, which is ideal for calibrating WPD under current or stable wind conditions with high spatial resolution. However, for regions experiencing

substantial changes in wind conditions under global warming scenarios, recalibrating WPD would require insights from both Fig. 3 and Supplementary Fig. 8.
To ensure clarity, we have included a detailed explanation of the practical applications of both Fig. 3 and Supplementary Fig. 8 in the revised manuscript. Additionally, we have made the datasets from Fig. 3 and Supplementary Fig. 8 available in the source data file for easy access and practical use.

5. L283: I believe this should refer to Fig. 1d) instead of 1c).

Reply: Thank you for catching this error. The figure reference has been corrected in the revision.

6. L317-321: I would argue that wind gustiness is definitely a factor in the decay of WPD with increasing aggregation period length, however not at the timescales investigated in this study. While gusts occur at a second to sub-second scale, the biggest point here in my opinion is that the characteristic timescale of synoptic-scale high-wind features (e.g., the strongest part of an extratropical cyclone) lies somewhere between an hour and a day. You demonstrate very nicely that increasing the temporal resolution can resolve most of the biases and strong underestimation of WPD arising from daily-mean wind speed data. Gusts, however, are already averaged out, both in hourly and daily-resolved time series.

Reply: Thank you for this insightful clarification. We fully agree with your assessment. In the revised manuscript, we explicitly state that the decay in WPD is primarily driven by the resolution of synoptic-scale high-wind features, rather than gustiness. This distinction is now clearly articulated in the revised manuscript (Page 16, Lines 258-259).

7. L345-347: I would call this a similarity instead of a coincidence, as the high correlation between 10-minute and 1-hour wind speed averages is not coincidental, but rather caused by variability on timescales longer than 1 hour that is present in both time series.

Reply: We agree with this clarification. The term "coincidence" has been replaced with "similar" in the revised manuscript. The updated text appears on Page 16, Line 275.

8. L367-368: This sentence reads like a repetition of L349-350.

Reply: We have removed the repetitive sentence to improve conciseness and avoid redundancy.

9. L390-391: The link to the dataset should appear in the reference list and/or in a data availability statement at the end of the manuscript, along with the date of last access. The main body of the text should just contain the short citation. Same suggestion for L408.

Reply: Thank you for this suggestion. The dataset links have been moved to the Data Availability statement, which now includes last access dates. The main text contains only brief citations. Please refer to the updated Data Availability statement in the revised manuscript.

10. L404: You specify it in the next sentence, but I still would not use the plural form "models" here if you only took data from EC-Earth3. It is perfectly valid to use one model only as a representative for the larger cohort, but I would not recommend introducing this paragraph by creating the expectation of a multi-model study.

Reply: Thank you for pointing this out. The text has been revised to clarify that only EC-Earth3 data were used as a representative model output. This model was selected because it provides 3-hourly wind speed data, which meets the requirements of this analysis. The revised wording appears on Page 18, Lines 324-326.

11. L408: The link should be in the reference list, see my comment to L390-391.

Reply: Corrected as suggested.

12. L446: Please indicate the meaning of the gamma function in Eq. (4) in the text.

Reply: Added as suggested.

13. Figs. 2, S8, possibly also S5/S6: Could you mark the locations of the selected stations on any of the maps?

Reply: Thank you for this suggestion. We have updated Fig. 2 to include markers indicating the selected station locations for better clarity. For supplementary figures (e.g., Fig. S8, now Supplementary Fig. 6), where displaying station locations on the map is less effective, the locations have been explicitly described in the captions.

14. Figs. 2a & S2: The color of the curves in Fig. 2a) suggest a WPD level of somewhere between 100 and 200 W/m² for daily data, using the colorbar in Fig. 2g). However, in Fig. S2a), the observed WPD(daily) is 310 W/m² for the same location. Could you please explain the disparity between the two plots? Maybe I am missing something here. For WPD(hourly), both figures seem to agree on >400 W/m².

Reply: Thank you for pointing this out. The disparity arises because Fig. 2a) represents WPD values based on annual averages (2014–2021), while Fig. S2a) corresponds to data for May 2021 only. This distinction has been clarified in the captions. To avoid misunderstanding, we have used different color bars for subplots. The color bars in Fig. 2a) has been adjusted to

distinguish them more clearly from Fig. 2g).

15. Fig. 3c: On first glance, it appears counterintuitive why the seemingly sparse scatter points in the lower left part of the plot are colored yellow, indicating a high point density. I assume this is because you calculate the density in linear WPD space, not in log(WPD) space, and the logarithmic x-axis distorts the densities, correct?

Reply: Yes, the density was calculated in linear WPD space. A clarification has been added to the figure caption.

16. Fig. 4: The uppercase roman numerals in subplots 4f) and 4g) might be confused with the WPD levels (I through VI) that you introduced in Figure 3. I suggest to consider choosing another label here, for example lowercase roman numerals (i, ii, iii) or uppercase letters.

Reply: Thank you for this suggestion. We have revised the labeling to lowercase Roman numerals (i, ii, iii) to avoid confusion.

17. Supplementary Text L7-10: Please specify that study by Li et al. (2022) focuses on China only, contrary to Vautard et al. (2010). Thus, the statement on declining WPD trends in most regions should also be constrained to China.

Reply: Thank you for this important suggestion. We have revised the statement to specify that Li et al. (2022) focuses on China, and this adjustment has been reflected in the revised Supplementary Note 1.

18. Supplementary Figures: Would it be possible to order the supplementary figures by their appearance in the main text? For instance, currently Figure S7 is mentioned before S5 and directly after S2.

Reply: We appreciate this suggestion. The supplementary figures have been reordered to align with their sequence in the main text for improved consistency and readability.

We sincerely thank Reviewer #1 for the thoughtful feedback and constructive suggestions. We believe the revisions have addressed all concerns and enhanced the clarity and impact of our manuscript. We hope we have addressed all your concerns. Please let us know if there are any further questions or suggestions.

Reviewer #2 (Remarks to the Author):

This paper proposes a calibration method to reduce the biases in global wind power assessment across different temporal and spatial scales. As such, it could be a highly relevant and important contribution to the literature on this topic, potentially provides insight into the common underestimation of wind potential in large-scale studies and fits well in the scope of Nature Communications. The methodology is straightforward and the obtained result provides a good overview of the magnitudes of biases in wind power assessment.

Reply: We sincerely appreciate your detailed feedback and critical comments on our manuscript. Your insights and suggestions have been invaluable in improving the quality, clarity, and overall impact of this work. Below, we address each of your comments in detail and describe the corresponding revisions made to the manuscript.

But in its current form, the manuscript falls short in many respects, especially for Nature Communications, as outlined in detail below. Our main criticism is that the paper is written rather like a standard research paper, it contains a lot of repetition and does not deliver a concise, clear and powerful analysis that is expected from this journal. The ultimate problem that is addressed, that of the bias due to low/different data resolutions, is the result of a trade-off between accuracy and efforts. But the authors do not raise this important trade-off in their paper, instead only focusing on the point of the bias. If a method is 20% off but uses 50% fewer resources, then for some applications it may suffice, whereas others may need the extra effort to attain 10% or lower errors. This means that the method is not systematic in deriving a bias for a specific resolution, which is generalizable and thus useful to decide what resolution is required based on the above considerations.

The style contains a lot of long, nested sentences, which does not aid readability, for example on p.4 and p. 16. More importantly, the "so what?" question is not answered by the majority of the paper, in other words it is written in a rather conventional style that does not push the main implications / takeaways to the forefront and present them in a convincing way. So the relevance, importance and wider context for the main outcomes of the paper is missing or not well communicated.

Overall, then we consider this topic interesting for analysis but this current version of the manuscript cannot be considered for publication in the current form. A complete rewrite of the paper is required that goes beyond major revisions. We leave it at the editor's discretion which overall decision this results in.

Reply: Thank you for your detailed feedback and constructive criticism. In response, we have

made the following major revisions to address these concerns:

1. *Revised structure and writing*: We have rewritten the manuscript to emphasize key takeaways, improve readability, and remove redundancy. Long and nested sentences have been simplified to enhance clarity and engagement. The structure has been refined to ensure logical flow and to bring the main implications of the study to the forefront.

2. *Trade-off Between Accuracy and Effort*: We fully agree with the importance of emphasizing the trade-off between accuracy and resource requirements in WPD assessment. To address this, we have incorporated detailed discussions in the revised manuscript, focusing on the balance between bias reduction and computational efficiency. This highlights the utility of the proposed calibration method, particularly in resource-limited scenarios. Please refer to Page 2, Lines 44-46; Page 3, Lines 60-62; Page 13, Lines 233-235; Page 16, Lines 271-277 in the revised manuscript. The rationale for using WPD(1-hour) as the "target" for rectification has been explained in details in the Methods section (Part 5, Pages 21-22, Lines 407-417).

We would also like to take this opportunity to clarify further. While high-resolution data undoubtedly provides more precise results when resources allow, as reviewers have recognized, our calibration method significantly reduces the relative bias of WPD estimates derived from coarse-resolution data. This offers a cost-effective alternative to relying solely on high-frequency wind speed data, which—especially in climate models—is not always available.

Moreover, this new method enables cross-resolution comparisons of WPD estimates, which is not only crucial for future projections but also highly beneficial for historical data analysis. Many sites only retain records with coarse resolutions from past periods.

In the revised version, we have emphasized the practical value of this calibration method in enabling more accurate WPD assessments, balancing accuracy with computational cost, and facilitating comparisons across temporal resolutions. We have revised and streamlined the text to improve readability and ensure clearer communication of these key points.

3. *Conciseness*: Non-essential details and redundant content have been removed, particularly in the Introduction and Results sections. The title has been revised according to the reviewers' comments, for clarity and conciseness to better reflect the study's focus.

Overall, we have carefully gone through the manuscript and done our best to improve the writing throughout, tightening the prose to facilitate readers' ability to grasp the big picture. Thus, we believe these revisions address the core concerns and improve the manuscript's readability, relevance, and impact.

Below are some more detailed comments on the paper:

1. The introduction is too long, waffling and contains much repetition of the same content.

Instead the authors are advised to introduce a clear structure to break their article down into digestible and logical chunks.

Reply: Thank you for this suggestion. The Introduction has been significantly shortened and streamlined to eliminate redundancy and improve clarity. The revised version focuses on three main points: 1. Background on WPD and its importance. 2. Challenges in assessing WPD across temporal scales. 3. Objectives and contributions of this study.

The revised Introduction provides a concise and focused overview, setting the stage for the subsequent sections.

2. A proper definition of wind power density (WPD) is not given. WPD can be easily confused with wind farm power density, which depends on turbines' technical characteristics, as explored by other studies such as: https://doi.org/10.1016/j.enpol.2018.08.023.

Reply: We fully agree. In the revised manuscript, we have provided a clear and precise definition of WPD early in the Introduction (Page 2, Lines 27-31). Additionally, the detailed calculation method for WPD is now explicitly described in the Methods section for clarity.

3. The dependence of WPD calculations on Δt: Figure 1 shows clearly the underestimation of WPD at lower temporal resolutions, but Figure 2 (b) and (c) are unclear due to missing color legend, (e) and (f) have unclear message: are they to show a weak or intermediate correlation?

Reply: Thank you for pointing this out. We have made the following clarifications and revisions:

Figures 2b and 2c: Each site's fitted curve is now differentiated by distinct colors, which have been explained in the figure caption for clarity.

Figures 2e and 2f: These panels highlight the significant correlations between parameters a and b, and between b and WPD(1-hour). We have clarified in the caption that these correlations are statistically significant and regions with higher WPD (larger a) exhibit slower decay rates (smaller b) with increasing Δt.

4. Calibration of WPD across temporal scales: When R < 0.7, the correlation is moderate instead of "pronounced", although it passed the statistical significance test.

Reply: Thank you for pointing this out. We have revised the wording to describe correlations with R<0.7 as "moderate." The updated text appears on Page 10, Lines 179-180.

5. The m in Equation (1) should be called the shear exponent in the power law. The power law is in general less accurate than logarithmic wind profile equation. Since there is already available global land cover data (WorldCover: https://esa-worldcover.org/en), it would be

valuable to use logarithmic law based on the land cover (e.g. https://www.researchgate.net/publication/228474930), which also aligns with the goal of the paper to improve the accuracy of WPD calculation.

Reply: Thanks for this great suggestion. We have carefully considered your suggestion. In many previous studies, the shear exponent *m* (representing surface roughness coefficient) is often assumed as a value of 1/7 for open land (Elliott, 1986; Tian et al., 2019; Zeng et al., 2019), and the equation of wind profile power law is commonly referred to as the *one-seventh* power law (Elliott, 1986).

However, we fully agree with you that the logarithmic wind profile law is more accurate for extrapolating hub-height wind speeds, especially when paired with land-use-specific surface roughness lengths (z_0). Previous studies have also addressed the error in global wind power assessments arising from applying a constant value (1/7) of power law exponent (Jung and Schindler, 2021). Therefore, following your suggestions, we have replaced the power law with the logarithmic wind profile equation for hub-height wind speed extrapolation. In the revised manuscript, we have used the global land cover data (ESA WorldCover) to determine land-use-specific surface roughness lengths (z_0).

This approach improves WPD calculation accuracy, and all relevant figures and analyses have been updated accordingly. Details are provided in the revised Methods Section (Part 2, Page 19, Lines 333-357) and Supplementary Note 2.

6. Implications for future wind power assessment: findings are clearly presented but figures can be improved to aid readability and interpretation, both in the figure itself and in the caption.

Reply: Thank you for this suggestion. We have revised the figures to improve readability. The legends, annotations, and axis labels (e.g., in Fig. 4d,e,f,g) have been clarified or revised where necessary. Figure captions now provide more detailed explanations of the content and purpose of each panel.

7. Discussion and summary: In Figure S14, the numbers at the top are not clearly defined.

Reply: Thank you for this suggestion. We have clarified the numbers in the figure caption of Figure S14 (now Supplementary Fig. 12). These numbers represent trends in WPD changes across different scenarios.

8. Supplementary text, figs and tables: whilst reference is made to some of these figures in the article, it is not clear what purpose the whole collection of supplementary material serves and this should be clarified in the main article.

Reply: Thank you for this suggestion. We have ensured that each supplementary figure and table is explicitly referenced. Their purpose in supporting the main findings is now clarified in the main text.

We sincerely thank Reviewer #2 for these detailed and constructive comments. Your insights have greatly improved the manuscript's clarity, structure, and scientific rigor of the manuscript. We hope the revisions adequately address all your concerns, and we welcome any further feedback.

Reviewer #3 (Remarks to the Author):

While it is well-known that there is an influence of temporal resolution on WPD, there has been no systematic global quantification of this effect to date. This article closes this research gap. In addition, the calibration factors presented are also very useful for use in future studies. However, some aspects of this investigation need to be clarified. I hope the comments will help the authors to optimize the manuscript.

Reply: Thank you very much for recognizing the significance and value of our work. We greatly appreciate your thoughtful and constructive comments, which have been instrumental in improving the manuscript. Below, we provide point-by-point responses to your comments and detail the corresponding revisions. We hope we have adequately addressed your concerns.

1. P1L22: Please cite the latest Global Wind Report 2024.

Reply: Thank you for pointing this out. We have updated the manuscript to include the latest *Global Wind Report 2024* in the references. Please refer to Page 1, Lines 22-24 in the revised manuscript.

2. P3L82: Does the data set also include data from buoys? If not, what about the transferability of the study to offshore regions? Since the article deals with onshore wind energy, the term "onshore" should be mentioned in the title.

Reply: The dataset used in this study does not include buoy data, as it focuses primarily on onshore observations and reanalysis products. However, our findings suggest that the calibration methods and conclusions may also be applicable to offshore regions. The decay of WPD with increasing Δt reflects intrinsic properties of wind speed distributions, which are related to general wind energy conditions.

Offshore wind speeds are generally higher, and thus WPD over the ocean may be less affected by Δt compared to onshore regions. Our supplementary analysis (Supplementary Figs. 3 and 10) shows that offshore areas exhibit smaller parameter *b* values (indicating weaker Δt effects) and lower K values across reanalysis datasets (ERA5, NCEP/NCAR, and NCEP-DOE). Nevertheless, we acknowledge that further validation using offshore-specific datasets is necessary to confirm the transferability of our findings.

To clarify the study's scope, we have revised the title to explicitly include the term "onshore."

3. Figure 1: What is the white bar for the ERA5 wind speed? Is it the prime meridian?

Reply: Yes, the white bar in Figure 1 corresponds to the prime meridian, an artifact of the

plotting procedure. We have corrected this issue in the revised figure, which no longer includes the white bar.

4. P1L20-25: Do the values in the first section refer to the combined offshore and onshore wind energy?

Reply: Yes, the values in this section refer to the combined offshore and onshore wind energy capacity. We have clarified this in the revised text (Page 1, Lines 22-24).

5. P17L312: "asΔt" --> Please correct the typo.

Reply: Thank you for identifying this typo. It has been corrected in the revised manuscript.

6. P19L357: However, wind turbines are also sometimes installed in other forms of land use. It would be interesting if the authors could also subdivide their analysis into different types of land use.

Reply: Thank you for this great suggestion. In the revised manuscript, we have subdivided our analysis by land-use and land-cover (LULC) types, such as tree cover, built-up, grassland, and cropland, as specified in the ESA WorldCover dataset.

Our analysis shows that the decay of WPD with Δt is consistent across different LULC categories (Supplementary Figs. 15 and 16), further confirming that this decay is an intrinsic property of wind speed distributions rather than a site-specific characteristic.

To improve accuracy of WPD calculation, we used the logarithmic wind profile equation and LULC-specific surface roughness length (z_0) values to extrapolate hub-height wind speeds. While the recalculated spatial patterns of WPD using this method are similar to those derived from the one-seventh power law in previous submission, the logarithmic profile equation provides more accurate results. Corresponding figures and discussions have been updated in the revised manuscript (Discussion and Methods Sections, and Supplementary Note 2).

7. P20L379: It should already be mentioned in the first section (Main) that the time issue is only one of many uncertainty factors. The article partly gives the impression that this is the main problem of wind resource assessment.

Reply: We agree that a broader perspective on uncertainties in wind resource assessments is necessary. The revised Introduction now acknowledges that temporal resolution is one of several factors, alongside spatial resolution and air density variability, that influence WPD calculations (Page 2, Lines 31-34).

8. P20L393: What is the rationale behind the 80% threshold?

Reply: The 80% threshold ensures data quality while maintaining sufficient sample sizes for reliable statistical analysis. This threshold has also been used in previous studies (e.g., Yizhaq et al., 2020) and is now explicitly stated in the revised manuscript.

9. P21L406: The general uncertainty of climate models should be mentioned even more clearly.

Reply: Thank you for this suggestion. We have expanded the discussion in the "Implications for future wind power assessment" Section to explicitly address the general uncertainties of climate models. These include biases in wind speed magnitude and temporal variability, which can influence WPD projections. Please refer to Page 14, Lines 243-244; Pages 17-18, Lines 300-306 in the revised manuscript.

10. P21L421: The surface roughness coefficient depends on the land-use. It is also variable in terms of time. The authors should consider the use of more accurate methods for extrapolating wind speed. Otherwise, it is essential to quantify the possible error magnitudes.

Reply: We fully agree with this great suggestion. We acknowledge that surface roughness (z_0) varies with land-use and land-cover (LULC). In the revision, we used the logarithmic wind profile equation, incorporating LULC-specific z_0 values, to extrapolate hub-height wind speeds. This approach improves accuracy of WPD calculations compared to using a constant z_0 value. However, we also would like to emphasize that the decay relationship of WPD with Δt, and the calibration coefficient K, are minimally affected by LULC types (Supplementary Note 2). This reinforces our conclusion that the bias is an intrinsic property of wind speed distributions rather than site-specific attributes. Nevertheless, incorporating LULC-specific z_0 values enhances the reliability of absolute WPD estimates. Details are provided in the revised Methods Section (Part 2, Page 19, Lines 333-357) and further explained in Supplementary Note 2. Relevant figures have been updated accordingly.

11. P22L434: Instead of equation 2, it would make sense to use a real wind turbine power curve and calculate the actual power output.

Reply: Thank you for this excellent suggestion. In the revised manuscript, we incorporated the power curve of a General Electric GE 1.5s wind turbine (Supplementary Fig. 4) to calculate actual electrical power production (AEP). This analysis highlights discrepancies between daily and 1-hour AEP estimates (Supplementary Figs. 4 and 5), consistent with observed WPD biases. Updated results are described in the revised Results Section (Page 4, Lines 101-105). The calculation of AEP has been added in the revised Methods Section (Part 3, Page 20, Lines 369-

374).

12. P22L436: Air density is not a constant. Especially in regions with high elevation, it is usually significantly lower than the specified 1.225 kg/m3. Although this fact has no influence on the research question of this study, it should nevertheless be mentioned.

Reply: We agree. While air density was assumed constant in this study to isolate Δt effects, we have added a note acknowledging its variability, especially in high-elevation regions. This is now mentioned in the revised manuscript (Page 20, Lines 365-367).

13. P22L441: Although the Weibull distribution is indeed used in most cases, it does not appear to be suitable for all wind regimes in the world. The use of other, more flexible distributions should be examined.

Reply: Thank you for this insightful comment. While the Weibull distribution is widely used (e.g., Justus et al., 1978; Jamil et al., 1995; Pryor et al., 2004; Pryor and Barthelmie, 2010), our analysis highlights significant discrepancies between Weibull-derived and statistically calculated WPD estimates, particularly in extreme wind regimes (Supplementary Figs. 19 and 20). These discrepancies align with findings from prior studies (Pryor et al., 2004; Gil Ruiz et al., 2021), which also note uncertainties in Weibull-based WPD estimates for coarse temporal resolutions. To ensure consistency across Δt values, we adopted the statistical method (Eq. 5) for WPD calculations, as detailed in the revised Methods Section (Part 4, Page 21, Lines 387-394).

We sincerely thank Reviewer #3 for these valuable comments, which have greatly improved the manuscript. We hope the revisions address all concerns, and please let us know if there are any further feedback or suggestions.

Reviewer #4 (Remarks to the Author):

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reply: Thank you very much for participating in the review process for our manuscript and for supporting this collaborative initiative to involve Early Career Researchers in peer review. The comments and suggestions from the reviewers have greatly helped us to improve the quality of our manuscript.

We have carefully revised the manuscript based on all the constructive comments provided by the reviewers. These changes have enhanced the clarity, structure, and scientific impact of the work. We hope that you find the updated version satisfactory. Please feel free to reach out with any further suggestions or questions.

References cited here in the reply:

Elliott DL, Holladay CG, Barchet WR, Foote HP, Sandusky WF (1986) Wind Energy Resource Atlas of the United States (Solar Technical Information Program, US Department of Energy, Washington, DC), p210

Li, M. et al. An assessment of observed wind speed and wind power density over China for 1980–2021. *Wind Energy*. **25**, 2052-2070 (2022).

Tian, Q., Huang, G., Hu, K. & Niyogi, D. Observed and global climate model-based changes in wind power potential over the Northern Hemisphere during 1979-2016. *Energy*. **167**, 1224-1235 (2019).

Zeng, Z. et al. A reversal in global terrestrial stilling and its implications for wind energy production. *Nat. Clim. Chang.* **9**, 979-985 (2019).

Global Wind Energy Council. Global wind report 2024 (GWEC, 2024).

Yizhaq, H., Xu, Z. & Ashkenazy, Y. The effect of wind speed averaging time on the calculation of sand drift potential: New scaling laws. *Earth Planet. Sci. Lett.* **544**, 116373 (2020).

Justus, C. G., Hargraves, W. R., Mikhail, A. & Graber, D. Methods for Estimating Wind Speed Frequency Distributions. *J. Appl. Meteorol*. **17**, 350-353 (1978).

Jamil, M., Parsa, S. & Majidi, M. Wind power statistics and an evaluation of wind energy density. *Renew. Energy*. **6**, 623-628 (1995).

Pryor, S. C., Nielsen, M., Barthelmie, R. J. & Mann, J. Can Satellite Sampling of Offshore Wind Speeds Realistically Represent Wind Speed Distributions? Part II: Quantifying Uncertainties Associated with Distribution Fitting Methods. *J. Appl. Meteorol*. **43**, 739-750 (2004).

Pryor, S. C. & Barthelmie, R. J. Climate change impacts on wind energy: A review. *Renew. Sustain. Energy Rev.* **14**, 430-437 (2010).

Gil Ruiz, S. A., Barriga, J. E. C. & Martínez, J. A. Wind power assessment in the Caribbean region of Colombia, using ten-minute wind observations and ERA5 data. *Renew. Energy*. **172**, 158-176 (2021).

Response to the Reviews (Point-by-point Replies)

Once again, we sincerely thank the reviewers for their thoughtful and constructive comments on this work. Below, we provide our responses to the remaining comments from the last round of review. The reviewers' original comments are in blue, and our responses are in black.

Reviewer #1 (Remarks to the Author):

I thank the author for the revised version of their manuscript "Detecting and Calibrating Large Biases in Global Onshore Wind Power Assessment Across Temporal Scales", along with their response to my comments, which was detailed and concise. My previously raised comments and issues were addressed and answered in a satisfactory manner. The changes made to the manuscript, including those in response to the comments of the other referees, have improved the quality, conciseness, and scientific impact considerably. I can therefore recommend the publication of the manuscript in Nature Communications.

Reply: Thank you once again for your constructive comments in the previous round of review. We are very pleased to see that we have successfully addressed your concerns and that you find the revised manuscript improved in quality and impact.

Reviewer #2 (Remarks to the Author):

Thank you for carefully addressing the comments and implementing the suggested changes. The updated manuscript clearly describes the methodology and interprets the results. We are satisfied with this version and suggest that it be accepted for publication.

It is nice to see the surface roughness lengths based on WorldCover are considered for using logarithmic law. This could provide nice inputs for any large-scale onshore wind resource assessment.

Supplementary Figure 7: Interesting finding on overestimation cases of using daily resolution. The explanation was clear, but why is it only a "likely" cause? It should be easy to check whether the RB changes to negative if you remove the cutout to "confirm" your explanation.

Reply: Thank you once again for your constructive comments in the previous round of review. We are very pleased to see that we have successfully addressed your concerns.

Regarding the last comment, as you suggested, we have checked and confirmed that this is indeed the cause. The relevant wording has been revised accordingly in the manuscript.

Reviewer #4 (Remarks to the Author):

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reply: Thank you once again for participating in the review process for our manuscript and for supporting this collaborative initiative to involve Early Career Researchers in peer review. We truly appreciate your contributions to improving our work.
