

Response to the reviewers (egusphere-2026-1947)

Dear Editor and Reviewer,

We sincerely appreciate the time and efforts that the Editor and the reviewer have devoted to evaluating our manuscript and providing constructive comments. Our point-by-point responses are given as follows. The reviewer's comments are shown in black, and **our responses are purple**. Please note that all line numbers mentioned in our responses refer to the clean version of the revised manuscript.

Reviewer #2 (Comments to Author):

Reviewer #2 General Comments: This manuscript isolates the impact of land-use and land-cover changes LULCC on Near Surface Wind Speeds (NSWS), using ridge regression to determine how the individual components of LULCC contribute to NSWS changes. Differences between simulations with and without the LULCC forcings provide the basis for the NSWS analysis. NSWS changes are further decomposed by individual land use type and land use transitions. The study makes use of ridge regression to reconstruct NSWS stilling observed historically in the model simulations, and again uses ridge regression to find contributions of each land state and transition to the NSWS. I believe the research in this paper provides new insight on how LULCC component affect NSWS in future climate simulations, and is publication worthy, pending major revisions as follows:

[Response] We sincerely appreciate the time and effort you have devoted to reviewing our manuscript and providing valuable feedback. We have carefully considered all your comments and addressed them in the revised manuscript.

Reviewer #2 Major Comment 1: Throughout the text, quality of writing is inconsistent. There are sections of the paper that appear well-written and only require light revision, but other sections of the paper are very tricky to understand. E.g., lines 81–86 make use of excessive pronouns, which are vague and unclear, and lines 246–251 seems to take too many words to describe that LULCCs give different wind ranges. While I will outline some of the technical writing errors in the minor revisions section, they are no means exhaustive, and I encourage the authors to consider a careful re-reading of the manuscript to ensure proper grammar and succinct wording.

[Response] Points taken. We have carefully re-read the whole manuscript and revised the grammar and wording throughout; all changes can be found in the version with tracked changes. For the two examples you mentioned, **Lines 81–86** have been revised to: “In this study, CN05.1 was interpolated to a $1^{\circ} \times 1^{\circ}$ grid and used to evaluate the performance of the global climate models in simulating NSWS changes over China from 1970 to 2014 (Wu et al., 2017; Long et al., 2021). CN05.1 also served as the dependent variable of the ridge regression and as the reference for assessing how well the ridge-regression calibration corrects the models (Sect. 2.5).” (**Lines 85–88**), and

Lines 246–251 have been condensed to: “We further attribute LULCC-induced NSWs to individual LULCC types. Our premise is that, with other forcings held constant, different land states and management practices correspond to different expected NSWs, so that changes in land-state composition and management lead to NSW changes (Sect. 2.6).” (*Lines 264–266*). Besides the items listed in your minor comments, the Introduction (*Lines 18–30* and *51–54*), the end of Sect. 3.1 (*Lines 242–245*), the beginning of Sect. 3.3 (*Lines 314–318*) and the first paragraph of Sect. 4 (*Lines 343–345*) have also been rewritten to remove vague wording.

Reviewer #2 Major Comment 2: The authors do a good job demonstrating that individual models poorly capture the observed historical NSW trend and that using ridge regression helps to improve this result. However, the authors also state that ridge regression still appears to underestimate the overall mean historical trend. I am curious whether the authors consider this underestimation important to consider when evaluating the LULCC-Induced NSW changes in the future climate scenario. Furthermore, I believe including simulated future NSW trends like 1h, for both SSP126 and SSP370, would help the reader more easily understand whether wind speed trends continue to decline overall and whether the trends remain consistent with historical simulation.

[Response] Thanks for raising this point.

1. Answer to the comment: “*whether the authors consider this underestimation important to consider when evaluating the LULCC-Induced NSW changes in the future climate scenario*”: We expect the underestimation mainly affects the absolute magnitude of the LULCC-induced changes, while their sign, spatial pattern, and relative ranking are likely to be less affected, for two reasons. First, the LULCC-induced NSW is obtained by applying the same ridge weights to the historical and hist-noLu experiments and taking their difference (Eq. 1), and the relative contributions of the LULCC types are ratios of regression coefficients (Eqs. 4, 6 and 7), so a common scaling error largely cancels out. Second, the trend ratio of 0.4 should not be interpreted as a 60% underestimation of the LULCC effect, because part of the observed trend reflects internal variability that free-running simulations cannot be expected to reproduce in phase with the observations.

We nevertheless agree that this underestimation remains a caveat when interpreting the absolute magnitude of the projected changes, and we have added the following discussion in Sect. 4: “*A related caveat is that the ridge-regression reconstruction still underestimates the observed national-mean NSW trend (trend ratio of about 0.4; Fig. 1b). Part of the discrepancy between the reconstructed and observed trends likely reflects internal variability that free-running simulations cannot be expected to reproduce in phase with the observations. Because the LULCC-induced signal is derived as the difference between two experiments calibrated with the same weights (Eq. 1), and the relative contributions in Sect. 3.2 are calculated as ratios (Eqs. 4, 6 and 7), we expect this underestimation to primarily affect the absolute magnitude of the LULCC contributions, while their sign, spatial pattern,*

and relative ranking are likely to be less affected. Nevertheless, the absolute magnitudes of the LULCC-induced changes, including the projected changes in Sect. 3.3, should be interpreted with this additional uncertainty in mind.” Please refer to **Lines 357–365** in the revised manuscript for details.

2. Answer to the comment: “including simulated future NSWS trends like 1h, for both SSP126 and SSP370”: Good suggestion. We have added Fig. S1 in the Supplementary Materials to show the ridge-regression-weighted NSWS trends under SSP1-2.6 and SSP3-7.0, and have added the corresponding results to the revised manuscript: “We first examine the future NSWS trends under SSP1-2.6 and SSP3-7.0 to provide the background for interpreting the LULCC-induced changes (Fig. S1). Overall, the continued decline in NSWS is largely maintained under SSP3-7.0, with stronger decreases over northern China, broadly consistent with the spatial pattern of the historical simulated trend. In contrast, under SSP1-2.6, the decline weakens and NSWS begins to recover in some regions. Within each scenario, the near- and long-term patterns are broadly similar, while the mid-term shows some spatial differences.” Please refer to **Lines 313-317** in the revised manuscript for the added text. Fig. S1 is provided below.

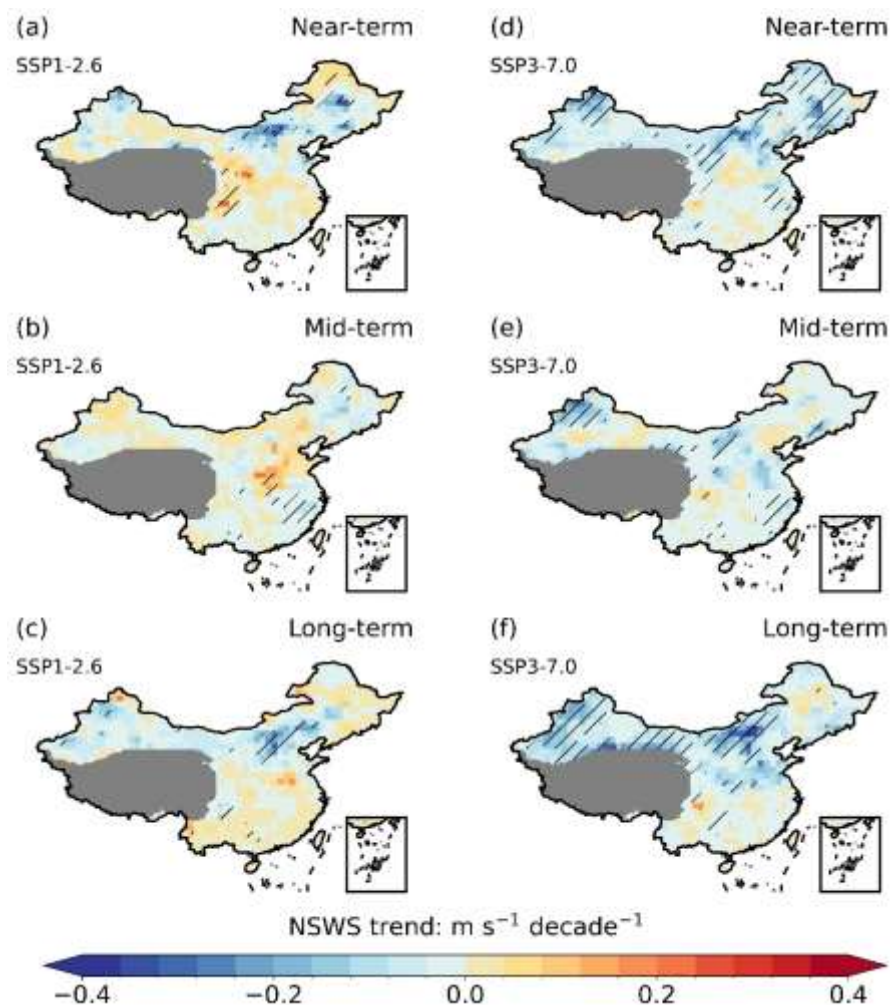


Figure S1. Future NSWS trends derived from ridge-regression-weighted projections. (a–c) NSWS trends under SSP1-2.6 for (a) the near term (2020–2040), (b) the mid term (2040–2060), and (c) the long term (2080–2099); (d–f) as in (a–c), but under SSP3-7.0. Hatching indicates trends that are statistically significant at the 0.1 level.

Reviewer #2 Major Comment 3: Fig. 4 can be improved with the addition of hatching for statistical significance.

[Response] Done. Hatching for statistical significance (Student’s t-test at the 0.1 level) has been added to Fig. 4, and the caption has been revised accordingly (*Lines 337–341*).

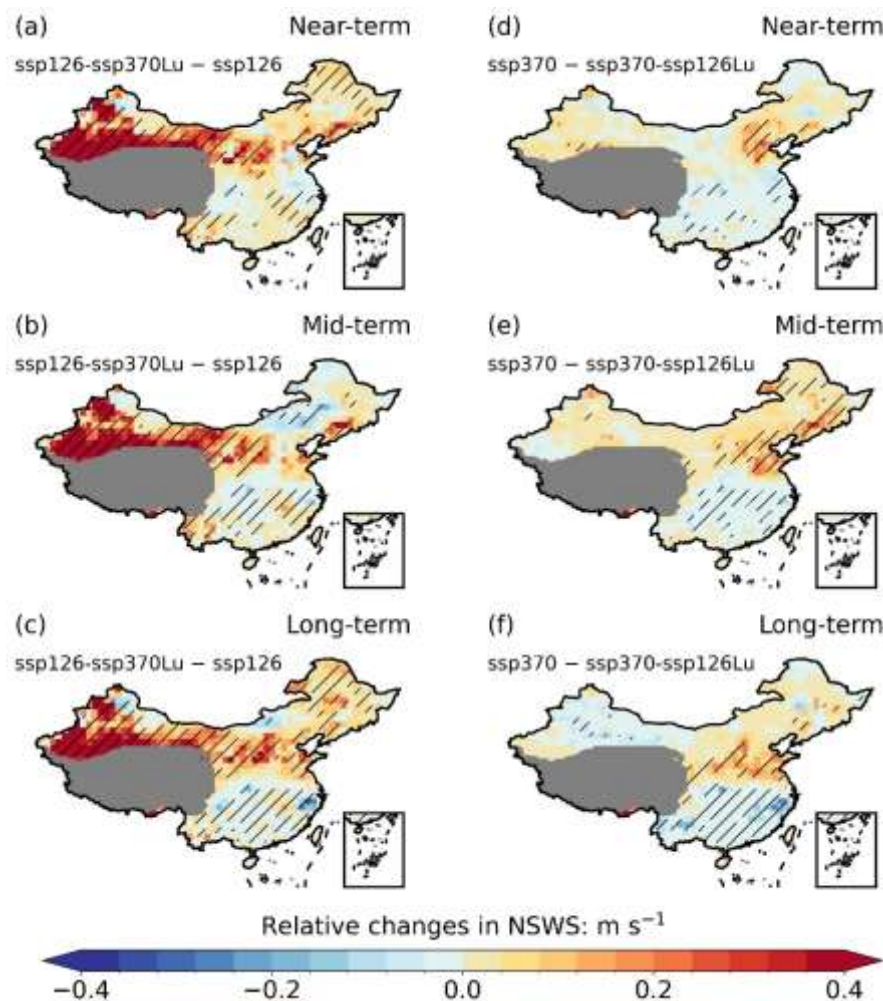


Figure 4: Future LULCC-induced NSWS changes. (a–c) Differences in climatological NSWS between ssp126-ssp370Lu and ssp126 for (a) the near term (2020–2040), (b) the mid term (2040–2060), and (c) the long term (2080–2099); (d–f) as in (a–c), but for ssp370 minus ssp370-ssp126Lu. Hatching indicates grid cells where the differences between the paired experiments are statistically significant at the 0.1 level based on a Student’s t-test across years. ssp126, ssp370, ssp126-ssp370Lu, and ssp370-ssp126Lu denote different experimental designs.

Minor and Technical Revisions:

Reviewer #2 Minor Comment 1: Make sure spacing after periods is consistent, either 1 or 2 spaces, not a mix of both.

[Response] Corrected. We have checked the whole manuscript and a single space is now used after periods.

Reviewer #2 Minor Comment 2: Line 12: With urbanization contributes... -> While urbanization contributes...

[Response] Corrected (*Line 12*).

Reviewer #2 Minor Comment 3: Line 14: These results highlighting -> These results highlight

[Response] Corrected (*Line 15*).

Reviewer #2 Minor Comment 4: Line 18: Near-surface wind speed (NSWS), typically measured at 10 meters above the ground -> 10-meter near-surface wind speed (NSWS)

[Response] Revised as suggested (*Line 18*).

Reviewer #2 Minor Comment 5: Line 22: Remove “In recent four decades”. Between 1980’s and 2010’s is sufficient.

[Response] Removed (*Line 22*).

Reviewer #2 Minor Comment 6: Line 25: Remove “where”.

[Response] Removed (*Line 24*).

Reviewer #2 Minor Comment 7: Line 25: Largest reduction of what?

[Response] Sorry for the confusion. It refers to the reduction of NSWS. The sentence has been revised to: “A similar decline has also been observed in China, with the largest NSWS reduction occurring in spring and the largest increase occurring in autumn...” (*Lines 24–25*).

Reviewer #2 Minor Comment 8: Line 26: Unclear of “its” here refers to NSWS or stilling.

[Response] It refers to NSWS. We have revised “its future evolution and underlying causes” to “the future evolution and underlying causes of NSWS changes” (*Lines 25–26*).

Reviewer #2 Minor Comment 9: Lines 26-28: Line reads as if stilling refers to both NSWS decline over land and NSWS increase over marine- but “stiling” feels like it should just refer to the NSWS decline?

[Response] Yes, you are right. Stiling refers to the decline over land only. The sentence has been revised to: “In contrast to this decline over land, marine NSWS increased before 2010 (Zheng et al., 2016), and this land–ocean contrast highlights the key role of land-surface modifications in terrestrial stilling.” (*Lines 26–28*).

Reviewer #2 Minor Comment 10: Line 31: attributions -> iterations

[Response] What we meant here was “attribution studies”. To avoid confusion, the sentence has been revised to: “Previous attribution studies of NSWS changes related to LULCC have largely relied on a limited number of land-use processes...” (*Line 31*).

Reviewer #2 Minor Comment 11: Lines 51-54. Run-on sentence, also, very difficult to digest.

[Response] Points taken. The sentence has been split into two: *“To address these gaps, we combine experiments from the Land-Use Model Intercomparison Project (LUMIP) of the Coupled Model Intercomparison Project Phase 6 (CMIP6) with ridge regression to investigate the effects of LULCC on NSWs over China. Specifically, we quantify the contributions of individual LULCC types, including land-use transitions and management practices, during 1970–2014 and assess the impacts of future LULCC on NSWs.” (Lines 51–54).*

Reviewer #2 Minor Comment 12: Line 76: Make Table 1’s header “List of CMIP6 experiments used.”

[Response] Revised to “List of experiments from CMIP6 used in this study” (**Line 78**), which also follows the suggestion of Reviewer #1.

Reviewer #2 Minor Comment 13: Line 78: Make Table 2’s header “List of CMIP6 models used.”

[Response] Revised to “Global climate models selected from CMIP6” (**Line 80**).

Reviewer #2 Minor Comment 14: Table 2: Second column should be “Number of Ensemble Members” or “Ensemble Member Count”

[Response] Revised to “Number of ensemble members”.

Reviewer #2 Minor Comment 15: Lines 81-86. Lots of vague wording. Please remove “it” statements, and on line 85, what is “that method”?

[Response] Revised. “It” has been replaced by “CN05.1”, and “that method” referred to the ridge-regression calibration, which is now stated explicitly. Please refer to **Lines 85–88** in the revised manuscript (the revised sentences are quoted in our response to Major Comment 1).

Reviewer #2 Minor Comment 16: Line 115: “Comparing each swap with its parent scenario”, could you add an e.g. to make it clear what this means? I feel like it would be something like, “e.g., SSP126 and SSP126-SSP370Lu”

[Response] Added: “Comparing each swap with its parent scenario (e.g., ssp126-ssp370Lu versus ssp126) yields...” (**Lines 117–118**).

Reviewer #2 Minor Comment 17: Line 118: Should be difference between SSP370-SSP126Lu and SSP370?

[Response] Yes, this was a typo. It has been corrected to “the difference between ssp370 and ssp370-ssp126Lu” (**Lines 123–124**).

Reviewer #2 Minor Comment 18: Section 2.5/2.6, I think at the end of section 2.6 it is implied that the ridge regression analysis is performed on each grid point separately, is that true about ridge regression analysis performed in section 2.5?

[Response] Yes. The ridge regression in Sect. 2.5 is also performed at each grid cell separately. We have clarified this in Sect. 2.5 (*Lines 131–132*) and Sect. 2.6 (*Lines 145–146*).

Reviewer #2 Minor Comment 19: Line 138-140: State instead, “All variables were standardized to the same scale before model fitting to address predictors with different magnitudes. The way to quantify ...” Remove “This analysis enabled clear interpretation of the proportional impact of each variable”.

[Response] Revised as suggested (*Lines 151–153*), and the sentence “This analysis enabled clear interpretation of the proportional impact of each variable...” has been removed.

Reviewer #2 Minor Comment 20: Line 155: Instead of saying “step 1”, better to say Eq. (3)? I understood eventually what this meant but it was tricky to connect the dots when using “step 1”.

[Response] Revised to “the corresponding linear term in Eq. (3)” (*Line 165*).

Reviewer #2 Minor Comment 21: Line 157: What is harvest in this context? I originally thought it referred to crops but it seems to refer to Primary/Secondary land instead? State instead, “Here Y_i denotes transition or harvest, RC_{state} the state-level relative contribution, $RC_{state A}$ to state B the estimated relative contribution for the harvest of the state (if present).”

[Response] Harvest here is the wood harvest from primary and secondary land in LUH2, not the crop harvest. We have clarified it where the term is first used: “...further attributed to specific transition types and, where applicable, harvest (in LUH2, harvest denotes wood harvest from primary and secondary land; Hurtt et al., 2020)” (*Lines 166–167*). Table 4 lists the corresponding “primary harvest” and “secondary harvest” terms. For the sentence after Eq. (7), we have kept the definitions of the four symbols separate, because they refer to different equations (Eqs. 5–7).

Reviewer #2 Minor Comment 22: Line 170: “Table 4: The variables relevant to each state.”

[Response] Revised to “Table 4. Variables relevant to each land state.” (*Line 182*).

Reviewer #2 Minor Comment 23: Line 174: “To retain it, also employ...” -> “As such, we apply...”

[Response] Revised as suggested (*Line 185*).

Reviewer #2 Minor Comment 24: Line 184: Describe why the Tibetan Plateau is masked out?

[Response] We have added the reason: “...with the Tibetan Plateau masked out, because the sparse and uneven station coverage there makes the gridded CN05.1 NSWs unreliable (Minola et al., 2023) and because the coarse-resolution models poorly resolve its complex terrain.” (*Lines 194–196*).

Reviewer #2 Minor Comment 25: Line 190: I do not know what the conventional multi-model ensemble is. Please describe it.

[Response] The conventional MME is the equal-weight arithmetic mean of all models, with the ensemble members of each model averaged first. This definition has been added at its first mention (*Lines 203–205*).

Reviewer #2 Minor Comment 26: Lines 226–228: Again, vague wording. Avoid “it” statements, also, “It also implies that credible LULCC attribution requires reliable skill.”, but how? And wouldn’t accurate LULCC attribution always require reliable skill?

[Response] Points taken. What we wanted to say is that the LULCC-induced signal is the difference between calibrated simulations, so the skill of the calibration directly determines the reliability of the attribution. The end of this paragraph has been rewritten to: “Overall, ridge regression provides consistent gains in both the national-mean metrics and the spatial pattern, offering a more reliable reconstruction of the observed NSWs than either individual models or the MME. Because the LULCC-induced signal is obtained as the difference between calibrated simulations (Sect. 2.4), this improved skill is a prerequisite for the attribution that follows.” (*Lines 241–244*).

Reviewer #2 Minor Comment 27: Line 234: State instead, “Hatching indicates a statistically significant trend at the 0.01 level.

[Response] Revised as suggested (*Line 250*). The same sentence in the caption of Fig. 1 has also been revised (*Line 221*).

Reviewer #2 Minor Comment 28: Lines 235–237. Rewrite first sentence to: “We next employ ridge regression to historical LULCC-induced NSWs given ridge regression’s superior performance over MME and any single model.

[Response] Revised to: “Given the superior performance of ridge regression relative to the MME and any single model, we employ it to derive the historical LULCC-induced NSWs signals.” (*Lines 251–252*).

Reviewer #2 Minor Comment 29: Lines 247–251: Seems to be a lot of verbiage to describe that different LUCCs give different wind ranges.

[Response] Points taken. These sentences have been condensed into one sentence (*Lines 263–265*; see our response to Major Comment 1).

Reviewer #2 Minor Comment 30: Lines 298–303/309. I feel like the section up to line 303 would be better stated in the data and methods, possibly up to line 309 as well.

[Response] Good suggestion. Following the reviewer’s suggestion, we have moved the relevant information from Sect. 3.3 to the Methods and reorganized and rewritten the corresponding methodological descriptions for clarity. Specifically, the description of the two land-use trajectories (*Lines 300–303* in the original manuscript) has been moved to Sect. 2.1 (*Lines 68–70*), while the description of the factorial experimental design and its policy interpretation (original *Lines 304–309*) has been moved to Sect.

2.4 (*Lines 114–125*). Sect. 3.3 has been revised accordingly, with the methodological details removed and the description of the future LULCC analysis updated (*Lines 316–320*).

Reviewer #2 Minor Comment 31: Line 322: “divergence” can be confused with the meteorological parameter here. Consider “differences between periods” instead.

[Response] Revised to “the differences between periods” (*Line 332*). “Divergence” in *Lines 322–323* has also been replaced by “difference”.

Reviewer #2 Minor Comment 32: Line 328: Should it be SSP370-SSP126Lu minus SSP370 instead, according to figure?

[Response] Thanks for pointing this out. We have checked the plotting script: panels (d–f) show ssp370 minus ssp370-ssp126Lu. The caption (*Lines 336–340*), Sect. 2.4 (*Lines 121–125*) and Sect. 3.3 (*Lines 322–323*) are now consistent with the figure.