

Regional priorities in implementing forestation and wind energy as climate solutions in facing their trade-offs

Corresponding Author: Dr Chao Yue

Parts of this Peer Review File have been redacted as indicated to remove third-party material.

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)
General Assessment

The manuscript titled “Regional priorities in implementing forestation and wind energy as climate solutions in facing their trade-offs” examines the trade-offs between wind energy development and the amount of carbon that can be sequestered through forestation across a range of potential forestation areas. The authors use coupled land-atmosphere simulations from eight earth system models to examine changes in surface wind speed in response to potential forest expansion, finding a significant trade-off between carbon sequestration via forestation and wind energy development.

After an extensive review of the manuscript, I found the paper relevant as it advances the knowledge in the field. The manuscript used an adequate structure for a research manuscript and provides reasonable methods (see major comments for some suggestions). The research question and the scope of the study are well articulated, and the manuscript provides a comprehensive literature review that positions the work within the existing body of research. Finally, the methods and data used in the study allow the study to be reproduced and acknowledge the limitations introduced by the study design.

Comments

R-1) A substantial fraction of the results of the paper are focused on reporting the changes in wind capacity of different regions under scenario S4. However, when reporting these changes, the authors do not account for the various areas of each of the regions. I understand that the introduction of the “efficiency” metric tries to account for this, but I suggest avoiding reporting changes in kW when directly comparing regions of different sizes or at least presenting normalized metrics.

R-2) The use of a neutral stability exponent (i.e., 1/7) in the wind power law does not reflect the diverse land surfaces explored in the study. The study would benefit from using spatially explicit information that uses geography-specific exponents, at the minimum, for each of the 13 explored regions.

See for example recent work reported here: <https://www.sciencedirect.com/science/article/pii/S0306261923018056>

R-3) The discussion would benefit from interpreting the results of the study with other recent papers published in this journal, mainly:

<https://www.nature.com/articles/s41467-025-57606-y>

<https://www.nature.com/articles/s41467-022-33279-9>

R-4) The axis limits of figures included in Figure 1 can be problematic, as they distort the relationship between the presented values. I suggest that all graphs are reported using 0 as the starting point to preserve the point of reference among figures.

R-5) Line 26: The statement “by a factor of 3 to 5, to 2870–5806 GW, with wind energy contributing 36% of the projected global electricity demand” reports a range for the growth factor (i.e., 3 to 5) and capacity (2870-5806 GW), but only one number of projected contributions to global capacity (36%). It would also be reasonable to provide the range.

R-6) Panel f) of Figure 2 is color-coded to represent the 13 regions analyzed in the study. However, there is no legend in this figure describing which color corresponds to each region. It's impractical for the reader to visit Figure 4 to interpret the figure.

R-7) Panel b) of Figure 4 would benefit from text labels indicating a few of the 13 regions depicted at each bar (I understand they are color-coded to represent each region, but this forces the reader to contrast the result with the map while also not accounting for color blindness).

R-8) The y-axis labels of the bar plots in Figure 4 have a typo, as currently, they read “Caplcity Factor” rather than “CapAcity Factor.”

R-9) The use of capital letters across figure labels should be homogenized. For example, the labels in Figure 3 capitalize the first letter only (i.e., Capacity factor), while the bar plots in Figure 4 capitalize all starting letters (i.e., Capacity Factor).
 R-10) Equation 3 in Line 425 reports variables using W, but they are defined in the text using u. The nomenclature needs to be unified.
 R-11) Figures S1 and S2 would benefit from using a color scale with a bigger contrast, as the current white-to-green scale works for the upper part of the scale but not as well for the bottom values.
 R-12) Figure S3 shows that under V1, the wind speed reduction is the minimum among the nine scenarios explored. Given this is an interpolation scenario between S2 and S3, what is the explanation for the less prolonged decrease? Also, why is it that even when wind speeds are increased under this scenario (in comparison to S2), its capacity factor is lower?

Suggestion

Based on the points above, I recommend that the paper be allowed to be reviewed and resubmitted once the proper corrections have been introduced.

Disclosure: I have no affiliation with any of the papers I suggested as part of this review. Authors should feel free to cite the recommended literature as they see fit or replace it with more relevant papers they identify.

Reviewer #3

(Remarks to the Author)

Comments are also attached as a PDF for convenience of authors and editor-

Review of "Regional priorities in implementing forestation and wind energy as climate solutions in facing their trade-offs"

Summary and recommendation

In this paper, the authors examine the tradeoffs between forestation and wind energy expansion since future forestation policies will likely alter surface wind speeds. The authors utilize projections from the Land use inter comparison project (LUMIP) to assess the difference in surface wind speeds on account of additional forestation. The change in the wind speed is then used to calculate the effect on wind energy potential. Previous studies have quantified and established the link between forest cover and surface wind speeds. This is a first study that actually links the same to current wind energy potential. The authors acknowledge that they are using coarse land use projections (0.25 degrees) that they are using to determine local effects on wind energy potential. Regardless, I found this to be a generally well written paper with some interesting quantifications. However, given the limitations in the methods discussed above, I had some questions and suggestions to the authors so that the findings can be explained in more concrete terms for the general audience. I therefore recommend major revisions.

Major comments-

1. Definitions of forestation and effect of protecting current forest vs additional forest plantations- The forestation scenarios are described in a lot of detail which is appreciated; however forestation is a combination of protecting current forests (letting them grow to maturity) and planting new forests. What effect would each of these have on the surface wind speeds and therefore the wind energy potential? I believe this is an important point that the authors don't address. I was interested in understanding whether forest protection i.e. growth in the biomass stock itself would affect the surface winds or would a bigger effect be as a result of new plantations (which tend to be managed monocultures). It seems from Supplementary Figure 5 (also see point 2 below) that forestation and wind energy efficiency is highest close to existing forests with large biomass values (These forests are more likely to be protected rather than replanted). Related to these points, it would be helpful if the authors can discuss the pathways from forestation to surface roughness to wind energy effects by summarizing to what extent, the following factors play a role-

- A. Growth in biomass stock (above and belowground)
- B. Tree heights and biomes
- C. Land effects through new plantations

2. Forestation efficiency map- The forestation efficiency map (Supplementary Figure 5) is a bit confusing and perhaps misleading. Aren't these values more negative close to old growth forests (e.g. Amazon, West Africa, Indonesia). This is mainly because these forests already sequester a lot of carbon regardless of wind energy projects? Perhaps you should compare a "ratio" of current to future carbon compared to the delta in wind energy potential to make this accurate? That would also lead to a very different picture of the efficiency number.

3. Methodology of calculating wind energy potential using wind speeds- One of the biggest value adds of this study (in my opinion) is the translation of wind speed effects to the actual wind energy potential by considering capacity factors. Authors mention the usage of data for a specific 1 MW wind turbine with some sensitivity analysis conducted for another type of turbine in the supplement. However, can the authors elaborate on this point? What is the current type of turbine prevalent on wind farms across different regions (It would be helpful to see the type of turbine currently prevalent or at least characteristics of wind turbines by pixel)? Are there alternatives that are available to this? Moreover, it seems there are differences in the rotor heights of the older and newer turbines (older being between 80-120 m and newer being between 40-80). Can the authors consider a sensitivity analysis to this rotor height as well?

4. Quantification of outcomes- While I appreciate the outcomes as presented by the authors, I think it raised a few questions.

Based on point no 3. above in terms of the turbine and rotor height diversity, what kind of capacity factor improvement would be expected from the current onshore wind farms to maintain their current potential even under a forestation scenario? This seems like an important question that a reader of this study would think of and I believe the authors would have the data to quantify the same. I believe the answer would be regionally specific and would be extremely useful (especially to modelers current working on future wind energy potential).

5. Discussion of results for regions (African continent as an example)- In several places, authors have calculated average CF effects for the African continent as a whole (Figure 2). But this is a bit confusing. Firstly, current installed wind turbines in the continent (based on Figure 3), seem to be largely in the south, but the current dense forests are in West Africa. It also seems that the forestation induced wind energy loss is going to be more prominent in southern Africa rather than western Africa (Supplementary Figure 4). Given these points, can the authors consider amending Figure 4 to include an analysis like Supplementary Figure 4 to make sure these subnational differences are not glossed over?

6. Relating wind energy potential to carbon avoidance- While the metric presented Figure 2 (panel d) is really useful, what are the implications of the reduction in wind energy potential to carbon emissions. After all, wind energy is attractive as a form of carbon emission avoidance. Would a plot looking at forest carbon increase vs carbon avoidance (from wind) look similar? This should be discussed.

7. Implications for future wind energy potential- In the abstract and the discussion, the authors also say that “. Large-scale forestation in Europe and US mainland, however, will diminish future wind energy potential”. However, this point is not really discussed in this manuscript which seems concerned with current wind energy potential, right? Can the authors elaborate on this point.

8. Implications for IAM and earth system modelers – In the discussion, the authors should also add some key takeaways for other modelers. I can see a couple of low hanging fruit takeaways for other modeling teams-
A) For integrated assessment modelers- this analysis suggests that they should consider reductions in wind capacity factors when modelling forestation scenarios (Once again differentiating between protecting existing forests vs planting new forests). IAMs already rely on aggressive forestation scenarios as climate mitigation options.
B) For earth system modelers- This would be a good opportunity to explore coupled effects with climate and land use. An interesting next step would perhaps be to understand the effect of wind energy expansion on forests as well.

Reviewer #4

(Remarks to the Author)

The study titled “Regional priorities in implementing forestation and wind energy as climate solutions in facing their trade-offs” offers a fresh perspective on the trade-off between global forest restoration as a climate mitigation strategy and the development of green wind energy.

However, there are three minor points that should be clarified before the paper is published.

First, in the methods section, the authors assess the influence of reforestation on local surface wind changes at a height of 10 meters. However, to my knowledge, wind turbine blades are positioned well above 10 meters, and they harness wind energy from much higher altitudes. Furthermore, forest canopy heights in many global regions exceed 10 meters (Lang et al., 2023). I recommend that the authors refine their analysis based on regional mature forest canopy heights, at the biome or ecoregion level, which can often reach 20, 30 meters and even higher. This additional analysis would provide a more accurate quantification of the trade-off between reforestation and wind energy generation.

Second, the authors use the term “forestation” to describe forest restoration activities, including both afforestation and natural forest regeneration. However, the appropriateness of afforestation as a natural climate solution remains debated. Converting land that is not naturally suitable for forests may cause unintended long-term ecological consequences. Therefore, I suggest that the authors exclude afforestation from the modeling and analysis to focus solely on the restoration of naturally forested ecosystems.

Third, it is commendable that the authors excluded protected areas to safeguard biodiversity. However, biodiversity outside protected regions is also crucial for global conservation efforts, especially in the context of the Kunming-Montreal Global Biodiversity Framework. Destructive activities associated with wind energy infrastructure, such as road construction, turbine installation, and operational noise, can negatively impact birds and other wildlife. While this issue may fall slightly outside the scope of the current study, I recommend including a brief discussion of these potential biodiversity impacts in the manuscript.

References

Lang, N., Jetz, W., Schindler, K. et al. A high-resolution canopy height model of the Earth. *Nat Ecol Evol* 7, 1778–1789 (2023). <https://doi.org/10.1038/s41559-023-02206-6>

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors have adequately addressed all my suggestions and provided a clear rationale for their decisions. Consequently, I recommend the study be accepted in its current form.

Reviewer #3

(Remarks to the Author)

I would like to thank the authors for all the detailed responses to my comments. Thanks especially for taking the time to add additional metrics and making the paper more policy relevant. I am happy to recommend acceptance of this paper. I have only two minor comments for the authors-

1. Based on the responses, I think it would be really good to clarify that your paper is dealing with "afforestation/reforestation potential" rather than explicit forest protection. I suppose one could gather this by reading the paper and all the responses but would be good to make this clear explicitly in the introduction. After re-reading the paper, I feel that forest regrowth and its effects on wind speeds would be an entirely different topic altogether and confusing/combining this with afforestation/reforestation potential would open up a rabbit hole of new questions.
2. Related to the above, the authors take a biomass/carbon centric approach. This is noted in their responses as well where you say if forests were allowed to regrow by protection, it would have the same effect due to biomass growth (which is true to a certain extent). But forest protection gives certain explicit ecosystem benefits that go beyond carbon. As a simple example, this would lead to less biodiversity loss or even a biodiversity gain. Perhaps similar to point number 1, it would be good to mention that this study does not deal with such ecosystem benefits.

Reviewer #4

(Remarks to the Author)

The authors have done an excellent job in this revised version. They have addressed all of my previous comments and concerns, and I have no further issues with the manuscript.

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Response Letter to Reviewers' Comments

We sincerely thank all referees for their efforts in reviewing our paper and for their valuable comments. Please find below our point-by-point responses to the comments. To make it easy to track our revisions, all substantially modified texts are marked in blue in the revised manuscript.

Reviewer #1 (Remarks to the Author):

[General Comments] The manuscript titled “Regional priorities in implementing forestation and wind energy as climate solutions in facing their trade-offs” examines the trade-offs between wind energy development and the amount of carbon that can be sequestered through forestation across a range of potential forestation areas. The authors use coupled land-atmosphere simulations from eight earth system models to examine changes in surface wind speed in response to potential forest expansion, finding a significant trade-off between carbon sequestration via forestation and wind energy development. After an extensive review of the manuscript, I found the paper relevant as it advances the knowledge in the field. The manuscript used an adequate structure for a research manuscript and provides reasonable methods (see major comments for some suggestions). The research question and the scope of the study are well articulated, and the manuscript provides a comprehensive literature review that positions the work within the existing body of research. Finally, the methods and data used in the study allow the study to be reproduced and acknowledge the limitations introduced by the study design.

[Response] We appreciate the time and effort that the referee has dedicated to providing valuable feedback on our manuscript. The referee’s constructive comments have helped us substantially improve the manuscript quality. Following the referee’s suggestions, additional experiments and substantial revisions have been made. Please find our detailed point-by-point responses below.

[Comment 1] A substantial fraction of the results of the paper are focused on reporting the changes in wind capacity of different regions under scenario S4. However, when reporting these changes, the authors do not account for the various areas of each of the regions. I understand that the introduction of the “efficiency” metric tries to account for this, but I suggest avoiding reporting changes in kW when directly comparing regions of different sizes or at least presenting normalized metrics.

[Response] As the referee pointed out, in the original manuscript, we reported the wind energy changes on an area basis in the unit of kW and hence did not consider the differences in regional areas. Following the referee’s suggestion, we have used the concept of “technical potential” in the revised manuscript, which was widely

adopted by international agencies and extensively used in previous research (Davidson et al., 2016; Gao et al., 2018; Hoogwijk et al., 2004; European Commission., 2025). Technical potential refers to the electricity amount that can be generated over the total land area suitable for wind turbine installation for a given region, after accounting for the constraints of geographical suitability. The use of “technical potential” enabled us to report the changes in total wind energy technical potential and total carbon sequestration potential by accounting for the land areas of different regions, thereby supporting fairer cross-regional comparisons.

To estimate the wind energy technical potential, we first identified the global land area suitable for onshore wind turbine installation at a 1-km resolution. Consistent with the definition used in previous studies (Davidson et al., 2016; Gao et al., 2018; Hoogwijk et al., 2004; European Commission., 2025), the global land area suitable for onshore wind turbine installation (Figure R1) was obtained after accounting for geographic and environmental constraints. Existing forests, water bodies (ESA CCI land cover dataset), high elevation areas (more than 3000 m, 1-km SRTM digital elevation database version 4.1), steeply sloped areas (>20%) (derived from 1-km SRTM digital elevation database) and urban areas are considered as unsuitable for wind turbine installation. When forestation scenarios are considered in our study, the respective potential forestation areas were further excluded from the suitable land for wind energy. Subsequently, the global land area suitable for onshore wind turbine installation at a 1-km resolution was aggregated to a 0.25° resolution. Finally, we calculated the global and regional onshore annual wind energy technical potential (PE, in the unit of kWh/year) in different forestation scenarios according to Equation R1, following a similar approach employed in previous studies (Gao et al., 2018).

$$PE = P_{rated} \times CF \times \frac{A}{0.58} \times 8760 \quad (R1)$$

where P_{rated} represents the wind turbine rated-power (kW), CF denotes capacity factor, and A denotes the land area suitable for onshore wind turbine installation for each grid (in km²). A factor of 0.58 (in the unit of km² per wind turbine), which represents the land area required by a wind turbine, was used to derive the number of turbines that could be installed. The value 8,760 corresponds to the total number of hours in a year.

According to the concept of wind energy technical potential, we have updated Figure 2 in the revised manuscript. We confirm that the revision made in the Methods section did not influence our major conclusions. A detailed description of the method can be found in revised manuscript (Lines 615–680), and the corresponding results are presented in Lines 90–141. The added Figures can be found in the revised supplementary file (Supplementary Figure 4, Supplementary Figure 22).

[Figure Redacted]

Figure R1 (Supplementary Figure 22 in the revised manuscript) The Land area suitable for onshore wind turbine installation prior to accounting for forestation scenarios examined in this study. For each forestation scenario, the corresponding potential forestation areas were further excluded from the land area suitable for onshore wind turbine installation.

[Comment 2] The use of a neutral stability exponent (i.e., $1/7$) in the wind power law does not reflect the diverse land surfaces explored in the study. The study would benefit from using spatially explicit information that uses geography-specific exponents, at the minimum, for each of the 13 explored regions. See for example recent work reported here:

<https://www.sciencedirect.com/science/article/pii/S0306261923018056>

[Response] We agree that using a constant neutral stability exponent ($\alpha=1/7$) ignores its variations caused by change in surface roughness or atmospheric stability. In the revised manuscript, following the referee's suggestion, we derived a gridded map of the power law exponent (the Equation R2 see below) by using the gridded wind speed at 10 m and 100 m heights, averaged over the period 2000–2020 for each month, of the ERA5 climate reanalysis dataset with a spatial resolution of 0.25° .

$$\alpha = \frac{\ln\left(\frac{W_{100}}{W_{10}}\right)}{\ln\left(\frac{100}{10}\right)} \quad (R2)$$

where W_{100} and W_{10} represent the wind speeds (in the unit of m s^{-1}) at 100 m and 10 m height, respectively. The values of 10 and 100 in $\ln(\frac{100}{10})$ represent the height (in the unit of m).

The derived spatial maps of α for the 12 months are shown in Figure R2 below. Hence, in response to the referee's comment, we obtained the spatiotemporal variations of the power law exponent, similar to that reported by Yang et al., (2024). Then, we applied these gridded values of the power law exponent in the revised manuscript and updated all the wind energy calculations. We confirm that the revision made in the Methods section did not influence our major conclusions.

This revision was described in the revised manuscript (Lines 661–671).

[Figure Redacted]

Figure R2 (Supplementary Figure 27 in the revised manuscript) Global maps of the power law exponent (α) used in wind speed scaling for 12 months derived from the 2000–2020 average wind speed data from ERA5.

[Comment 3] The discussion would benefit from interpreting the results of the study with other recent papers published in this journal, mainly:

<https://www.nature.com/articles/s41467-025-57606-y>

<https://www.nature.com/articles/s41467-022-33279-9>

[Response] Following the referee’s suggestion, in the revised manuscript, we have expanded the discussion of how forestation influences near surface wind speed (Lines 247–251) and clarified our justification for partly ignoring nonlocal effects of forestation (Lines 320–330).

Lines 247–251:

“This result highlights the important impact of future forestation on surface wind speed through increasing land surface roughness and by altering regional-scale circulation driven by orography, land-sea contrasts, and thermally-driven circulation contrasts between the open land and forested area.”

Lines 320–330:

“Second, widespread forestation might alter atmospheric circulation patterns at mesoscales or larger, thereby affecting the wind speed of non-forestation areas. However, our results show that wind speed change in grid cells without forest change, i.e., the nonlocal effect, has been largely insignificant. Hence, in this study, our methodology focused on local surface wind effect of forestation and ignored the nonlocal effect. It should be noted, however, that the local surface wind effect of forestation simulated by ESMs reflects the well-mixed conditions of the lower atmosphere, and hence applies over the whole grid cell rather than being differentiated among vegetation types within it. It is thus reasonable to apply the simulated wind speed to wind installations in forestation grid cells that still contain some non-forest areas suitable for wind turbine installation.”

[Comment 4] The axis limits of figures included in Figure 1 can be problematic, as they distort the relationship between the presented values. I suggest that all graphs are reported using 0 as the starting point to preserve the point of reference among figures.

[Response] Thank you for your suggestion regarding the axis limits in Figure 1. We understand the concern about ensuring a consistent reference point across figures. Following your suggestion, we plot Figure 1 by using 0 as the starting point (Figure R3, see below). Compared to the original Figure 1, changing the starting point of the axis does not improve the tradeoff relationship between forestation-induced carbon sequestration and wind energy reduction. Hence, we conclude that the original Figure 1 did not distort the relationships.

Given this result, we think the axis settings in the original Figure 1 are appropriate, as they allow the relevant differences and relationships in the data to be more clearly presented.

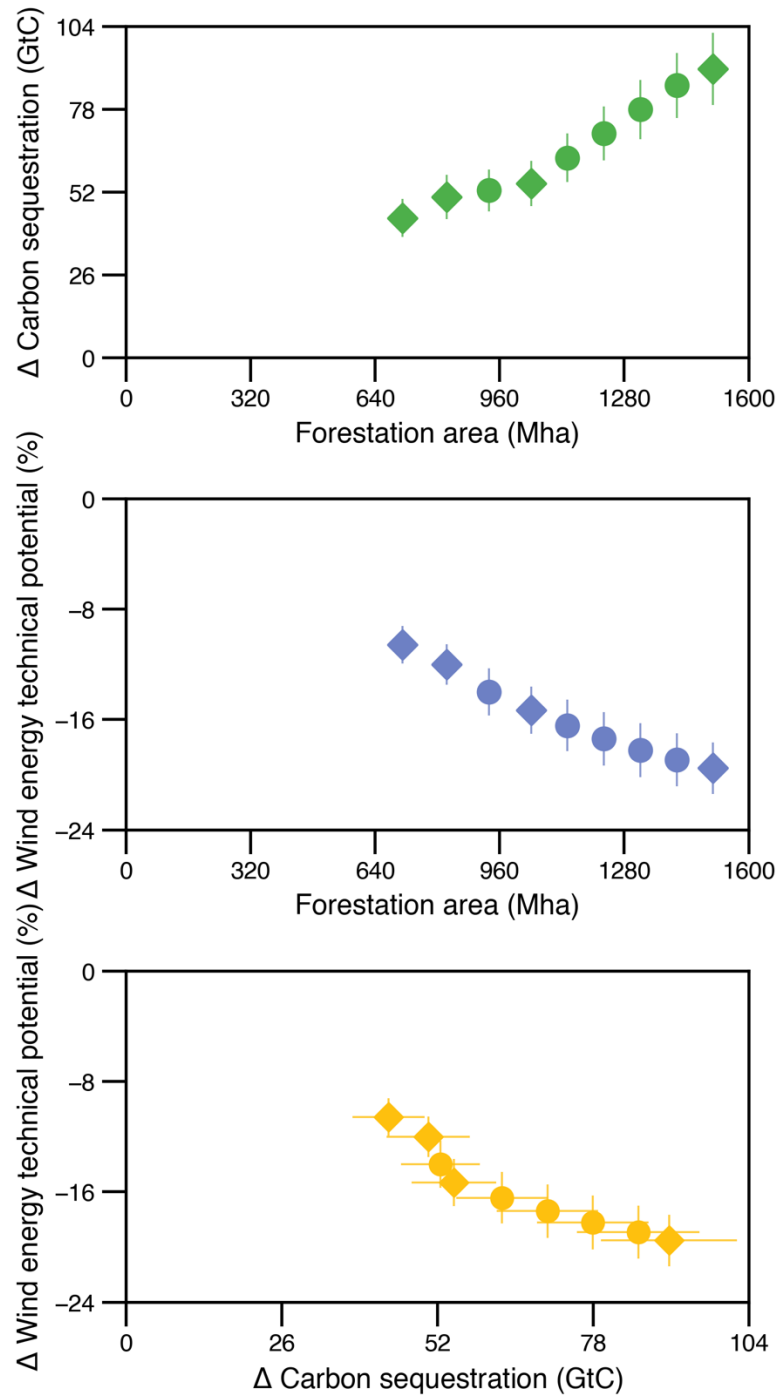


Figure R3 The same as the original Figure 1 in the main text but using 0 as the starting point in all panels.

[Comment 5] Line 26: The statement “by a factor of 3 to 5, to 2870–5806 GW, with wind energy contributing 36% of the projected global electricity demand” reports a range for the growth factor (i.e., 3 to 5) and capacity (2870-5806 GW), but only one number of projected contributions to global capacity (36%). It would also be reasonable to provide the range.

[Response] Thank you for highlighting this issue. We have added the range number in Lines 32–34 as below:

“Looking ahead, it is projected that by 2050 the global installed wind-energy capacity will increase by a factor of 3 to 5, to 2870–5806 GW, with wind energy contributing 18%–36% of the projected global electricity demand.”

[Comment 6] Panel f) of Figure 2 is color-coded to represent the 13 regions analyzed in the study. However, there is no legend in this figure describing which color corresponds to each region. It's impractical for the reader to visit Figure 4 to interpret the figure.

[Response] Thanks for your suggestion. We have added a legend showing the colors of different regions with their abbreviated names. Please refer to the revised Figure 2 in our manuscript.

[Comment 7] Panel b) of Figure 4 would benefit from text labels indicating a few of the 13 regions depicted at each bar (I understand they are color-coded to represent each region, but this forces the reader to contrast the result with the map while also not accounting for color blindness).

[Response] Thanks for your suggestion. We have added the abbreviations of region names in Panel b) of Figure 4 due to the space limitation. In the meanwhile, the abbreviations of region names have been put in brackets in the central map just after their full names. Please refer to Figure 4 in our revised manuscript (also shown as Figure R4 below).

[Figure Redacted]

Figure R4 (Figure 4 in the revised manuscript) Forestation-induced changes in the average capacity factor (CF) for current onshore wind installations for different regions. (a) The division of the globe into thirteen regions. (b) Total installed capacity of onshore wind installations in different regions. Panels (c) to (o) show the average CF of currently installed onshore wind turbines for different regions, for the case of no forestation (i.e., present day using the climate of 2000–2020) (yellow) and under the minimum (S1) (blue) and maximum (S4) (green) forestation scenarios. In addition, Panels (c) to (o) show the average CF of currently installed onshore wind turbines in different regions, for the case of T1 technical scenario (i.e., current technical scenario) (no texture), and under the T2 technical scenario (cross-hatched pattern) and T4 technical scenario (plus-hatched pattern). The results, based on the multi-model mean output, are shown as bar plots; the uncertainties (error bars) were derived by using the standard deviation of eight individual model outputs.

[Comment 8] The y-axis labels of the bar plots in Figure 4 have a typo, as currently, they read “CapIcity Factor” rather than “CapAcity Factor.”

[Response] We apologize for this typo. We have corrected it in Figure 4. Please see the updated Figure 4 in the revised manuscript.

[Comment 9] The use of capital letters across figure labels should be homogenized. For example, the labels in Figure 3 capitalize the first letter only (i.e., Capacity factor), while the bar plots in Figure 4 capitalize all starting letters (i.e., Capacity Factor).

[Response] We apologize for these typos. Now, we have homogenized figure labels of all figures by using consistently “Capacity factor”. Please see the updated Figure 4 in the revised manuscript.

[Comment 10] Equation 3 in Line 425 reports variables using W , but they are defined in the text using u . The nomenclature needs to be unified.

[Response] We apologize for these minor errors. We have corrected these texts. Now, we use “ W ” exclusively to define the wind speed in the texts (Lines 641–648).

[Comment 11] Figures S1 and S2 would benefit from using a color scale with a bigger contrast, as the current white-to-green scale works for the upper part of the scale but not as well for the bottom values.

[Response] Thanks for your suggestions. In the revised Supplementary Figures 1 and 2, we applied a color scale with increased contrast. In addition, the color bar was stretched at the lower end to enhance the visibility of low-change grid cells while preserving the relative differences among higher values. Please refer to Supplementary Figure 1 and Supplementary Figure 2 in our revised manuscript (shown also as Figure R5 and R6 below).

[Figure Redacted]

Figure R5 (Supplementary Figure 1 in the revised manuscript) Global forestation potential distribution under different scenarios.

[Figure Redacted]

Figure R6 (Supplementary Figure 2 in the revised manuscript) Spatial distribution of forestation-induced biomass carbon sequestration capacity under different scenarios.

[Comment 12] Figure S3 shows that under V1, the wind speed reduction is the minimum among the nine scenarios explored. Given this is an interpolation scenario between S2 and S3, what is the explanation for the less prolonged decrease? Also, why is it that even when wind speeds are increased under this scenario (in comparison to S2), its capacity factor is lower?

[Response] We appreciate the referee for pointing out this potential problem, and we sincerely apologize for the confusion it caused. This confusion arises because we calculated the average wind speed reduction in the forestation area only, rather than at global scale as for the capacity factor (panel b of Supplementary Figure 3).

We have corrected this by calculating the global mean wind speed change in Supplementary Figure 3 in our revised manuscript, ensuring the consistency between the two panels of this figure. After this correction the tradeoff relationships between forestation areas and wind speed is retained (Figure R7 below).

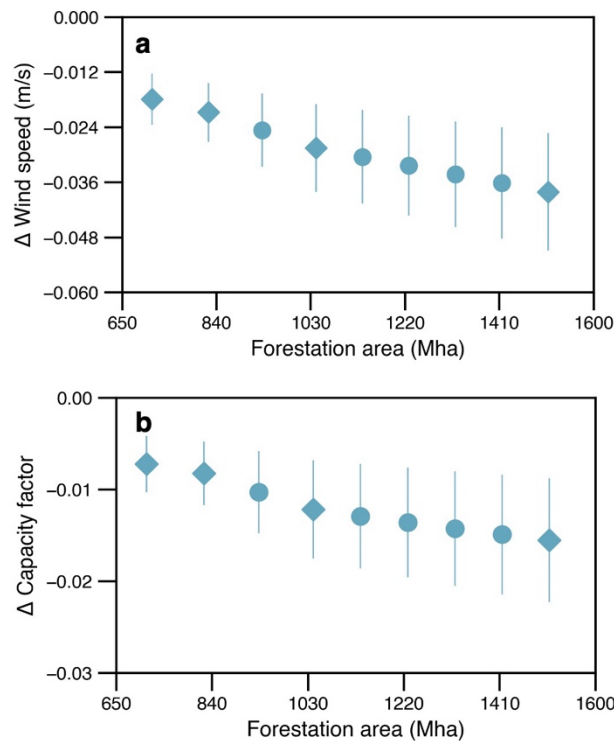


Figure R7 (Supplementary Figure 3 in the revised manuscript) Changes in global mean annual surface wind speed (10 m height) (a) and wind-energy capacity factor (b) following different forestation scenarios. The wind-energy capacity factor was calculated under the assumption that each pixel contains a wind turbine with current technological features (T1 scenario). The diamonds represent the four different forestation scenarios of S1–S4; the circles represent additional forestation scenarios

derived by interpolating S2 and S3, and S3 and S4. The vertical lines in (a) and (b) represent the standard deviations derived using the eight individual ESMs in LUMIP.

Reviewer #3 (Remarks to the Author):

[General Comments]

In this paper, the authors examine the tradeoffs between forestation and wind energy expansion since future forestation policies will likely alter surface wind speeds. The authors utilize projections from the Land use inter comparison project (LUMIP) to assess the difference in surface wind speeds on account of additional forestation. The change in the wind speed is then used to calculate the effect on wind energy potential. Previous studies have quantified and established the link between forest cover and surface wind speeds. This is a first study that actually links the same to current wind energy potential. The authors acknowledge that they are using coarse land use projections (0.25 degrees) that they are using to determine local effects on wind energy potential. Regardless, I found this to be a generally well written paper with some interesting quantifications. However, given the limitations in the methods discussed above, I had some questions and suggestions to the authors so that the findings can be explained in more concrete terms for the general audience. I therefore recommend major revisions.

[Response] We sincerely thank the referee's efforts in reviewing our paper and the constructive comments which help us greatly improve the manuscript quality. Please find below our point-by-point responses to your comments.

[Comment 1] Definitions of forestation and effect of protecting current forest vs additional forest plantations- The forestation scenarios are described in a lot of detail which is appreciated; however forestation is a combination of protecting current forests (letting them grow to maturity) and planting new forests. What effect would each of these have on the surface wind speeds and therefore the wind energy potential? I believe this is an important point that the authors don't address. I was interested in understanding whether forest protection i.e. growth in the biomass stock itself would affect the surface winds or would a bigger effect be as a result of new plantations (which tend to be managed monocultures). It seems from Supplementary Figure 5 (also see point 2 below) that forestation and wind energy efficiency is highest close to existing forests with large biomass values (These forests are more likely to be protected rather than replanted). Related to these points, it would be helpful if the authors can discuss the pathways from forestation to surface roughness to wind energy effects by summarizing to what extent, the following factors play a role-

- A. Growth in biomass stock (above and belowground)
- B. Tree heights and biomes
- C. Land effects through new plantations

[Response] Thanks to your insightful comments. Our responses are organized into the following three components:

1. Clarification of the definition of ‘forestation’

The referee’s comments encouraged us to revisit our definition of forestation through performing an extensive review of the usage of relevant terminologies, namely, forestation, afforestation, reforestation, and forest restoration, in the current literature. We reviewed about 40 usages of these terms in mainly Science and Nature family journals and reports from international organizations (e.g., FAO, UNFCCC) and summarized them in Supplementary Table 1.

Forestation in our original manuscript was defined as including afforestation, reforestation and forest restoration. Our literature review reveals that, (1) ‘afforestation’ was defined by most literature as ‘the establishment of forests, naturally or artificially, over lands not recently forested’ (Chazdon et al., 2016; Song et al., 2023), (2) ‘reforestation’ was defined as ‘the establishment of forests over lands that have been historically forested’(Anderegg et al., 2025; Chazdon et al., 2016; Song et al., 2023). Hence, both afforestation and reforestation stress land conversion from a ‘unforested’ to a ‘forested’ state and do not necessarily entail monoculture plantations as the referee likely implied.

We continue to find that most literature defined ‘forestation’ as including both afforestation and reforestation. We hence adopted this definition and excluded forest restoration, which includes carbon enhancement in existing forests, in our revised manuscript. The reasons are explained in the two paragraphs below.

We excluded forest restoration from our revised manuscript mainly because our literature review reveals that that ‘forest restoration’ carries multiple meanings — including (a) recovering lost forest (i.e., reforestation) (Cook-Patton et al., 2021), (b) increasing tree cover regardless of land-use history (thus including afforestation) (Hoek van Dijke et al., 2022), (c) restoring degraded forests to their original undisturbed state (Stanturf et al., 2015), and (d) enhancing ecological integrity and diversity of an existing forest by recovering native species composition (Chazdon et al., 2016). Hence, ‘protecting current forests and letting them grow to maturity’ as indicated by the referee is part of ‘forest restoration’ and is now explicitly excluded from our scope.

Another reason to exclude protecting current forests from our study scope is because (a) Although several studies explore the potential of carbon removal by increasing

carbon stock in existing forests—primarily through halting wood harvest (Erb et al., 2018; Roebroek et al., 2023)—this approach conflicts with rising global wood demand (Mishra et al., 2022; Peng et al., 2023), making widespread harvest cessation an unlikely large-scale solution for enhancing terrestrial carbon storage. (b) Moreover, site-level quantitative studies on how enhancing carbon stocks of existing forests (i.e., post-harvest recovery) affects local surface wind speed are scarce, and idealized simulations of large-scale wood harvest impacts on wind speed remain absent (see the introduction about LUMIP below), due in part to inadequate representation of harvest processes in current models (De Hertog et al., 2023; Shu et al., 2025).

Based on these considerations, our revised manuscript excludes carbon-stock enhancement in existing forests and focuses solely on forestation, defined as the expansion of forest area. We further emphasize that forestation here does not imply unmanaged, single-species, or unguided planting; rather, we strongly advocate careful, science-based planning and management to ensure positive ecological outcomes and prevent unintended long-term harm.

The revisions in response to the referee’s comment includes: (a) Supplementary Table 1 was added to summarize our review of ~40 usages of the relevant terms; (b) ‘forestation’ is now clearly defined in the revised main text (Lines 417–436); (c) Three paragraphs were added in the Methods to explain how we arrive at this definition and why enhancing carbon stock in existing forests were excluded from our scope (Lines 417–445 in the revised Methods).

2. Potential impacts of forest protection on carbon sequestration and wind speed

We explained above why forest protection was excluded from our study, although we agree with the referee that forest protection will influence wind speed. In response to the referee’s comment, in addition to our revision in the Methods mentioned above, the following texts were further added in Discussion section (Lines 308–320):

In addition, the biophysical effects of forest protection (allowing existing forests to restore to a mature or old-growth state) are not included in our study. Basic forest ecophysiology tells us that forest protection, or releasing currently managed forest by halting rotational wood harvest, will allow tree height, leaf area index and biomass to recover (Zhou et al., 2013), which will further increase surface roughness (Nakai et al., 2008) and thus reduce near-surface wind speed. However,

these biogeophysical effects can be incremental compared to the establishment of new forests. Indeed, a few rare modeling studies investigating the biogeophysical effects of current global wood harvest showed very small, almost negligible impacts on surface roughness (Shu et al., 2025) or land surface temperature (De Hertog et al., 2023), compared to the effects of deforestation. Moreover, although several studies explored the potential of carbon removal through halting wood harvest (Erb et al., 2018; Roebroek et al., 2023), this approach conflicts with rising global wood demand (Mishra et al., 2022; Peng et al., 2023), making widespread harvest cessation an unlikely large-scale solution for enhancing terrestrial carbon storage.

3. Forestation and wind energy efficiency close to existing forests with large biomass values

The referee mentions that ‘*It seems from Supplementary Figure 5 (also see point 2 below) that forestation and wind energy efficiency is highest close to existing forests with large biomass values (These forests are more likely to be protected rather than replanted).*’. As is explained above, we excluded forest protection from our analysis and hence the high wind energy efficiency close to existing forests with large biomass values is not because of forest protection. Instead, it can be attributed to both a high forest biomass carbon density, if forests are to be established, and relative low wind energy technical potential loss in these regions. As this comment is very close to the Comment 2 below, please also refer to our responses there.

[Comment 2] Forestation efficiency map- The forestation efficiency map (Supplementary Figure 5) is a bit confusing and perhaps misleading. Aren’t these values more negative close to old growth forests (e.g., Amazon, West Africa, Indonesia). This is mainly because these forests already sequester a lot of carbon regardless of wind energy projects? Perhaps you should compare a “ratio” of current to future carbon compared to the delta in wind energy potential to make this accurate? That would also lead to a very different picture of the efficiency number.

[Response] We are sorry for the potential confusion in the calculation of forestation efficiency. The forestation efficiency was originally defined as the ratio of the forestation-induced wind-energy power loss (in kW) to carbon sequestration potential (Lines 522-526 of the original manuscript). In the revised manuscript two modifications are made: (1) following the Comment 1 of Referee #1, we now use ‘technical potential’ of wind energy (in Wh) rather than the power value. (2) the forestation efficiency is calculated as the arctangent of the ratio, of the wind-energy technical potential loss (in the unit of TWh, including both effects of the loss of suitable wind installation area and the reduction in capacity factor due to wind speed

decrease), to biomass carbon sequestration potential following forestation (in the unit of TgC) in each pixel. To make this very clear, we added a new equation, which is Equation 7 (reproduced below as Equation R3), in the revised manuscript to describe its calculation.

$$\text{Forestation efficiency } (\alpha) = \text{ArcTan} \left(\frac{\Delta \text{ wind energy technical potential}}{\Delta \text{ carbon sequestration}} \right) \quad (\text{R3})$$

In this calculation, a higher forestation efficiency value (α is closer to 0) corresponds to a smaller decrease in wind energy technical potential for a given carbon sequestration potential by forestation, whereas a lower value (more negative) indicates the reverse.

Indeed, tropical regions (the Indonesia, Amazon, West Africa, and South Asia) tend to exhibit higher forestation efficiency values (closer to 0), primarily because newly established forests in these regions have higher potential carbon density (Supplementary Figure 28 in revised manuscript), rather than being related to the carbon already sequestered in existing old forests in these regions. Hence, it is not related to forest protection (related to Comment 1 above).

In addition, we suspect that some of the confusion may come from the colorbar. To avoid potential confusion, the meaning of forestation efficiency with the color are now clearly explained in the caption of the revised Supplementary Figure 5 as: “*A less negative number (darker color) indicates greater carbon sequestration with a corresponding less reduction in wind energy following forestation and hence greater forestation efficiency.*”

The detailed revisions can be found in Supplementary Figure 5 and Lines 750–762 of the revised manuscript.

Specifically, the change in biomass carbon sequestration potential following forestation was calculated by multiplying forestation area (m²; Supplementary Figure 1) and potential forest carbon density (gC/m²; Supplementary Figure 28). The wind energy loss in forestation areas was calculated by multiplying the forestation fraction and the response of wind speed change to forestation (β_1 in Equation 2). Regional forestation efficiency (α) was quantified as the arctangent of the ratio of the total wind-energy technical potential loss (in the unit of PWh) to total biomass carbon sequestration potential (in the unit of GtC) for each region (Equation 7). To avoid the regional forestation efficiency obscure subnational differences, we additionally present its spatial difference at pixel scale by calculating the arctangent of the ratio of the wind-energy technical potential loss (in

the unit of TWh) to biomass carbon sequestration potential (in the unit of TgC) in each pixel (Supplementary Figure 5).

[Comment 3] Methodology of calculating wind energy potential using wind speeds- One of the biggest value-adds of this study (in my opinion) is the translation of wind speed effects to the actual wind energy potential by considering capacity factors. Authors mention the usage of data for a specific 1 MW wind turbine with some sensitivity analysis conducted for another type of turbine in the supplement. However, can the authors elaborate on this point? What is the current type of turbine prevalent on wind farms across different regions (It would be helpful to see the type of turbine currently prevalent or at least characteristics of wind turbines by pixel)? Are there alternatives that are available to this? Moreover, it seems there are differences in the rotor heights of the older and newer turbines (older being between 80-120 m and newer being between 40-80). Can the authors consider a sensitivity analysis to this rotor height as well?

[Response] We agree with the referee that our results are potentially sensitive to the wind turbine technical feature (rated power and rotor height) used to assess wind energy change following forestation, and that it is highly desirable to test such sensitivity.

As the referee pointed out, ideally, the technical feature of the locally prevalent (or dominant) wind turbine type, specific to each pixel, should be used. However, to the best of our knowledge, such a global gridded map of the dominant wind turbine type is not yet available. Previous studies of global wind energy evaluation hence exclusively used a fixed wind turbine type across all pixels (Karnauskas et al., 2018; Lei et al., 2023) and we followed such a practice. Nonetheless, inspired by the referee's comment, in the revised manuscript, as an alternative, we used the technical feature that is representative of the mean rated power of global current wind fleet (see Lines 577–680 of the revised Methods, also briefly explained below), instead of using an idealized 1MW wind turbine, to enhance the degree of realism of our study.

A recent study by Jung and Schindler (2022) reviewed the availability of site-level wind turbine information and the authors succeeded in compiling an onshore wind turbine fleet dataset by mainly including those in the United States, Germany, and Canada, which together accounted for 26.6% of global onshore installed capacity in 2020. They found an average rated power of 1.69 MW of these countries, which closely matches those of turbines in China (1.7 MW) but also worldwide (1.71 MW). Jung and Schindler (2022) classified wind turbines into 29 power and height classes (PHC). The technical features of currently installed global onshore wind turbines

and their potential evolution in the future, can then be described by using different shares of the wind turbine PHC classes (Jung and Schindler, 2022). They defined the shares of different PHC wind turbine classes (denoted as T1) which can represent the mean Pr value (about 1.7 WM) of the currently installed global wind turbine fleet.

Our revised manuscript mainly used this T1 scenario to evaluate the change in technical potential of wind energy following forestation. Based on this, we updated our main results (Figure 1, Figure 2 and Figure 3) and corresponding text (Lines 80–186). The updated results do not change our main conclusion that a notable trade-off between wind energy and carbon sequestration exists across a range of potential forestation areas.

We further analyzed the forestation-induced wind-energy technical potential reduction and evaluated forestation efficiency by using all 29 PHC classes of wind turbines, in order to investigate the sensitivity of our results to changes in rated power and rotor height of wind turbines. The results showed that the trade-off relationship between wind energy and forestation-induced carbon sequestration potential (Figure R8), and the ranking of different regions in terms of forestation efficiency (Figure R9), largely holds irrespective of the technical feature of the wind turbine used. The trade-off strengths increase with higher rated power and greater rotor heights. Larger turbines, with higher rated power and taller rotors height, tend to generate more wind energy under typical operating conditions. However, they are also more sensitive to reductions in wind speed. A given decrease in wind speed therefore translates into a relatively greater loss of wind energy generation (rated power $\times$ capacity factor) for larger turbines with higher rated power and taller rotors height, which intensifies the trade-offs.

The method and results of this sensitivity test have been included in the Method section (Lines 763–768) and the Supplementary File (Supplementary Figures 29, 30) of the revised manuscript.

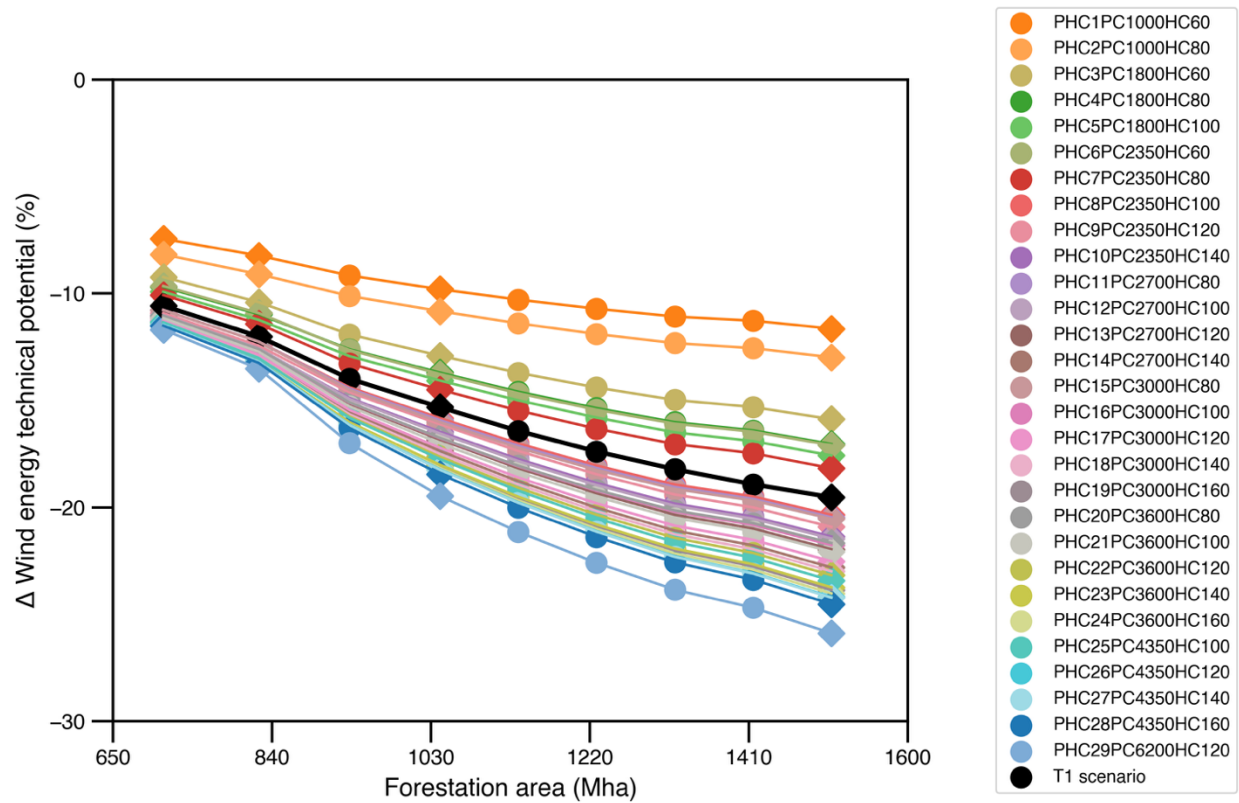


Figure R8 (Supplementary Figure 29 in the revised manuscript) Changes in global wind-energy technical potential following different forestation scenarios across the wind turbine types with different hub height. PHC in the legend refers to 29 power–height wind turbine combinations. PC denotes the rated power, and HC represents the rotor hub height.

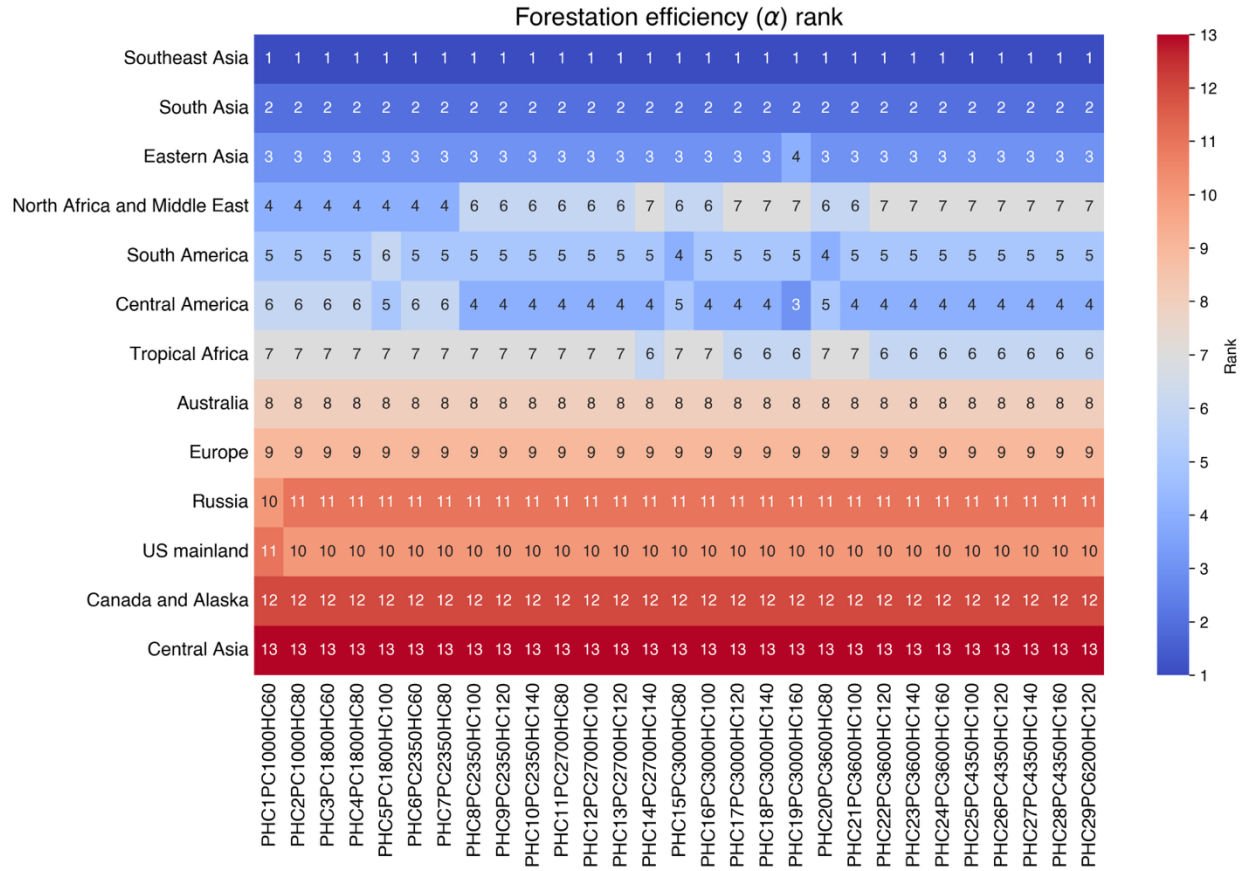


Figure R9 (Supplementary Figure 30 in the revised manuscript) The forestation efficiency ranks under the maximum forestation scenario of S4 across the wind turbine types with different hub height. PHC in the legend refers to 29 power–height wind turbine combinations. PC denotes the rated power, and HC represents the rotor hub height.

[Comment 4] Quantification of outcomes- While I appreciate the outcomes as presented by the authors, I think it raised a few questions. Based on point no 3. above in terms of the turbine and rotor height diversity, what kind of capacity factor improvement would be expected from the current onshore wind farms to maintain their current potential even under a forestation scenario? This seems like an important question that a reader of this study would think of and I believe the authors would have the data to quantify the same. I believe the answer would be regionally specific and would be extremely useful (especially to modelers current working on future wind energy potential).

[Response] We thank the referee for this excellent comment which enabled us to strengthen the policy relevance of our work. Linked to our response to the preceding comment (Comment 3 above), in the revised manuscript, we additionally used two scenarios of potential future development of global wind turbine fleet developed by Jung and Schindler (2022), namely the T2 (pessimistic) and T4 (optimistic) scenarios, respectively, to examine whether the forestation-induced reduction in capacity factor (CF) and technical potential of wind energy can be compensated for by the technological development of global wind turbine fleet.

Addressing this comment with full details led us to create a new Result section entitled “*Influence of potential technological improvements of the global wind turbine fleet*”, where we examined how potential technical advancement in global wind turbine fleet can influence CF at current global wind installation sites, and the technical potential of wind energy under S1 and S4 forestation scenarios. Figure 4 is also revised to accommodate the findings of this examination.

In summary, our results show that, when the whole globe is considered, both the pessimistic (T2) and optimistic (T4) wind turbine technological development scenarios can enable the post-forestation mean CF of global onshore wind installation sites or global total wind-energy technical potential to recover to or exceed the pre-forestation level, irrespective of which forestation scenario.

At the regional level, under the optimistic (T4) wind development scenario, all thirteen regions except South America, can recover to or exceed the CF at onshore wind installation sites and achieve the current wind-energy technical potential, irrespective of which forestation scenario (Figure 4 in revised manuscript). Under the pessimistic (T2) scenario, however, the CF of onshore wind installation sites and wind-energy technical potential in South America can not recover to pre-forestation level, irrespective of which forestation scenario. Under the same T2 scenario, regions of Canada and Alaska, Eastern Asia, North Africa and Middle East and South Asia can exceed the pre-forestation level, irrespective of which forestation

scenario; Europe and Southeast Asia can only recover to pre-forestation level with S1 forestation scenario.

The methods and results with details can be found in Method (Lines: 577–680), and Results (Lines: 187–230) sections of the revised manuscript. The Figure 4 in revised manuscript was also updated thanks to addressing this comment.

[Comment 5] Discussion of results for regions (African continent as an example)- In several places, authors have calculated average CF effects for the African continent as a whole (Figure 2). But this is a bit confusing. Firstly, current installed wind turbines in the continent (based on Figure 3), seem to be largely in the south, but the current dense forests are in West Africa. It also seems that the forestation induced wind energy loss is going to be more prominent in southern Africa rather than western Africa (Supplementary Figure 4). Given these points, can the authors consider amending Figure 4 to include an analysis like Supplementary Figure 4 to make sure these subnational differences are not glossed over?

[Response] We thank the referee for pointing out the potential confusion and apologize for this. Two types of analyses were performed in our study: (1) The analysis of trade-offs between technical potential loss in wind energy and forestation-induced carbon sink potential (Figure 1 and Figure 2), both for the globe and for different regions. This analysis was based on examining the changes in ‘technical potential’ of wind power and the carbon sink potentials of newly established forests, which are both integrated over all grid cells (or areas) of a given region. (2) how forestation will impact the capacity factor (CF) of currently installed wind turbines or turbine installation sites (Figure 3 and Figure 4). Hence, Figure 4 was derived by averaging the changes of the wind turbine installation sites (as shown in Figure 3) within different regions, which is now clearly stated in the caption of the revised Figure 4.

Following the referee’s Comment 4 above, we further investigated how technical advances in the global wind turbine fleet can compensate for forestation-induced reduction in the CF of currently wind installation sites (see the revised Figure 4 and our response to Comment 4 above).

We now return to the questions raised by the referee and provide further clarifications. The referee raised that “*current installed wind turbines in the continent (based on Figure 3), seem to be largely in the south, but the current dense forests are in West Africa.*” Indeed, the referee is correct by pointing out that the currently installed wind turbines are mainly in southern Africa, and more specifically, in southern coastal area of South Africa (Figure R10 below, a zoom in South Africa

out of maintext Figure 3 in the revised manuscript), and that the current dense forests are in tropical Africa (Supplementary Figure 14 in the revised manuscript), and the area for forestation are mostly in western part of southern Africa but also in the southern coastal area of South Africa (Figure R11 below, a zoomed-in Southern Africa out of Supplementary Figure 1 in the revised manuscript). But these facts are not in contradiction. Given that we are mainly investigating the local effect of surface wind change caused by forestation, and substantial forestation occurs in both scenarios of S1 and S4 in the area close to the sea in southern Africa where the wind turbine sites are found, it is reasonable that these turbine sites see clear decrease in CF (Figure R10). Hence, the fact that clear decrease in CF in south African turbine sites driven by substantial forestation there, is not in contradiction with the fact that dense forests are found in western Africa, because it is not the location of current forest, but the increase in forest area (i.e., forestation), that determines the CF change.

The referee also raised that “*It also seems that the forestation induced wind energy loss is going to be more prominent in southern Africa rather than western Africa (Supplementary Figure 4)*”. We see no contradiction here either based on the reason explained above, given that forestation areas are mostly in southern Africa as is shown in Supplementary Figure 1 (a zoom in southern Africa is provided in Figure R11 below).

To illustrate these points above better, we provide below two figures (Figure R10, R11) by focusing on Southern Africa only. Figure R10 shows a zoomed-in view of the capacity factor (CF) and its changes for currently installed onshore wind turbines in Southern Africa, corresponding to Figure 3 in the revised manuscript. Figure R11 shows a zoomed-in view of Forestation potential in Southern Africa, corresponding to Supplementary Figure 1 in the revised manuscript.

[Figure Redacted]

Figure R10 Capacity factor (CF) for currently installed onshore wind turbines in Southern Africa with current technical features **(a)** (T1 scenario) and its change in response to S1**(b)** and S4 **(c)** forestation scenarios.

[Figure Redacted]

Figure R11 Forestation potential distribution in Southern Africa under S1 and S4 forestation scenarios

In addition, we suspect that part of the reason for the referee's confusion is that the color bars of original Figure 3b and 3c make it difficult to distinguish places that have a wind turbine but with little change in CF, from the places without any wind

turbine. We hence improved the colorbars of Figure 3b and 3c in the revised manuscript.

The referee also suggest “amending Figure 4 to include an analysis like Supplementary Figure 4”. As is explained above, Figure 4 was derived by averaging the changes of the wind turbine installation sites as shown in Figure 3 and hence Figure 3 shows clearly the spatial details of Figure 4. Hence we don’t see any further need for new figures but if we misunderstood the referee, please let us know and we are happy to make further improvements.

[Comment 6] Relating wind energy potential to carbon avoidance- While the metric presented Figure 2 (panel d) is really useful, what are the implications of the reduction in wind energy potential to carbon emissions. After all, wind energy is attractive as a form of carbon emission avoidance. Would a plot looking at forest carbon increase vs carbon avoidance (from wind) look similar? This should be discussed.

[Response] We thank the referee for yet another excellent comment which helps strengthen the policy relevance of our work.

Carbon emission avoidance from wind energy refers to the CO₂ emissions avoided by assuming that the same amount of electricity were generated by fossil fuel rather than by wind (Pryor et al., 2020; Sims et al., 2003). For its calculation, we assumed that wind-generated electricity displaces a fossil-fuel-based electricity-generation source with an average emission intensity of 600 g CO₂-equivalent per kWh, according to the estimations by Pryor et al. (2020) and the Global Wind Energy Outlook report (Global Wind Energy Council, 2016). Subsequently, the total carbon emissions avoidance by global wind turbines can be calculated by multiplying the annual electricity generated by the global wind installations (the total rated power of all wind turbines $\times$ CF $\times$ 8760 hours), the avoided CO₂ emission intensity (600 gCO₂ per kWh), and the typical operational life cycle of a wind turbine, which is assumed being 30 years(Pryor et al., 2020; Wiser et al., 2011).

Our study showed that forestation will reduce global wind energy potential by reducing surface wind speed, thus leading to the reductions in the carbon emission avoidance by wind energy. To evaluate the potential gain in carbon removal by forestation versus the loss in avoided carbon emissions due to wind energy potential loss, their comparison should be based on a common, meaningful time frame. The typical operational lifetime of a wind turbine is 30 years, whereas it the time required for newly established forests to reach maturity and reach the carbon density assumed

in this study can several decades to even a century. Given that we are aiming for a rough comparison, we assume on average, the newly established forests can reach their mature carbon stocks in about 60 years globally, corresponding to two life cycles of global wind turbines.

Considering that forestation is to occur in the future, which is accompanied with inevitable expansion of global wind installation, we made the comparisons described above not only for the current global wind installation capacity, but also for several future scenarios of wind energy expansion. The current global wind installation capacity accounts for 6.2% of global electricity generation (IEA, 2020). For future scenarios, we consider: (a) a moderate expansion scenario in which the current installation capacity will be doubled, (b) a scenario achieving the COP28 renewable energy tripling target (COP 28, 2023), and (c) a high-ambition scenario according to the International Energy Agency (IEA) Net Zero 2050 roadmap (IEA), where an elevenfold increase over current capacity will contribute approximately 30.0% of global electricity generation then (IEA, 2021).

Our results showed that the reduction in CO₂ emission avoidance resulting from forestation-induced wind energy loss can partly counteract the CO₂ sequestered by forestation, depending on the global wind installation capacity (Table R1 below). Although the reduction in avoided CO₂ emissions remains negligible for current wind turbine installation capacity, it becomes substantial when the wind energy generation will contribute approximately 30.0% of global electricity generation, potentially offsetting 20% of CO₂ sequestered by forestation.

Table R1 (Supplementary Table 4 in the revised manuscript) The comparison between biomass carbon sequestration and the reduction in CO₂ emission avoidance caused by forestation. The reduction in avoided CO₂ emission was calculated based on several scenarios of global wind installation capacity (for details, refer to Supplementary Method).

	S1 Forestation Scenario	S4 Forestation Scenario
Forest carbon sequestration (GtCO₂)	160.7	332.5
Reduction in avoided CO₂ emissions (GtCO₂) with the current global wind installation capacity	-2.8	-6.2

Reduction in avoided CO₂ emissions (GtCO₂) with 2 × Current global wind installation capacity	-5.6	-12.4
Reduction in avoided CO₂ emissions (GtCO₂) with 3 ×Current global wind installation capacity	-8.4	-18.6
Reduction in avoided CO₂ emissions (GtCO₂) with 11 ×Current global wind installation capacity	-30.8	-68.2

Several uncertainties, however, exist for the comparisons made above. First, a single and constant factor of avoided CO₂ emission per kWh of wind-generated electricity was used, which can not account for technology development or changes in the regional distribution of wind installation capacity in the future. Second, we assumed it takes on average 60 years for the newly established forests to reach mature, which has great uncertainty. Hence, the results in Table R1 should be considered very preliminary and interpreted with caution.

The methods, results and discussions have been added in Supplementary Method (Lines 29–61 in Supplementary information file), Supplementary Table 4 and discussion section (Lines 268–279) in the revised manuscript.

[Comment 7] Implications for future wind energy potential- In the abstract and the discussion, the authors also say that “. Large-scale forestation in Europe and US mainland, however, will diminish future wind energy potential”. However, this point is not really discussed in this manuscript which seems concerned with current wind energy potential, right? Can the authors elaborate on this point?

[Response] We agree with the referee that our analysis provides an estimate of how potential forestation could influence wind resources based on present-day climatological conditions and hence the word “future” is inappropriate. Following the Comment 1 of Referee #1, we now investigate the ‘technical potential’ of wind energy which has greater policy relevance than the wind power (in kW) previously used in our original manuscript.

Hence, following your suggestions, we change “*future wind energy potential*” to “*wind energy technical potential*” in Abstract (Lines 13–15). We also discussed the implications for future wind energy potential in different countries (Lines 279–290 in revised manuscript).

[Comment 8] Implications for IAM and earth system modelers – In the discussion, the authors should also add some key takeaways for other modelers. I can see a couple of low hanging fruit takeaways for other modeling teams-

A. For integrated assessment modelers- this analysis suggests that they should consider reductions in wind capacity factors when modelling forestation scenarios (Once again differentiating between protecting existing forests vs planting new forests). IAMs already rely on aggressive forestation scenarios as climate mitigation options.

B. For earth system modelers- This would be a good opportunity to explore coupled effects with climate and land use. An interesting next step would perhaps be to understand the effect of wind energy expansion on forests as well.

[Response] Thank you for this valuable suggestion. We agree that it is important to highlight the broader implications of our findings for both integrated assessment modelers (IAMs) and earth system modelers (ESMs). Following your comments, the following paragraph was added in the revised Discussion (Lines 290–302):

The intricate but critical interaction whereby forestation reduces wind energy potential and, in turn, diminished wind generation partly offsets forestation's carbon-sequestration benefit arises from complex Earth-system feedback processes that must be incorporated into climate mitigation policy. Integrated Assessment Models (IAMs), which simulate energy–economy–land–climate interactions to design pathways that limit warming well below 2 °C, consistently project both massive forestation (500–1000 Mha) and a roughly tenfold increase in wind capacity—scales comparable to those examined here—as essential measures. Yet, to our knowledge, current IAMs do not account for the negative forestation–wind interaction identified in this study. Our findings therefore underscore the urgent need to enhance IAMs by including such trade-offs and synergies¹, potentially through better representation of Earth-system feedbacks, to improve the reliability of future mitigation pathways. Concurrently, future Earth-system model research should explore not only the impacts of land-based mitigation on wind resources but also the reciprocal effects of large-scale wind deployment on forest dynamics and surface–atmosphere exchanges.

Reviewer #4 (Remarks to the Author):

[General Comments] The study titled “Regional priorities in implementing forestation and wind energy as climate solutions in facing their trade-offs” offers a fresh perspective on the trade-off between global forest restoration as a climate mitigation strategy and the development of green wind energy. However, there are three minor points that should be clarified before the paper is published.

[Response] We appreciate the time and effort that the referee has dedicated to the constructive comments that have helped us improve the manuscript quality.

[Comment 1] First, in the methods section, the authors assess the influence of reforestation on local surface wind changes at a height of 10 meters. However, to my knowledge, wind turbine blades are positioned well above 10 meters, and they harness wind energy from much higher altitudes. Furthermore, forest canopy heights in many global regions exceed 10 meters (Lang et al., 2023). I recommend that the authors refine their analysis based on regional mature forest canopy heights, at the biome or ecoregion level, which can often reach 20, 30 meters and even higher. This additional analysis would provide a more accurate quantification of the trade-off between reforestation and wind energy generation.

[Response] We thank the referee for this thought-provoking comment. Our short answer is that, (1) we used the wind speed at the rotor height, and following the suggestion of the Comment 3 of Referee #3, we tested and confirmed the robustness of our results to different rotor heights (from 60 meters to 160 meters, Lines 763–768 in the revised manuscript), and (2) following the Comment 1 of Referee #1 (for details, please refer to our responses there), we used the ‘technical potential’ to quantify the changes in wind energy following forestation. The use of ‘technical potential’ excludes placing wind turbines over forests, and therefore minimized the influences of forest canopy height on our conclusion through their influences on wind speed. Our detailed responses are below.

In this study, we derived the wind speed at rotor height by interpolating from the wind speed at 10-m height as retrieved from the simulations using atmospheric models, a component of earth system models (ESMs). The 10-m height is strictly based on the ground surface. Within the atmospheric model, the wind speed at 10 m height is calculated by interpolating from the model’s lowest atmospheric level (typically located about 50 m above the surface) to 10 m based on the Monin–Obukhov similarity theory. This theory describes how wind speed (and other surface-layer variables) vary with height in the atmospheric surface layer, depending on surface roughness, friction and stability conditions.

The calculation of surface wind speed in ESMs makes sense because (a) at the spatial scale where atmospheric models operate (typically fifty to a few hundred kilometers), it can be fairly considered that the air near the land surface, or in the lower atmosphere, is well mixed so that they share a single wind speed; (b) the computation constraint of these models does not allow resolving atmospheric physical processes at a very fine spatial scale that account for turbulent eddies or local topographic influences. Hence, it is essential to note that atmospheric models provide simulated wind speed for ‘the whole grid cell’ but not separate wind speeds for different vegetation types within the grid cell.

More specifically, we derived the wind speed at the rotor height through the 10-m wind speed by applying the wind profile scaling component α (Lines 661–671 of the revised manuscript). This approach has been widely adopted in previous studies, as it provides a more realistic representation of the wind conditions experienced by wind turbines (Jung et al., 2022; Lei et al., 2023). A constant value of α was used in our original manuscript but following the Comment 2 of Referee #1, we now use a spatially explicit map of α derived by using the ERA5 reanalysis data (Lines 661–671 of the revised manuscript), which accounts for the influences of current vegetation distribution on the vertical profile of wind speed. Therefore, in this approach, the forest canopy effect on the wind profile was implicitly accounted for. Last, given that we now exclude the installation of wind turbines over forest canopy when evaluating the ‘technical potential’ of wind energy, an explicit consideration of the influence of forest canopy height, to adjust wind speed over forest canopies per se, is no longer necessary.

The reasoning above is briefly summarized in the Discussion section of the revised manuscript (Lines 325–329).

[Comment 2] Second, the authors use the term "forestation" to describe forest restoration activities, including both afforestation and natural forest regeneration. However, the appropriateness of afforestation as a natural climate solution remains debated. Converting land that is not naturally suitable for forests may cause unintended long-term ecological consequences. Therefore, I suggest that the authors exclude afforestation from the modeling and analysis to focus solely on the restoration of naturally forested ecosystems.

[Response] We appreciate the referee’s thoughtful comment. Referee #3 raised a comment (the Comment 1 of Referee #3) which is closely related to this one. Hence we kindly invite the referee to read our response there, but for the referee’s convenience, we summarize our response there as below:

- (1) We reviewed about 40 literature to provide clear definitions for related terms, including forestation, afforestation, reforestation and forest restoration.
- (2) Most studies investigating forest expansion as part of nature-based climate solutions considered both afforestation and reforestation, which is collectively named as ‘forestation’ in our revised manuscript. Hence afforestation remained in our research scope. We think afforestation per se is not a problem but we agree with the referee that the surrounding misunderstandings and sustainability risks need to be clarified (see below).
- (3) The usage of ‘forest restoration’ in literature carries multiple meanings including but not limited to restoring degraded forest to its original, undisturbed state. As we excluded the restoration of degraded forest in our study, we avoid using ‘forest restoration’ in our research scope in the revised manuscript.

Besides the points above, our responses to your specific comments, which are non-overlapping ones with those in Comment 1 of Referee #3, are as below:

- (1) The referee mentioned that ‘forest restoration activities, including both afforestation and natural forest regeneration’. We would like to point out that natural forest regeneration should be regarded as one possible implementation pathway of forestation (more specifically, reforestation), but it is not conceptually equivalent to either reforestation or afforestation or forest restoration. In fact, natural forest regeneration typically refers to the natural establishment of tree seedlings on land, either through local seed bank or seed dispersion, rather than through human planting. Although natural forest regeneration can be used to describe forest recovery on current non-forest land (hence equivalent to reforestation), but it is more often used to describe the establishment of forest after the original one is harvested or disturbed by natural agents such as wind, fire or drought. In the latter case, natural forest regeneration does not involve a shift in land state such as in reforestation.
- (2) We agree with the referee that the appropriateness of afforestation as a natural climate solution (partly) remains debated, but the fact that large-scale afforestation in countries such as China has generated substantial carbon benefits (Lu et al., 2018; Yu et al., 2022) can be hardly ignored. In addition, according to the mainstream literature, ‘afforestation’ refers to the establishment of forest on land that has not recently been forested and ‘reforestation’ refers to the re-establishment of forest (naturally or artificially) on land that was historically forested (see Supplementary Table 1 of our revised manuscript). Hence the difference between the two terms is rather subtle. For example, if a cropland which was forested 100 years ago is to be converted to forest, it is unclear whether we should call this ‘afforestation’ or ‘reforestation’. Given that our study mainly

investigates the trade-off between forestation-induced carbon sequestration and wind energy across a forestation gradient, we argue that our major conclusion remains robust irrespective whether afforestation is excluded or not.

- (3) We agree with the referee that *‘Converting land that is not naturally suitable for forests may cause unintended long-term ecological consequences.’* To provide warnings for this potential limitation, we made several revisions in the manuscript, shown as below:

Lines 425–426: *“Thus, both afforestation and reforestation involve converting land from an unforested to a forested state and do not necessarily entail establishing monoculture plantations.”*

Lines 432–436: *“We emphasize that forestation here does not imply unmanaged, single-species, or unguided planting; rather, we strongly advocate careful, science-based planning and management to ensure positive ecological outcomes and prevent unintended long-term harm. This could be realized through, for example, forest establishment guided by ecological suitability and effective management practices, including planting native species, promoting mixed-species forests and maintaining proper spacing.”*

We also discussed the appropriateness as well as limitations of our mapped forestation area for our research purpose, and also provided warnings to convert natural ecosystems, such as savanna, to forests, in Lines 453–462: *“We assume that this approach maps areas suitable for forest growth by including both places where forests historically distributed, and where recent changes in climate and other environmental conditions make it possible for forests to grow. Fire disturbance, which is reported as critical for maintaining low tree cover in savanna biomes where forests are climatically suitable (Staver et al., 2011), was not included in (Bastin et al., 2019) for predicting tree cover, thus likely resulting in an overestimation of potential forest area. But on the other hand, widespread woody encroachment in African dryland over past decades was reported in response to changes in precipitation, population and burned area (Venter et al., 2018), reflecting the possibility of forest distribution. Nonetheless, we urge that active afforestation should be avoided at places that are naturally unsuitable for forest growth, for example, the savanna biomes that are critical to maintain biodiversity including many animals.”*

[Comment 3] Third, it is commendable that the authors excluded protected areas to safeguard biodiversity. However, biodiversity outside protected regions is also crucial for global conservation efforts, especially in the context of the Kunming-

Montreal Global Biodiversity Framework. Destructive activities associated with wind energy infrastructure, such as road construction, turbine installation, and operational noise, can negatively impact birds and other wildlife. While this issue may fall slightly outside the scope of the current study, I recommend including a brief discussion of these potential biodiversity impacts in the manuscript.

[Response] We agree that biodiversity outside protected areas plays a critical role in achieving global conservation goals, including those outlined in the Kunming-Montreal Global Biodiversity Framework. We have added a brief discussion (Lines 356–359) in the revised manuscript acknowledging that wind energy infrastructure, through associated activities such as road construction, turbine installation, and operational noise, can have negative effects on wildlife, including birds, even in unprotected regions.

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Response Letter to Reviewers' Comments

We sincerely thank all referees for their efforts in reviewing our paper and for their valuable comments.

Reviewer #1 (Remarks to the Author):

[General Comments] The authors have adequately addressed all my suggestions and provided a clear rationale for their decisions. Consequently, I recommend the study be accepted in its current form.

[Response] We highly appreciate the referee's constructive comments that have helped greatly improve the manuscript quality.

Reviewer #3 (Remarks to the Author):

[General Comments] I would like to thank the authors for all the detailed responses to my comments. Thanks especially for taking the time to add additional metrics and making the paper more policy relevant. I am happy to recommend acceptance of this paper. I have only two minor comments for the authors.

[Response] Once again, we would like to express our gratitude to the referee for the constructive feedback, which has helped substantially improve the quality of the manuscript.

[Comment 1] Based on the responses, I think it would be really good to clarify that your paper is dealing with "afforestation/reforestation potential" rather than explicit forest protection. I suppose one could gather this by reading the paper and all the responses but would be good to make this clear explicitly in the introduction. After re-reading the paper, I feel that forest regrowth and its effects on wind speeds would be an entirely different topic altogether and confusing/combining this with afforestation/reforestation potential would open up a rabbit hole of new questions.

[Response] We thank the referee for the suggestion of explicitly clarifying the scope of this study in the Introduction, which help avoid any ambiguity. Accordingly, we have inserted the following sentence in the Introduction (Lines 50–53) to clearly specify the boundary of our analysis:

Lines 50–53:

Although forest protection—such as by halting wood production in currently managed forest—can increase carbon storage, we deliberately exclude this option from our study as large-scale, global cessation of wood production appears implausible given rising global wood demand (see Methods).

[Comment 2] Related to the above, the authors take a biomass/carbon centric approach. This is noted in their responses as well where you say if forests were allowed to regrow by protection, it would have the same effect due to biomass growth (which is true to a certain extent). But forest protection gives certain explicit ecosystem benefits that go beyond carbon. As a simple example, this would lead to less biodiversity loss or even a biodiversity gain. Perhaps similar to point number 1, it would be good to mention that this study does not deal with such ecosystem benefits.

[Response] We agree that our study adopts a primarily carbon-centric perspective and therefore does not explicitly address other ecosystem benefits provided by forests, such as biodiversity conservation, habitat provision, soil protection, and broader ecosystem services.

To avoid potential over-interpretation of our results, we have clarified this limitation in the Discussion section of the revised manuscript (Lines 327–330) by explicitly stating that the present analysis focuses on carbon benefits only and does not evaluate co-benefits related to biodiversity or other ecosystem functions. These important aspects remain to be explored in future research.

Lines 327–330:

We acknowledge that this study focuses on carbon storage as the primary metric for evaluating forest ecosystem benefits, without explicitly assessing other important services such as biodiversity conservation, habitat provision, or soil protection—benefits that forest protection can also deliver. These aspects fall outside the scope of our study and warrant investigation in future research.

Reviewer #4 (Remarks to the Author):

[General Comments] The authors have done an excellent job in this revised version. They have addressed all of my previous comments and concerns, and I have no further issues with the manuscript.

[Response] We are deeply grateful to the referee for the constructive feedback provided throughout the entire review process.