

Reply on RC2

Comment 1: *One issue is that some of figures from the 2024 publication are reproduced in the current manuscript without any indication that they were previously published. This is not appropriate, and the authors must provide proper attribution.*

We thank the reviewer for pointing out this oversight and apologize for this. The CTRL climatology shown in panels (a)–(d) of Figures 2 and 5 was previously published in Ma et al. (2024) as Figures 4 and 3, respectively. We retain these panels in the current manuscript because they provide the necessary present-climate baseline for interpreting the projected PGW–CTRL differences in panels (e)–(h), and placing them in the same figure allows readers to directly compare the changes with the current-climate low-level jet distribution. In the revised manuscript, we will add explicit attribution to both figure captions and to the main text where these panels are first referenced.

In the manuscript:

Figure 2 caption (Lines 171–173): the sentence "Panels (a)–(d) are reproduced from Ma et al. (2024)." will be added.

Figure 5 caption (Lines 255–257): the sentence "Panels (a)–(d) are reproduced from Ma et al. (2024)." will be added.

Section 3.1.1 (Line 137–138): "...in the CTRL simulation (Fig. 2a–d; reproduced from Ma et al., 2024)..."

Section 3.2.1 (Line 230–231): "...in the current climate (Fig. 5a–d; reproduced from Ma et al., 2024)..."

Comment 2: *Another concern is that the authors do not discuss the limitations of their future climate simulations. The authors used a Pseudo-Global Warming experiment where "deltas" (i.e., differences) were calculated at ERA-Interim grid point locations between the composite values of a 19-member GCM ensemble for 2071-2100 and 1976-2005, with the deltas simply added to the 2000-2013 ERA-Interim data which then served as input to WRF. While this approach substantially reduces computational requirements, it also has serious limitations including the inability to capture large-scale circulation changes and interannual and internal climate variability. The authors do not acknowledge these limitations, nor do they consider how their approach may have influenced their findings. Also, by using the PGW method and one mesoscale model (i.e., WRF), the authors have only a single future projection and no estimate of uncertainty. Also, the authors statement that "Crucially, the PGW approach obviates the need for extended simulations; it allows for centennial-scale projections to be derived from just 13 years of data through equivalent climate perturbations" is questionable, and it is unclear what they mean by "equivalent" climate projections. The use of only a 13-year period for the control and future simulations is another weakness of the manuscript, and the use of composite values for a*

single 30-year period at the end of the century is hardly "centennial-scale" as it ignores the evolution of the climate during the century. A data-related concern also raised in the reviews of the 2024 publication is that the WRF simulations are driven by the older ERA-Interim reanalysis rather than the more recent ERA 5 reanalysis.

Thanks for your important comments. We agree that the original manuscript did not sufficiently discuss the limitations of the PGW approach. In the revised version, we will expand the description of the PGW experiment in Section 2.1 and add a dedicated discussion of limitations in Section 5.

First of all, we agree that the PGW approach has limitations in representing changes in large-scale circulation variability and interannual and internal climate variability. It should be noted that, following Liu et al. (2017), the CMIP5 perturbation was applied not only to temperature and humidity but also to horizontal winds, geopotential height, and sea-level pressure, and spectral nudging was used to constrain the large-scale fields. Thus, the change in the monthly-mean large-scale state is included in the PGW simulation. However, the frequency, timing, and tracks of synoptic-scale weather systems are inherited from the ERA-Interim baseline period (2000–2013) and remain unchanged between CTRL and PGW. As a result, the PGW simulation cannot represent potential changes in storm tracks, synoptic variability, or interannual and internal climate variability (Liu et al., 2017). In this case, the PGW approach is a thermodynamic sensitivity experiment rather than a fully transient climate projection, and the projected LLJ changes in this study should be interpreted in this context. This limitation may also influence our findings. For example, the springtime strengthening of the Great Plains S-LLJ may also be affected by future changes in synoptic forcing, which are not represented in this simulation.

Also, there is indeed only a single future projection is provided in this study. The ensemble mean of 19 CMIP5 GCMs can reduce the internal variability and model errors of individual GCMs. However, it cannot represent the spread among different GCMs, emission scenarios, or regional model configurations. Therefore, the uncertainty of the projected LLJ changes cannot be quantified with the current experimental design. Considering the huge computational cost of convection-permitting simulations over the whole North American continent, multi-model and multi-scenario ensembles are still difficult at present, and they will be listed as an important direction for the future work.

Third, the statement "Crucially, the PGW approach obviates the need for extended simulations..." is inaccurate, and it should be removed. In fact, the PGW perturbation is the difference between two 30-year climatologies (2071–2100 minus 1976–2005), and it does not describe the evolution of the climate during the 21st century. So "centennial-scale" is not an appropriate description.

We also acknowledge that the 13-year period is shorter than the normal 30-year climatology. But considering the computational cost of 4 km simulations over the whole continent, it is a balanced compromise, and previous studies have shown that this period is sufficient for identifying robust climatological features and climate change signals (Liu et al., 2017). Besides, all the PGW–CTRL differences in this

manuscript were tested at the 95% confidence level. Nevertheless, the 13-year period may not fully capture low-frequency variability, such as different ENSO phases or decadal variability, which may affect the robustness of some regional signals.

Finally, regarding the ERA-Interim forcing dataset, the simulations used in this study are an existing and publicly available convection-permitting dataset produced by NCAR (Liu et al., 2017; Rasmussen and Liu, 2017). When these simulations were conducted, ERA5 was not yet available, and in the short time, re-running both CTRL and PGW simulations at 4-km resolution over the whole continent is beyond our computational resources. However, in the Appendix of Ma et al. (2024), we compared the winter LLJs simulated by WRF with those identified from ERA5 and found generally consistent spatial patterns. Meanwhile, with 4-km grid spacing and nine vertical levels below 1 km, the boundary-layer jet structures are dynamically resolved by WRF rather than directly inherited from the coarser driving data. Moreover, since CTRL and PGW are driven by the same reanalysis, the biases in the forcing data are expected to be partly cancelled in the PGW–CTRL differences, although they cannot be fully excluded.

Based on the statement above, there will be modifications in the manuscript:

The last paragraph in Section 2.1 will be replaced by:

“The climate perturbation was applied to eight variables, including horizontal wind components, geopotential height, temperature, specific humidity, sea surface temperature, soil temperature, sea level pressure, and sea ice fraction. The monthly-mean perturbations were linearly interpolated to each 6-hourly time step (Rasmussen et al., 2011). Meanwhile, spectral nudging was applied to geopotential height, horizontal wind, and temperature above the PBL for wavelengths larger than approximately 2000 km, with a relaxation timescale of 6 hours (Liu et al., 2017).

It should be noted that the PGW approach does not simulate the transient evolution of the climate during the 21st century. Instead, the end-of-century climate change signal, calculated as the difference between the 2071–2100 and 1976–2005 CMIP5 ensemble-mean climatologies, is added to the observed atmospheric states of 2000–2013. This design significantly reduces the computational cost compared with continuous transient simulations, which is important for convection-permitting modeling at the continental scale. However, it also brings several limitations, which are discussed in Section 5.”

The last paragraph in Section 5 should be replaced by:

Nevertheless, several limitations should be noted. First, as a thermodynamic sensitivity experiment, the PGW approach cannot represent future changes in synoptic variability and storm tracks. For example, the springtime strengthening of the Great Plains S-LLJ may also be affected by changes in synoptic forcing, which are not represented in this simulation. Moreover, the results are based on a single model and a single emission scenario, so the uncertainty of the projected LLJ changes cannot be estimated. Third, the 13-year simulation period may not fully capture low-frequency variability such as ENSO. Besides, the simulations are driven by ERA-Interim rather than the more recent ERA5 reanalysis,

although the biases in the forcing data are expected to be partly cancelled in the PGW-CTRL differences. Future research should use multi-model and multi-scenario convection-permitting ensembles to quantify these uncertainties, and further examine interactions between LLJs and extreme weather systems to deepen the understanding of low-level wind responses under climate change.

Comment 3: *I have reservations regarding the monthly analyses of jet frequency and speed in Section 3.5 of the manuscript. For the Great Plains LLJ, the authors summed (for frequency) or averaged (for speed) gridpoint values by month for a large area extending from southern Texas to the border between the Dakotas. However, a number of previous studies have shown that the location of frequent S-LLJs in the Great Plains shifts northward from the south Texas in spring to the central plains in summer and autumn. This shift can be seen in the monthly plots of S-LLJ frequency in Doubler et al. (2015), although other authors have documented similar spatial displacements. The monthly values in the bar charts shown in Figure 4 mask the intra-annual spatial variability in jet locations. Moreover, S-LLJs in the Great Plains can form under very different large-scale circulations, which Igau and Nielsen-Gammon (1998) broadly labeled as "active" and "quiescent" patterns, depending on location and season. Consequently, it is not clear how these spatially-averaged monthly values of frequency and jet speed should be interpreted. Similar large seasonal shifts in jet location are not seen for the California coastal N-LLJ, although the spatial extent of the area of frequent N-LLJs expands in the warm season (see the N-LLJ monthly plots in Doubler et al. 2015). These differences complicate comparison of the monthly values between the two jet types. Why not instead show monthly maps of PGW-CTRL for frequency and speed, similar to what was done by season? That way any spatial variations in the monthly differences are evident. Also, because the units for jet frequency (i.e., percent frequency) and jet speed (meters per second) differ, any comparisons of the relative impact of climate change on these two parameters requires that their units be converted to the same scale (i.e., percent change -- (PGW-CTRL)/CTRL). Without a uniform scale, statements such as "Compared with occurrence frequency, the monthly variability of core wind speed for the California coastal N-LLJ is considerably weaker" or "These results suggest that, for the California coastal N-LLJ, future changes are dominated by a redistribution of occurrence on the monthly scale, whereas modifications to individual event intensity are secondary and largely confined to the midsummer period."*

Thank you for this helpful comments. We assume that the reviewer refers to Section 3.3 and Figs. 8–9 in the manuscript. We agree that spatially averaging over a large region may mask the intra-annual migration of the Great Plains S-LLJ. Following the suggestion, we have plotted the monthly PGW-CTRL differences in LLJ frequency and core wind speed for all 12 months (Figs. R1–R4 below this answer), and these figures will be added to Appendix A of the revised manuscript as Figs. A1–A4. The analysis domains of Figs. 8a and 9a are also shown in these maps.

For the Great Plains S-LLJ, the monthly maps clearly show that the projected enhancement follows the seasonal migration of the jet. In April–May, the largest increase in frequency is located over the southern Great Plains (northern Texas, Oklahoma, and Arkansas). In June and July, the enhancement center shifts northward to Kansas and Nebraska, while a decrease begins to appear over southern Texas and northeastern Mexico. In August to September, the response becomes a dipole pattern, with increases along the western High Plains and

decreases over southern Texas and eastern Oklahoma. In October–November, the enhancement center returns to the southern Great Plains. In fact, the near-zero change of the regional-mean frequency in August (Fig. 8b) is mainly a result of these opposite changes cancelling each other, rather than an absence of response. Meanwhile, the changes in core wind speed are not spatially collocated with the changes in frequency. For example, in May and October, the largest increase in wind speed is located to the north of the largest increase in frequency.

For the California coastal N-LLJ, the monthly maps show that the midsummer decrease in frequency is not uniform. From June to September, the frequency decreases in a narrow band along the coast, especially in July and August, while it increases farther offshore to the northwest. Meanwhile, the core wind speed decreases over almost the whole jet region in July–August. Thus, the reduction in the regional-mean frequency mainly reflects an offshore displacement of the jet activity, together with a general weakening of the jet intensity. This finding will be added to Section 3.3.

We also agree that the Great Plains S-LLJ and the California coastal N-LLJ have different spatial and seasonal characteristics, so their monthly values should not be compared directly. In the revised manuscript, the two jets will be discussed separately, and the monthly bar charts will be described as the temporal evolution within the core activity region of each jet, together with the monthly maps in Appendix A. Regarding the different large-scale circulation patterns associated with the Great Plains S-LLJ (Igau and Nielsen-Gammon, 1998), it should be noted that the sequence of synoptic weather systems in the PGW simulation is identical to that in CTRL. Thus, the mixture of "active" and "quiescent" patterns is the same in both simulations, and the PGW–CTRL differences are not caused by changes in the frequency of these circulation patterns. Instead, they reflect the response of the LLJs to the imposed thermodynamic changes under the same synoptic conditions. Besides, since the monthly fields were calculated as multi-year climatologies, the monthly maps are shown without significance testing, and the statistical significance of the changes is presented in the seasonal maps (Figs. 2–7). Nevertheless, the main features in the monthly maps are spatially coherent and persistent over consecutive months. For example, the decrease over southern Texas persists from June to November, and the northward migration of the enhancement center evolves continuously from April to July, which suggests that these patterns are robust rather than random noise.

Regarding the comparison between frequency and wind speed, we agree that the two variables have different units, and the original statements comparing them were not appropriate. Following the reviewer's suggestion, we calculated the relative changes $(PGW-CTRL)/CTRL$ for the regional means in Figs. 8 and 9. For the Great Plains S-LLJ, the relative change in frequency ranges from about 1.5% (August) to 14% (November), with about 8–10% in April–May, whereas the relative change in core wind speed is within 2% in all months. For the California coastal N-LLJ, considering only the warm season (April–October) when the jet is active, the relative

change in frequency is about +21% to +25% in April–May and October, and –3% to –6% in July–August, whereas the relative change in core wind speed is within $\pm 3\%$. Thus, on a uniform scale, the response of frequency is indeed much larger than that of core wind speed for both jets. The relevant statements will be revised based on these relative changes.

In the manuscript:

After the first paragraph in Section 3.3, add this as a new paragraph:

The Great Plains S-LLJ shifts northward from southern Texas in spring to the central Great Plains in summer and autumn (Doubler et al., 2015), and it can form under different large-scale circulation patterns. Thus, the spatially averaged monthly values cannot fully represent the intra-annual spatial variability of the jet. For this reason, the monthly PGW–CTRL differences in frequency and core wind speed are also shown in Figs. A1–A4 of Appendix A. Since the synoptic sequence is identical in CTRL and PGW, the differences between the two simulations are not related to changes in the occurrence of different circulation patterns.

Later in the next paragraph, add this in the end:

By contrast, in midsummer (July–August), the regional-mean PGW–CTRL differences are much smaller. However, the monthly maps show that this small change is partly due to cancellation of opposite changes (Fig. A1). In August, the S-LLJ frequency decreases over southern Texas and eastern Oklahoma but increases along the western High Plains. The enhancement center also shifts from the southern Great Plains in April–May to Kansas and Nebraska in June–July, consistent with the seasonal migration of the S-LLJ.

The last sentence before Figure 8 has been replaced by:

In fact, the spatial standard deviations of core wind speed are generally within $1\text{--}2\text{ m s}^{-1}$, indicating relatively uniform intensity across the core region. When expressed as relative changes, the S-LLJ frequency increases by about 8–10% in April–May, whereas the core wind speed increases by less than 2% in all months. Besides, the increases in core wind speed are not spatially collocated with those in frequency (Figs. A1 and A2).

In the second-to-last paragraph before Figure 9:

In contrast, during midsummer (July–August), although the California coastal N-LLJ still represents the annual peak in occurrence frequency, its regional-mean frequency is reduced compared with CTRL. However, the monthly maps show that this reduction is not uniform (Fig. A3). The frequency decreases in a narrow band along the coast, while it increases farther offshore to the northwest. This indicates that future warming leads to an offshore displacement of the midsummer N-LLJ activity, rather than a simple decrease in jet occurrence.

And the whole last paragraph before Figure 9 will be replaced by:

In both the CTRL and PGW simulations, the core wind speed of the California coastal N-LLJ shows a relatively small annual variation. Nevertheless, the PGW core wind speeds tend to be enhanced during late spring (April–May) and October, while a weakening emerges over

almost the whole jet region during July–August (Fig. A4). When expressed as relative changes, the frequency increases by about 21–25% in April–May and October and decreases by 3–6% in July–August, whereas the difference in core wind speed is within $\pm 3\%$ in all months. These results suggest that, for the California coastal N-LLJ, the future changes are mainly reflected in the occurrence and spatial distribution of the jet, while the changes in jet intensity are relatively small.

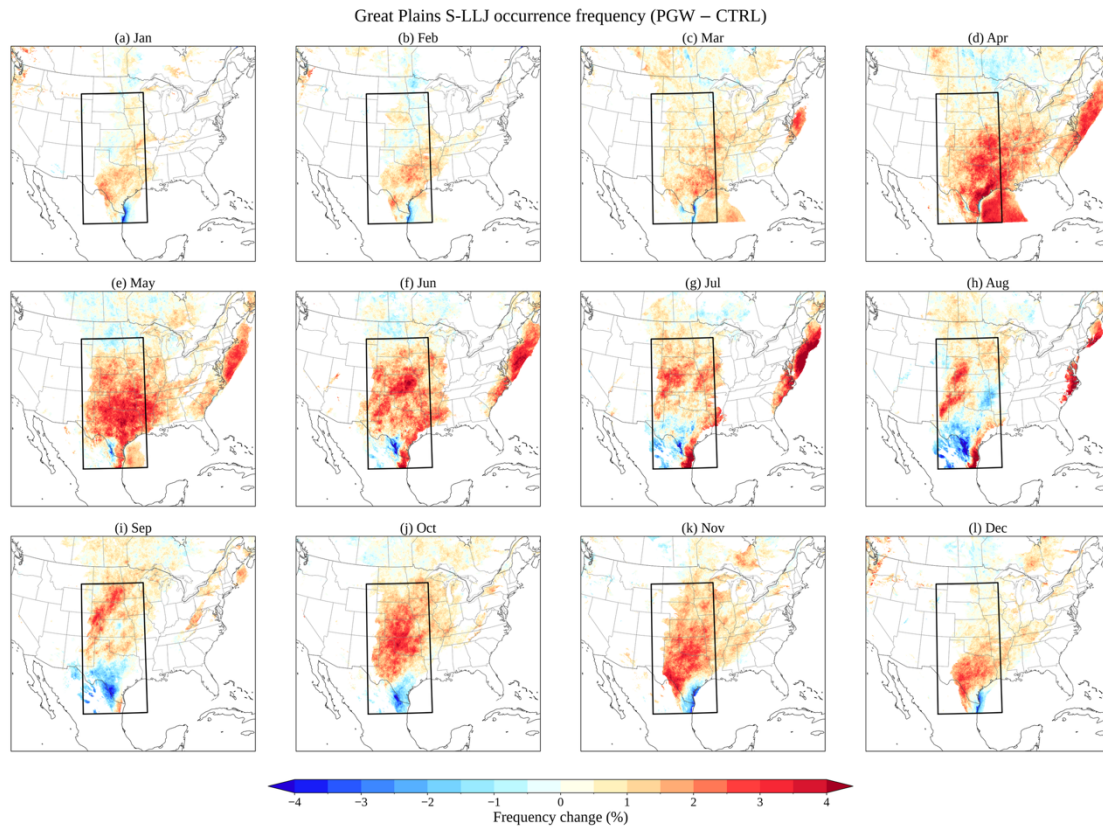


Figure R1. Monthly PGW – CTRL differences in Great Plains S-LLJ frequency (%) from January to December. The black box is the domain used in Fig. 8a. Grid points with CTRL frequency below 5% are masked

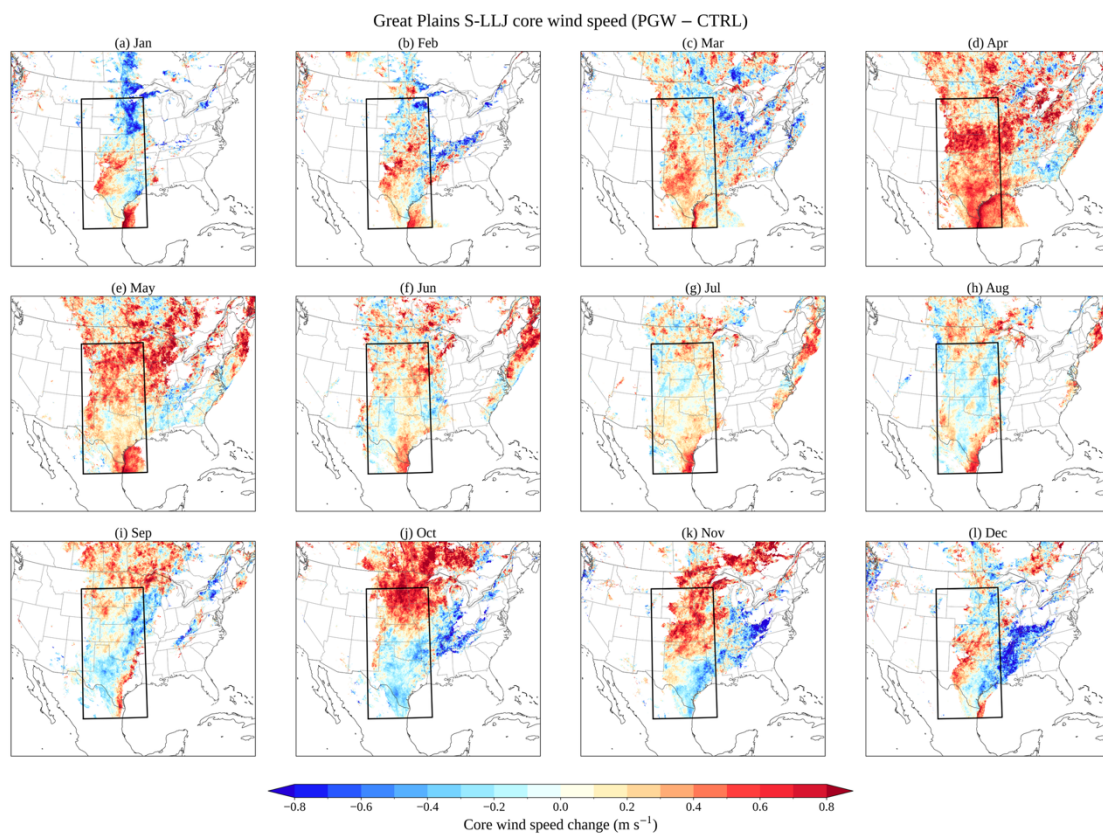


Figure R2. Same as Fig. R1, but for Great Plains S-LLJ core wind speed (m s^{-1}).

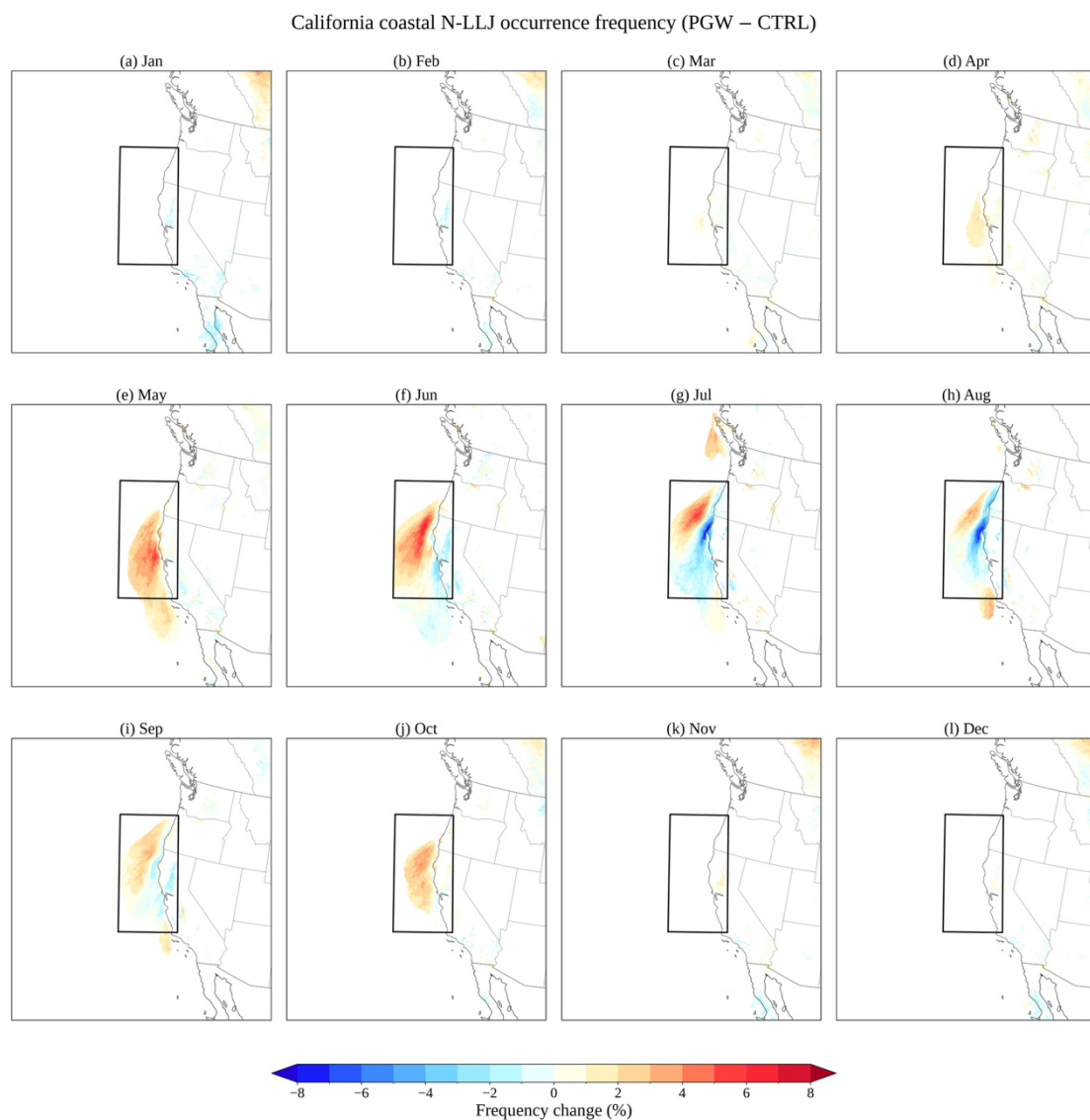


Figure R3. Same as Fig. R1, but for California coastal N-LLJ frequency (%). The black box is the domain used in Fig. 9a.

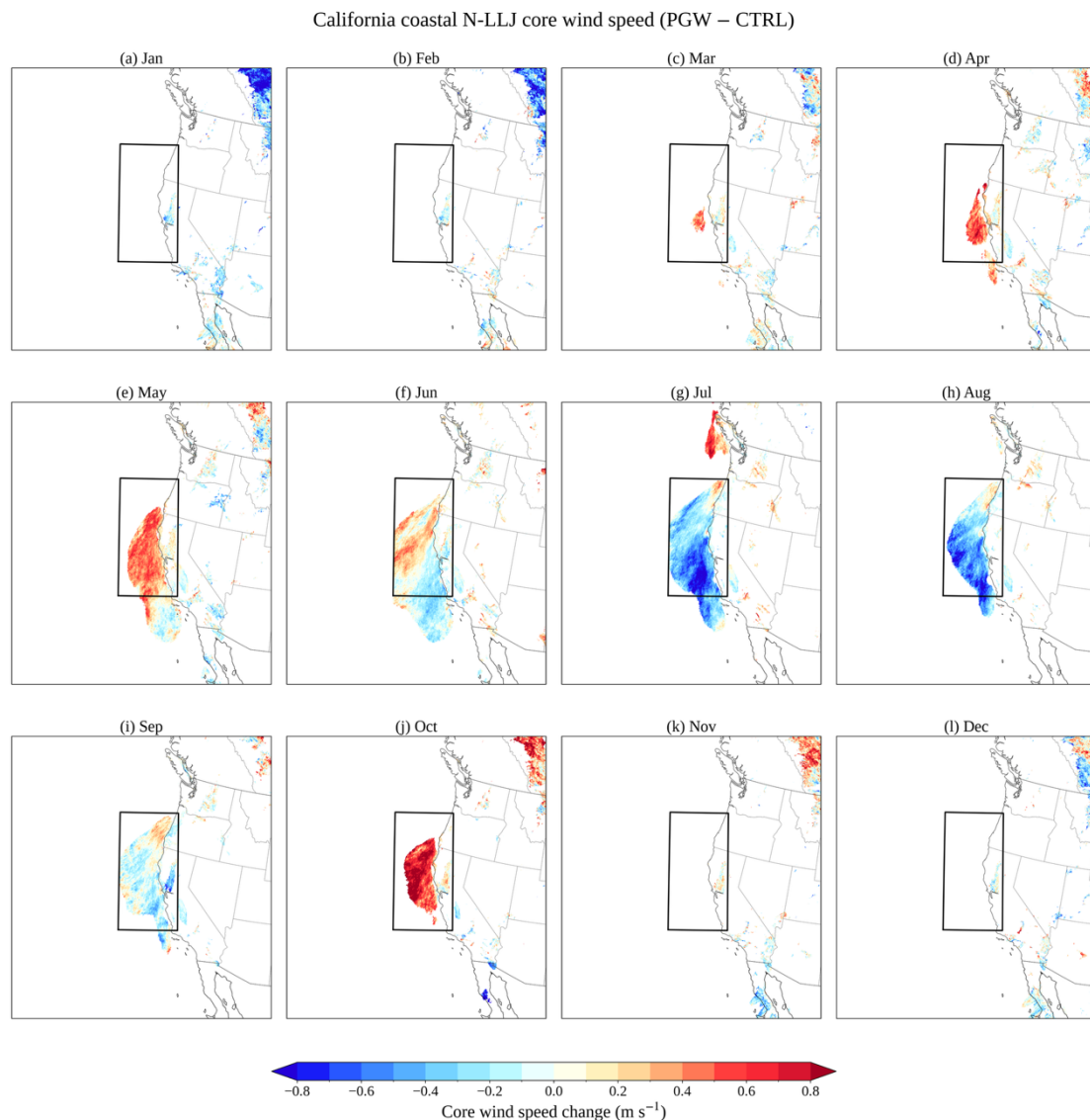


Figure R4. Same as Fig. R3, but for California coastal N-LLJ core wind speed (m s^{-1})

Comment 4: In section 4, the authors state "this study selects two representative jet core locations (refer to Points A and B in Fig. 1) to conduct a targeted diagnosis of the low-level wind structure under the CTRL and PGW scenarios", however what is labeled "A" and "B" in Figure 1 are transects rather than points. There are no "points" on the map. Also, how is the jet core height defined for the inertial oscillation analysis? Is the jet core height allowed to vary between May and August? The authors refer to Figure 11 as a "meridional cross-section", however meridional cross sections run along a line of longitude, in other words they run north-south. However, the cross section in Figure 11 runs west-east, or in other words along a line of latitude (depending on the map projection). The authors refer to "from the valley to the highlands", but isn't the lower elevations on these plots the plains (not necessarily a valley)? The authors focus on differences between PGW and CTRL in the inertial oscillation (for Great Plains LLJ) and the background geostrophic wind (for the California coastal jets), but other factors also contribute to jet formation such as synoptic-scale forcing for Great Plains jets (see

Burrows et al, 2019, Journal of Climate, doi:10.1175/JCLI-D-18-0891.1) and the elevation of the marine boundary layer for the coastal jets (see Beardsley et al., 1987, Journal of Geophysical Research).

We agree that the description of Points A and B was unclear. Points A and B were selected at the locations where the contrast between the spring and summer changes in LLJ frequency and core wind speed is large, with Point A over the southern Great Plains and Point B along the California coast. The hodographs in Figs. 10 and 13 are calculated at these two points, and the red lines in Fig. 1 mark the zonal cross-sections through them, which are analyzed in Figs. 11, 12, 14, and 15. In the revised manuscript, the caption of Fig. 1 will be revised as: "The red lines mark the zonal cross-sections through Points A and B, which are analyzed in Section 4." Meanwhile, "(refer to Points A and B in Fig. 1)" in Line 388 will be replaced by "(Points A and B, located on the zonal cross-sections marked by the red lines in Fig. 1)".

As to the inertial oscillation analysis, the jet core height is defined as the height of the maximum wind speed at the time when the LLJ reaches its peak intensity in the mean diurnal cycle, and this height is fixed throughout the diurnal cycle. Since the jet core height varies with season and climate, it is determined separately for May and August, as well as for CTRL and PGW. This definition will be added to the caption of Fig. 10 and to Section 4.1.

We agree that "meridional cross-section" is incorrect, and it will be replaced by "zonal cross-section" in the text and in the captions of Figs. 11, 14, and 15. Also, "valley" will be replaced by "plains" throughout Section 4.1.

Thanks for your comments on other factors that contribute to the LLJ formation. For the Great Plains S-LLJ, synoptic-scale forcing such as the coupling with upper-level jet streams plays an important role (Burrows et al., 2019). Since the synoptic sequence in PGW is the same as in CTRL, the changes in synoptically forced LLJs are not represented in this study, and our analysis mainly reflects the response to the thermodynamic background changes. For the California coastal N-LLJ, the jet core is usually located near the top of the marine boundary layer (MBL), where the inversion is strongest (Beardsley et al., 1987). Although the MBL height is not directly diagnosed here, Fig. 14f shows relatively weak warming near the coast at 300–500 m and pronounced surface warming near 125° W in July, implying a weakened coastal inversion, which may also contribute to the summer weakening of the jet. These factors will be discussed at the end of Sections 4.1 and 4.2.

Comment 5: *In Section 5 (Conclusions), the authors need to elaborate on their key findings and discuss the limitations of their analysis. For example, is the projected springtime increase in Great Plains S-LLJ frequency and speed not only a function in changes in boundary-layer forcing but also a function of changes in synoptic forcing in the future? How do the authors know that the difference between their findings and those of Tang et al. (2017) are due to the authors' use of convection-permitting simulations rather than their use of the PGW approach to drive the simulations compared to individual GCMs? Or perhaps the differences are a combination*

of both? In addition to Tang et al., several previous studies have investigated potential future changes in the Great Plains LLJ. How do the authors' findings compare to those studies? The authors also need to cite other studies when appropriate. For example, citations are needed for the authors' statement "This behaviour contrasts with large-scale or coarse-resolution projections that primarily emphasize monotonic changes in coastal wind strength and indicates that future coastal LLJ responses are governed not simply by background circulation changes, but by a reorganization of coastal boundary-layer structure and sea-breeze–baroclinicity interactions that can only be captured at convection-permitting scales." What previous studies are the authors referring to here? I also remind the authors that in neither this manuscript or the 2024 publication did they compare simulations at the same spatial resolution and for the same time period but with and without a convection-permitting scheme, thus they can only infer rather than directly evaluate the contribution of the convection-permitting scheme in simulating LLJs and to understanding the mechanisms responsible for their formation. Other factors such as horizontal resolution and choice of mesoscale model may also contribute to differences with previous studies. Another limitation is the authors' use of only one mesoscale model and only one driving future scenario. Consequently, the authors are unable to provide an estimate of the uncertainty surrounding their future projections.

Thanks a lot for these constructive comments. We agree that the key findings should be discussed more carefully in the context of previous studies, and that several statements in the original Conclusions were overstated. The Conclusions will be revised accordingly.

For the limitations, the discussion of the PGW approach, the single model and single scenario, the 13 years, and the ERA-Interim forcing has been added to Section 5, as described in our response to Comment 2. For the role of synoptic forcing in the springtime strengthening of the Great Plains S-LLJ, the synoptic sequence in PGW is identical to that in CTRL. Thus, the projected changes mainly reflect the response to the imposed thermodynamic changes under the same synoptic conditions, and the potential contribution of future changes in synoptic forcing is not represented in this study. This will be clearly stated in Section 5.

For the comparison with Tang et al. (2017) and other previous studies, we agree that the original statement attributing the differences to the convection-permitting configuration was not appropriate. After re-examining the results, especially the monthly maps added in response to Comment 3, we found that our results are broadly consistent with previous studies. Zhou et al. (2021) found, based on global climate models, that the Great Plains LLJ intensifies and extends northward in spring but shows little change in summer. Tang et al. (2017) projected increases in nocturnal LLJ frequency over the southern Great Plains in spring and over the central Great Plains in summer, with small changes in jet speed. Similarly, our results show the strongest enhancement over the southern Great Plains in spring, the northward shift of the enhancement center to the central Great Plains in summer, and relatively small changes in core wind speed. The muted summer response in our regional mean is partly due to the cancellation between the increase over the central Great Plains and the decrease over southern Texas. We also agree that the remaining differences may be related to the forcing approach (PGW versus GCM-driven simulations), horizontal resolution, and model configuration, which cannot be separated in this study. Since

no simulation with parameterized convection at the same resolution and period is available, the contribution of the convection-permitting configuration can only be inferred rather than directly evaluated. This will be clearly stated in the revised Conclusions.

For the California coastal N-LLJ, we agree that citations are needed and the statement was overstated. Rykaczewski et al. (2015) showed, based on CMIP5 models, that the future changes in coastal upwelling-favorable winds are not a uniform intensification, but rather a poleward displacement associated with the shift of the subtropical high. Our results are consistent with this view, and further show an offshore displacement and a weakening of the jet near the coast in summer. The statement will be revised with this citation, and "can only be captured at convection-permitting scales" will be softened. Similar expressions in Lines 585–586 and 596–597 will also be revised.

In the manuscript:

Lines 561–568 will be replaced by:

The most pronounced strengthening of the Great Plains S-LLJ occurs in spring, with occurrence frequency increasing by up to about 3 percentage points and core wind speed increasing by about 0.3 m s^{-1} . In contrast, the summer response in the regional mean is muted, which is partly due to the cancellation between the increase over the central Great Plains and the decrease over southern Texas. These results are broadly consistent with previous studies. Zhou et al. (2021) found that the Great Plains LLJ intensifies and extends northward in spring but shows little change in summer, based on global climate models. Tang et al. (2017) also projected increases in nocturnal LLJ frequency over the southern Great Plains in spring and over the central Great Plains in summer, with small changes in jet speed. Furthermore, the convection-permitting simulation suggests that the muted summer response is associated with the increased nocturnal boundary-layer instability, which weakens the decoupling and damps the inertial oscillation, although the contribution of the convection-permitting configuration cannot be directly evaluated in this study.

Lines 569–574 will be replaced by:

This study also projects that the California coastal N-LLJ exhibits enhanced jet activity in spring and autumn, but a summer weakening accompanied by an offshore shift of the jet activity. Previous studies based on global climate models have suggested that the future changes in coastal upwelling-favorable winds are not a uniform intensification, but rather a poleward displacement associated with the shift of the subtropical high (Rykaczewski et al., 2015). Our results are consistent with this view, and further show an offshore displacement and a weakening of the jet near the coast in summer. This suggests that, besides the large-scale circulation changes, the reorganization of the coastal boundary-layer structure and the

interaction between the sea breeze and coastal baroclinicity may also play an important role, which may be better represented at convection-permitting scales.

In Lines 585–586, "that are poorly represented in coarse-resolution projections" will be replaced by "which may be better represented at convection-permitting scales". In Lines 596–597, "giving a dimension of LLJ response largely unresolved in previous coarse-resolution projections" will be replaced by "providing additional insights into the LLJ response that are difficult to obtain from coarse-resolution projections".

Comment 6: I did a quick scan of the references and found at least one misspelled author name, citations in the text that are not in the reference list, and one article that is listed twice in the reference section. The authors need to carefully check the references for accuracy.

Thank you so much for pointing out these errors, and we apologize for the oversight. We have carefully checked all citations and the reference list. The misspelled author name "Walter et al., 2008" in Line 130 will be corrected to "Walters et al., 2008". The missing references, Juliano et al. (2025) and Walters and Winkler (2001), will be added to the reference list. The duplicated entry of Tang et al. (2017a, b) will be merged into a single reference, Tang et al. (2017). In addition, the format of all references will be checked for consistency, and the new references cited in the revised manuscript will be added.