Response letter to Referee Reviewers

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REVIEWER COMMENTS - Referee Reviewer 1

This study evaluates the sensitivity of the PGW method to regionally uniform vs. gridpoint scale perturbations and the inclusion of different perturbed meteorological variables. The authors study the results in the context of 3 different flood events that occurred in the Northeastern U.S. using WRF-simulations forced by ERA5 and ERA5 perturbed with CESM-LENS.

I thought the basis of this study was interesting, given that there is little consistency in how PGW simulations are designed, but the lack of clarity in the methods, the lack of including key PGW literature, and arguments that did not make sense resulted in a paper that had too many issues to make a strong argument for what the authors were trying to show. For this reason, this paper cannot be accepted unless substantial revision is undergone. I will provide more specific comments below to explain my rationale.

We thank the reviewer for the questions and suggestions that help improve the manuscript. We have responded to all the issues raised and hope this response addresses all the concerns. Corresponding revisions have been made to the paper.

General:

1. I am confused about how you evaluate the influence of different meteorological variables in your paper for each flood event. The figure captions merely state "2055 October", "2056 May" and "2056 June". However, each flood event corresponding to those months only lasts a few days of the month. So, when you present your Figures 9-13, are these fields being averaged over the timeframe of the month that contains each flood or is it averaged over the time period of the flood? This must be clearly stated because it muddles the interpretation of your figures.

I am concerned that if you take the mean over the month in which the flood occurs in the future simulation, the changes in fields like pressure and temperature are not just a result of the future perturbation, but are also being modified by the mesoscale processes within the storm-producing flood (and any other precipitation events that occurred during that month).

If these figures are just averaged over the time period of the flood (including when it is raining), again, you are introducing mesoscale modifications of the perturbations, including mesolows, outflows, and cool pools. It is impossible to evaluate these figures and what the changes in pressure, temperature, and CAPE mean if they are already being contaminated by the presence of storms.

Sorry for the confusion with the figure captions. On line 99-100, we state "This period is chosen as it includes the three major flood events in Table 1." And, as we defined in Table 1, the 2005 Oct Flood, 2006 May Flood, and 2005 June flood periods refer to the Northeastern U.S. flooding of October 2005, the New England Flood of May 2006, and the 2006 Mid-Atlantic United States flood, which has the duration from October 7th to 17th 2005, May 12th to May 20th, 2006 and June 23rd to July 5th, 2006. Therefore, in all analysis and corresponding figures (for example Figures 9-13), "2055 October", "2056 May" and "2056 June" refer to the period of October 7th to 17th 2005, May 12th to May 20th, 2006 and June 23rd to July 5th, 2006, respectively, in the PGW experiments for the perturbed climate of 2055/2056. The period mean refers to the average of the three flood periods within each month. We have added clarification in the manuscript.

We do show the simulations averaged over the flood period since our focus for the PGW experiments is to project how extreme events may change in the future. Our goal is to compare the results from different PGW experiments that include perturbations of different variables (T, U/V, geopotential height, sea level pressure) to determine the sensitivity of the PGW simulations to the variables perturbed. Surely the differences between the PGW experiments reflect not only the differences in the perturbations added to the initial and boundary conditions because the storms in the simulations are affected by the perturbations, which would in turn modify the atmospheric states including T, U/V, geopotential height, and sea level pressure. Hence the differences between the PGW simulations will be different from the original climate perturbations (from GCMs) added to initial and boundary conditions. Furthermore, the differences between the PGW experiments would depend on the storms being simulated because as noted above, the storms can provide feedback on the atmospheric states. This is why we studied three different storm events and concluded that the 2005 Oct Flood, which is caused by a cold frontal system, is more sensitive to the climate perturbations applied at the boundary.

More importantly, we confirm that the occurrence of the storms will not significantly change the impacts of sea level pressure perturbation on surface air temperature. For example, in Figure R1, we plot the simulated monthly mean 2-meter temperature difference between PGW_T_gp and PGW_T_SLP_gp during the months with lowest monthly mean precipitation (2056 January to March) in our simulation period. We can see that PGW_T_SLP_gp still has consistently lower 2-meter temperature over the sea. This illustrates that our conclusions regarding the SLP perturbation hold even in the absence of storms.

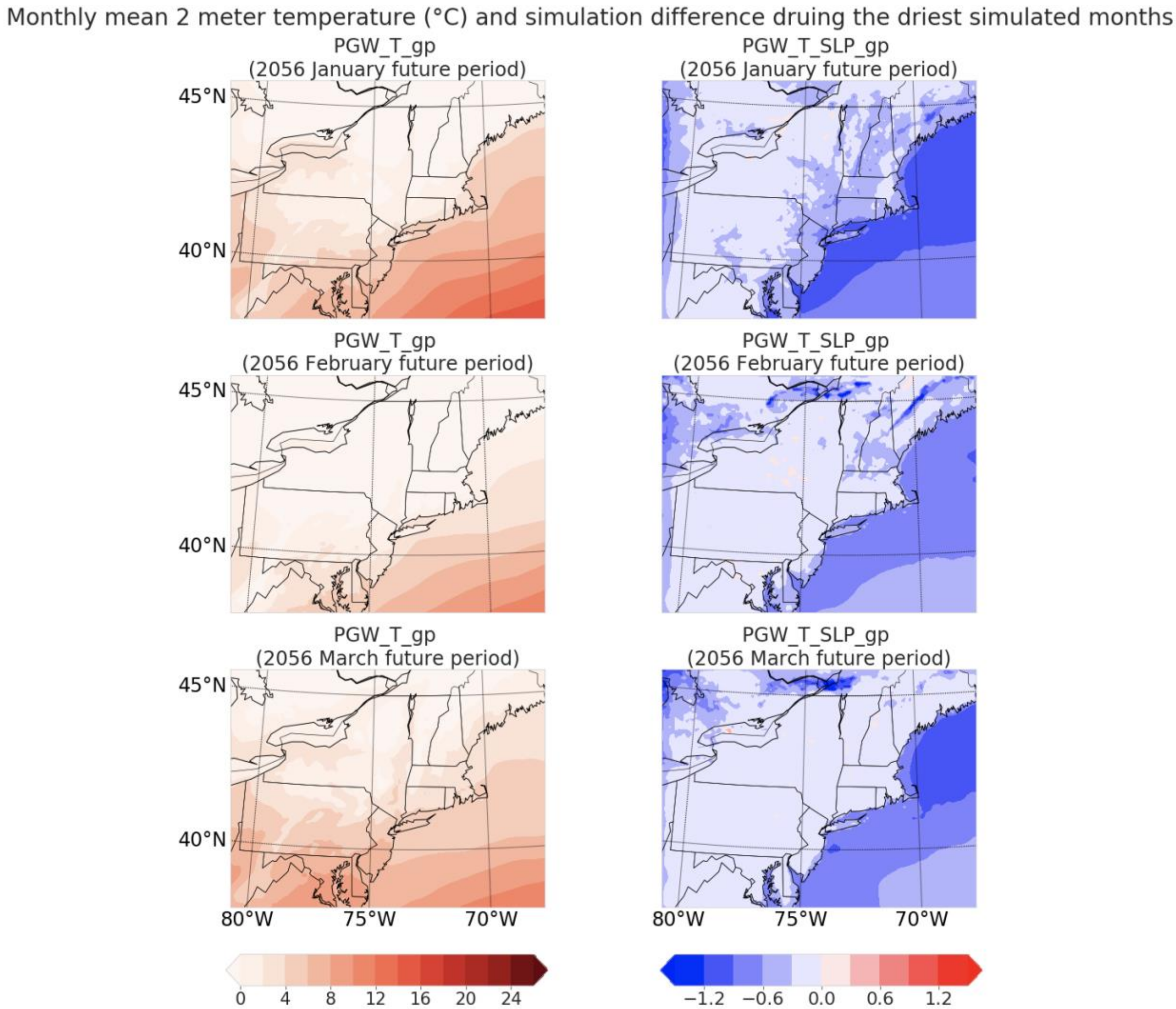


Figure. R1 Monthly mean 2 meter temperature (°C) and simulation difference during the driest simulated months

2. This paper lacks a complete understanding of the PGW literature and is missing some key papers, which is necessary if you are evaluating the utility of this method. Papers to include (not exhaustive):

Dougherty, E., and K. L. Rasmussen, 2020: "Changes in flash flood–producing storms in the United States. J. Hydrometeor., 22, 2221–2236, https://doi.org/10.1175/JHM-D-20-0014.1."

Dougherty, E., and K. L. Rasmussen, 2021: "Variations in flash flood-producing storm characteristics associated with changes in vertical velocity in a future climate in the Mississippi River Basin. J. Hydrometeor., 21, 671–687, https://doi.org/10.1175/JHM-D-20-0254.1.

Mahoney, K., D. Swales, M. J. Mueller, M. Alexander, M. Hughes, and K. Malloy, 2018: An examination of inland-penetrating atmospheric river flood event under potential future thermo-dynamic conditions. J. Climate, 31, 6281–6297, https://doi.org/ 10.1175/JCLI-D-18-0118.1.

Lackmann, G. M., 2013: The south-central U.S. flood of May 2010: Present and future. J. Climate, 26, 4688–4709, https://doi.org/10.1175/JCLI-D-12-00392.1.

Liu, C., and Coauthors, 2016: Continental-scale convection-permitting modeling of the current and future climate of North America. Climate Dyn., 49, 71–95, https://doi.org/10.1007/s00382-016-3327-9.
Prein, A. F, C. Liu, K. Ikeda, S. B. Trier, R. M. Rasmussen, G. J. Holland, and M. P. Clark, 2017: Increased rainfall volume from future convective storms in the US. Nat. Climate Change, 7, 880–884, https://doi.org/10.1038/s41558-017-0007-7.
Rasmussen, K. L., A. F. Prein, R. M. Rasmussen, K. Ikeda, and C. Liu, 2017: Changes in the convective population and thermodynamic environments inconvection-permitting regional climate simulations over the United States.

We thank the reviewer for providing a list of relevant papers. These papers provide important context for our study and support the gaps we identified and intended to address. We have cited these papers, along with ~20 other PGW-related papers already cited, in the revised manuscript.

3. I think the gravity wave noise section needs to be reevaluated. Previous PGW studies including Lackmann (2013) and Mahoney et al. (2018) have found that gravity wave adjustments in their studies are relatively small and only apparent during the early spinup periods. This makes me skeptical of what you are arguing, given that I do not see any evidence of gravity waves in your figures and the presence of wave-like noise in the presence of storms implies that you are seeing storm-generated gravity waves, which are physical and not just a model artifact.

Two of the papers mentioned above (namely, Lackmann (2013) and Mahoney et al. (2018)) do mention that the gravity wave adjustments are only strong during the early stages of the simulation. For example, Lackmann (2013) stated: “Any imbalance between the wind and mass field is sufficiently small to preclude strong gravity wave adjustment early in the future simulation.” Mahoney et al. (2018) stated: “Gravity wave adjustment between the wind and mass fields early in the simulations is accordingly short-lived.” Because these papers do not provide further explanations and figures, we cannot comment on their conclusions. We want to note that gravity wave noise is invisible in the mean field like daily mean, and it’s not clear if the authors examined their simulations at high temporal resolution.

In our simulations, we find that the gravity waves are not obvious in the period mean (for example as shown in Figures S6 and S7). However, if we look at the sea level pressure at high temporal resolution (e.g., the animation cited in Ln 203 and Figure R3 below, available at https://zenodo.org/record/6544880#.Y0YyQuzMJ9s), wave-like noise during the flood period in the PGW simulations is much stronger compared to the historical run. Also, the wave-like noise is more significant during storm events so we noted that “the observed noise is enhanced by strong advection during these extreme weather events, as it is much more obvious during these storm events.” More importantly, as shown in Figure R3, we can see the wave-like noise in the magnitude spectrum after applying Fourier Transform to the PGW runs (high-frequency signals are shown as the light circles in the magnitude spectrum) but we do not observe such a signal in the historical run. This further confirms that the wave-like noise does exist in our simulation during the flood periods and that it is not obvious (or present to the same degree) in the historical simulation.

Please refer to https://zenodo.org/record/6544880#.Y0YyQuzMJ9s

Figure. R2 The animation of sea level pressure during the 2005 October Flood and returned 2055 October Flood

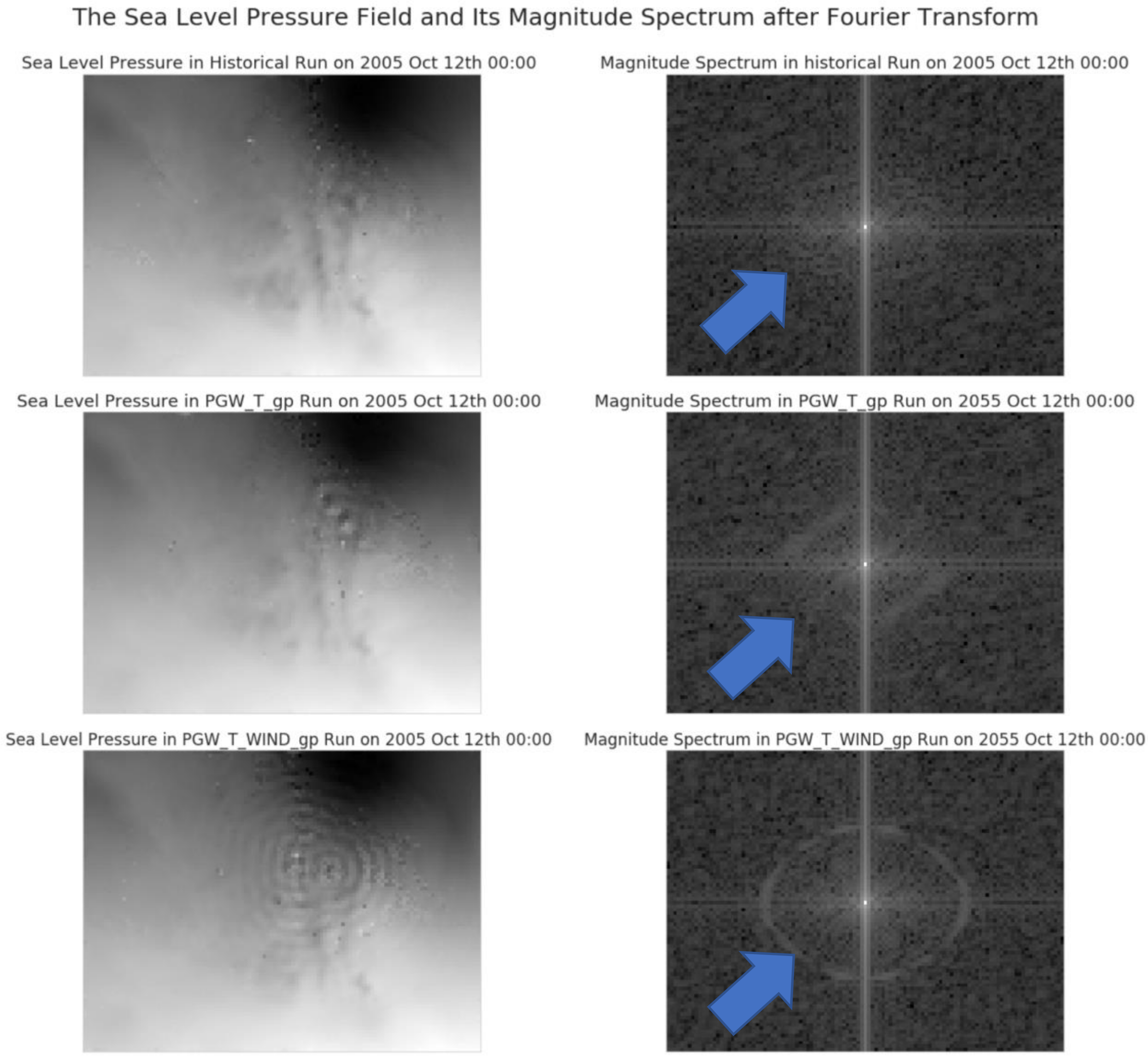


Figure. R3 The sea level pressure field and its magnitude spectrum after Fourier transform

Specific comments

Lines 9–11: I would be careful about drawing conclusions from the experimental design alone, as even running simulations in different versions of WRF can cause storm displacements.

Thanks for the suggestion. We replaced "experimental design" with "perturbation modification" here to make it more accurate.

Lines 20–23: I would recommend looking at the IPCC report and CORDEX studies to cite how confident future projections are of extreme precipitation in the Northeast, in addition to Melillo et al. 2014.

This citation has been added as you suggested.

Line 55: Please add Liu et al. 2016 to this list of PGW studies.

This citation has been added.

Lines 85–86: Please add Dougherty and Rasmussen (2020,2021), Prein et al. (2017), and Rasmussen et al. (2020) to this list.

Corresponding citations have been added.

Lines 89–91: I am curious why you decided not to perturb moisture in this study–can you please explain this in your methods?

We perturb the moisture condition by fixing the relative humidity and applying the warming perturbations to calculate the specific humidity instead of directly perturbing the specific humidity based on the GCM simulations. This is a common approach under the PGW method (e.g., the original PGW papers of Schär et al., 1996 and Frei et al., 1998), since an examination of future and historical relative humidity over the study region suggests that the relative humidity changes are not statistically significant. Perturbing specific humidity could also lead to relative humidity > 100%, which is unphysical. For these reasons specific humidity is not perturbed directly.

Line 103: Please add Liu et al. (2016) and Beck et al. (2019) to this citation: Beck, H. E., and Coauthors, 2019: Daily evaluation of 26 precipitation datasets using Stage-IV gauge radar data for the CONUS. Hydrol. Earth Syst. Sci., 23, 207–224, https://doi.org/ 10.5194/hess-23-207-2019.

Citations have been added.

Line 103–105: Please briefly list these parameterizations in the text.

The physics parameterizations have been added to Table S1.

Line 105: Why is CLM rather than Noah-MP used for a land model?
Lines 105–107: I think the choice of the land model is actually quite important. I understand it is not central to your study, but Barlage at el. (2021) showed the warm, dry bias in the Central U.S. from PGW simulations could be reduced by adding groundwater to Noah-MP in CONUS-wide PGW simulations.

There are a number of reasons why CLM was selected for this study: As described in the paper, both the physics parameterizations and the land model (CLM) we used in WRF are from CESM1, which is widely regarded as a high-performant climate model (Kay et al., 2015; Sillmann et al., 2013; Karmalkar et al., 2019). Additionally, using similar physics parameterizations and land model in the regional and global models help improve model consistency. We agree that the choice of the land model is quite important, but since the focus of this study is to evaluate the sensitivity of the PGW method to specific aspects of how PGW is implemented, we consider the choice of the physics parameterizations and land model as secondary to the specifics of the PGW approach. We have added a discussion section to mention some limitations of this study, including possible dependence of the sensitivity results to the specific physics parameterizations and land model used.

Lines 110–111: Can you explain why you chose to use 10 km for your inner domain? Many of the PGW studies use this method in order to simulate storms in a future climate at convection-permitting resolutions. At 10-km, you are not quite at convection-permitting scales so it would be helpful to understand the rational why this grid-spacing is beneficial for your study. A caveat that the cumulus parameterization is turned on would also be helpful, since this will likely greatly affect your results.

We also expect that conducting the simulation at a finer resolution, particularly at convection-permitting resolution, will improve the simulation accuracy; however, due to the large number of simulations we have to run, we choose a relatively coarse resolution along with a cumulus scheme. While we agree this is a shortcoming of the study that is largely necessitated by our limited computational resources, we do note that it is not uncommon in the literature to perform PGW simulations at analogous resolutions (a similar setup has been employed successfully in our previous studies: Ullrich et al., 2018 and Xue and Ullrich 2021b). Also, because we use the spectral nudging and suitable parameterizations, our simulations perform well as shown in Figure 2 and 3. As the aim of this study is to examine the response of the PGW simulations to different perturbation modification methods, the simulation accuracy itself is not the primary concern in this study. This shortcoming is now discussed in the paper in the newly added discussion section; we also hope to explore these conclusions at higher resolution in future studies.

Lines 112–114: Can you specify the spatial scales here? Did you employ nudging through the entire vertical domain or just above the boundary layer?

The spectral nudging uses guv, gt, gq and gph equal to 0.0003. The xwavenum and ywavenum are equal to 3. The nudging is only applied above the boundary layer.

Table 1: How were the start and end dates of the flood determined?

The start and end dates of flood periods are informed by the reference papers in the Meteorological Cause column in Table 1. To get the exact dates, we define the start of the flood as the day when regional daily mean precipitation over the inner domain is larger than 1.8 mm/day, and the end of the flood as the day when two consecutive days after that day is less than 1.8 mm/day.

Lines 138–140: Do you also vary the 2D 10-m winds, 2-m temperature in ERA5?

We do not modify the 2D 10-m winds but modify the 2-m temperature as CESM1 LE only provides the 10-meter wind speed project instead of the projections of u10 and v10 for both historical and future simulations. Modifying 2D 10-m winds (u10 and v10) is unnecessary because they are diagnostic variables and are only required in the initialization. Considering the length of our simulation and spin-up period, the impacts of 2D 10-m winds are neglectable.
https://forum.mmm.ucar.edu/threads/forcing-surface-winds.8666/
https://forum.mmm.ucar.edu/threads/running-wrf-with-cesm2-data.9965/#p20292

Lines 150–153: Why did you decide to use the CPC precipitation? It has nice global coverage but is much coarser resolution than your 10-km model data. I would suggest comparison to something higher resolution like Stage-IV or PRISM precipitation.

We use CPC precipitation because it's one of the most widely used and accepted observational precipitation datasets. We admit its resolution is coarser compared with our simulations and that's why we also use high-resolution and high-quality precipitation datasets – ERA5 and IMERG_V6.

Figure 3: Did you regrid these data to all be at the same grid-spacing?

We did not regrid the data. However, motivated by this comment, we have replotted Figure 3 with all observed/reanalysis data interpolated to the same grid as our simulations.

Lines 165–167: This is for enhanced warming over land for gridpoint perturbations if I am interpreting Fig. 4 left column correctly, right? Please indicate that you are referring to Fig. 4 in this statement for clarity.

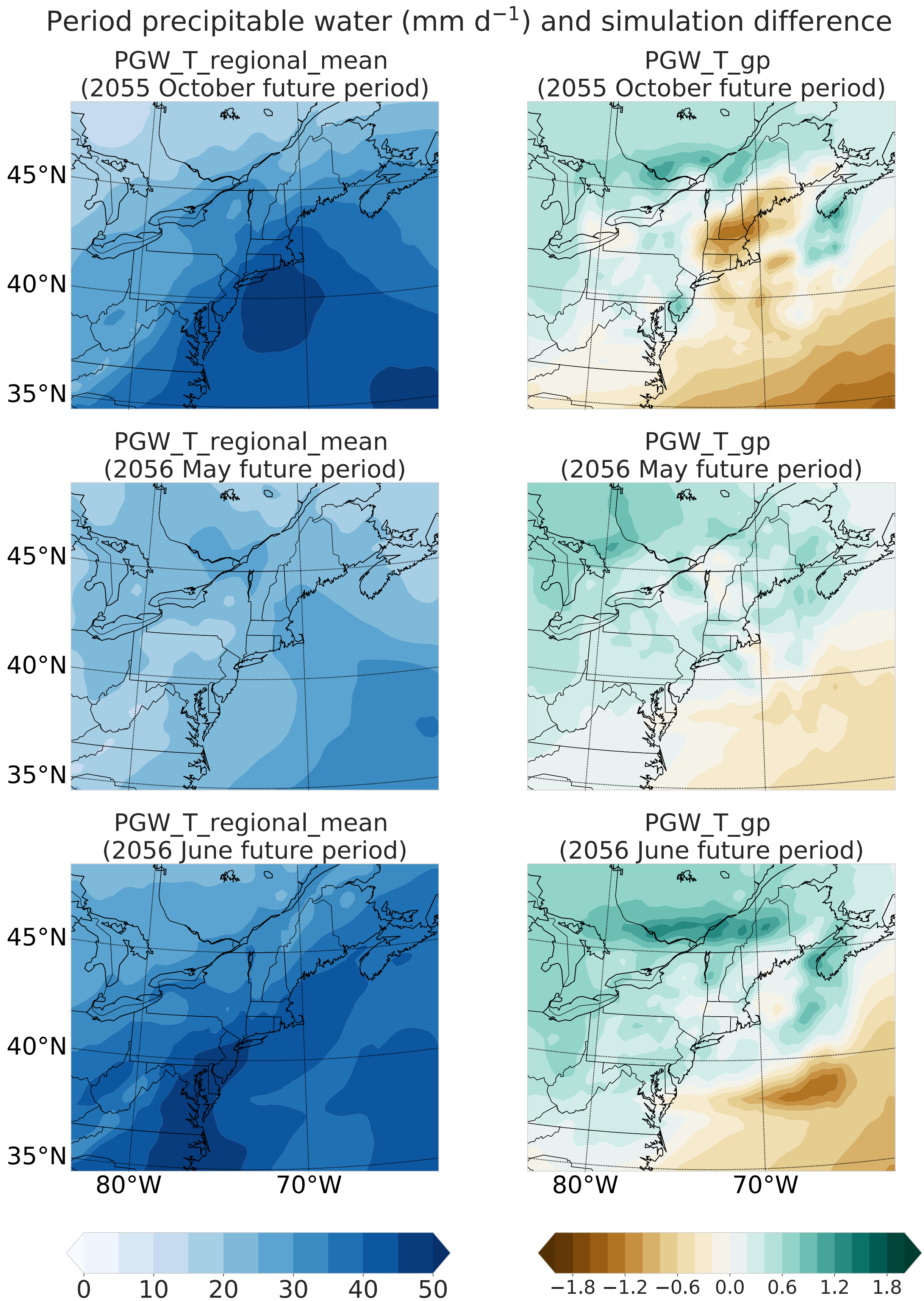
Thank you for pointing it out. We have mentioned it in the paper.

Lines 174–177: I don't see much difference in Fig. S9 or Fig. S10- can you show a difference plot to highlight this more clearly?

Sorry for the misunderstanding. When we refer to Figures S9 and S10, we mean to say that Figures S9 and S10 show that the 2005 October Flood (2055 October Flood) has a much stronger uplift (as shown by the green areas in the first row in Fig. S10) near the coastal region and much stronger onshore wind (as shown by the red areas in the first row in Fig. S9) compared with the 2006 May Flood (2056 May Flood) and the 2006 June Flood (2056 June Flood). In Figures S9 and S10, we can easily observe it by comparing the first row with the second and third rows.

Lines 181–182: Again, I am failing to see where precipitable water is much higher in the regional mean simulation than the gridpoint. Can you provide a domain mean in Figure S2 and put those numbers in each panel?

To better illustrate the precipitable water difference between PGW_T_regional and PGW_T_gp, we have plotted the figure below and added it to the supplement.



Period precipitable water (mm d^{-1}) and simulation difference
PGW_T_regional_mean
(2055 October future period)
PGW_T_gp
(2055 October future period)
PGW_T_regional_mean
(2056 May future period)
PGW_T_gp
(2056 May future period)
PGW_T_regional_mean
(2056 June future period)
PGW_T_gp
(2056 June future period)
45°N
40°N
35°N
80°W
70°W
0
10
20
30
40
50
−1.8
−1.2
−0.6
0.0
0.6
1.2
1.8

Figure. R4 (Left) Period mean precipitable water (mm) over the whole domain from the simulation with temperature perturbation at regional mean scale. (Right) Difference between the period mean precipitable water (mm) from the gridpoint perturbation simulation and regional mean simulation from the left column.

Line 203–205: When you are saying that dynamical fields are assumed to be unchanged, I think you are speaking very specifically about uniform temperature perturbations in the regional sense. However, I do want to clarify that you have to be careful about making these statements since this is not the case in all PGW simulations. I think it is more accurate to say that the PGW method assumes that the dynamical changes are much smaller order magnitude than thermodynamic changes (Liu et al. 2016; O'Gorman 2015). Furthermore, it is impossible to assume the dynamics don't change due to change in thermodynamics (i.e., temperature), because these interact with each other. For example, Dougherty and Rasmussen (2021) show that storms with stronger updrafts show greater increases in rainfall in 4-km PGW simulations, likely due to the latent heat feedback suggested by Trenberth (1999).

Sorry for the misunderstanding. By saying that dynamical fields are assumed to be unchanged, we mean that the dynamical field in the WRF input is not changed. Certainly, the modified thermodynamic fields will change the original dynamical fields as a result of mechanisms like the geostrophic adjustment. We have clarified this issue in the newly added discussion section and cited the reference you mentioned.

Lines 209–210: How do you know PGW_T_regional is overestimating precipitation when there is no future "truth" or observations to compare it to? What is your baseline for this statement?

While we cannot know for sure if PGW_T_regional overestimates the precipitation, we mean to say that PGW_T_regional overestimates precipitation compared with the gridpoint perturbation methods. However, there are good reasons to believe that PGW_T_regional is behaving unphysically. Because the PGW_T_gp employs the time-varying temperature perturbations at each gridpoint and pressure level (3D space + 1D time), it can capture the horizontal spatial variance of the temperature perturbation. But PGW_T_regional only uses the time-varying temperature perturbations at the regional mean scale at each pressure level (2D space + 1D time), and so does not include any information about the horizontal spatial variance of the temperature perturbation. There is ample evidence that the temperature perturbation over the land will be larger than the temperature perturbation over the sea, and so it is likely that the regional mean modification leads to future temperatures over the sea that are higher than what climate model projections indicate. The overestimated temperature perturbation in PGW_T_regional can be seen in Figure 4 (in the main paper). Since the relative humidity is fixed in our PGW runs, the change of specific humidity is determined by the temperature perturbation, and is subsequently larger in the regional mean simulation. Since precipitation in this region is largely driven by on-shore flow from the Atlantic, the enhanced specific humidity leads to enhanced vapor transport and increased moisture convergence (and subsequently higher precipitation in Figure 5 and 6 of the paper). Therefore, we claim for this choice of domain, the simulated precipitation in PGW_T_gp is more reliable compared with PGW_T_regional.

Line 226: Are you referring to the purple cross in this figure? If so, please state it as such.

Yes, the enhanced precipitation refers to the purple cross in Figure 6 and we have stated it.

Line 229: Is the reduction over the sea compared to the historical simulation or T-only experiment?

It's compared with PGW_T_gp and we have clarified it in the paper.

Lines 232–233: I think it's important to think about the larger view of why the wind magnitudes are stronger. The other two floods are warm-season events in May and June that have weaker synoptic forcing, whereas in October, synoptic dynamics and the forcing from it are quite strong. This would be my guess as to why the wind perturbation is stronger in the October flood than the others, and I think you should note that somewhere in your discussion

We agree that understanding why the onshore wind magnitude increase is stronger in October is important; however, in this case it's mainly determined by the wind projection and the corresponding perturbation from CESM1 LE. Because this study focuses on how the PGW simulations respond to different perturbation modification methods, explaining what causes the wind projections in GCMs are out of the scope of this study. Other papers (for example, Kulkarni et al., 2014) also show that the surface wind speed is projected to increase more in the winter over NEUS in GCMs' projections. We have briefly discussed this in the discussion section.

Line 235: Again, please be clear which marker you are referring to in Figure 6 when discussing these results.

"Referring to the difference between the purple cross and green triangle in Fig. 6" has been added.

Lines 262–263: That's not true- hypsometric equation states that a thicker layer = a warmer layer. However, by ideal gas law, $p = \rho RT$, pressure and temperature are proportional such that higher pressure = warmer temperature.

As we stated, WRF uses a hybrid vertical coordinate which is of roughly constant layer thickness at ground level, and so near-surface changes to SLP are incorporated at constant volume. In the hypsometric equation ($Z_2 - Z_1 = \frac{R \times T_v}{g} \times \ln\left(\frac{P1}{P2}\right)$), when $Z_2 - Z_1$ is fixed, an increasing P_1 indicates a decreasing T_v (mean virtual temperature). The ideal gas law describes the state of a hypothetical ideal gas (air parcel), and it's not applicable here because, although density varies inversely with layer thickness, air density is not immediately known.

Line 266–267: The colder temperatures and reduced precipitable water appears to be *true only over the sea surface.*

Note that precipitable water is defined as the depth of water in a column of the atmosphere instead of precipitable water in the surface atmosphere, and as we show in Figure 12 that there is a generally reduced precipitable water over the sea region instead of the surface of sea. In fact, most of the atmospheric moisture and precipitable water is present at the near surface. Therefore, a decrease in surface temperatures will cause the decrease in total precipitable water.

Line 27–274: Were the wave-like noises found only in the paper you cite? Because I do not see any in Figure S6 or S7.

Please refer to our response to the general issue 3. Figures S6 and S7 are the period mean plots and so do not exhibit the wave-like noise as this noise is not stationary; however, if we look at the animation of sea level pressure during flood periods (https://zenodo.org/record/6544880#.Y0YyQuzMJ9s), we can clearly observe the wave-like noise.

Figure 14: Is this just the 2056 May mean temperature from LE CESM1 without running any PGW simulations?

Yes, the black line illustrates the period mean temperature difference between the historical and future periods projected by the multi-ensemble mean of CESM LE.

Line 290–292: I would add "stronger synoptically-forced systems" after frontal systems.

This has been added.

Technical comments:

Table 1: Please replace "huge" with "large", since the former is too colloquial.

Corrections have been made.

Figure 2:
The top left panel appears to have a different color scheme than the rest of the figure. Please fix this.
Caption: Is the average temperature the average monthly temperature? If so, please include that in the figure caption.

Thank you for pointing it out. As suggested by reviewer 2, we plotted the temperature difference in the second and third columns in Figure 2. The average temperature is the period mean temperature during three flood periods (the periods we defined the Table 1). We have replaced the average temperature with period mean temperature in the caption.

Lines 170–172: This is confusing as written. I would suggest rewriting to say "increase *from 12.69 mm/d to 13.86 mm/d in Oct 2005, 7.19 to 7.83 mm/d in May 2006, and 7.43 to 7.88 mm/d in June 2006)".*

We have modified the sentence as you suggested.

Figure 4:
I would change the label of the left-column figures to PGW_T_gp - PGW_T_regional since you are showing a difference.
Please flip the colorbar so it matches the intuition that red is warmer and blue is cooler.

In all period mean and simulation difference figures (like Figures 2, 3, 5, 8 and 9), the left column shows the baseline simulation, while the rest of the columns indicate the differences compared with the baseline (as stated in the captions). Otherwise, the subtitles might be too long. For example, for the difference between *PGW_T_WIND_SLP_gp and PGW_T_gp, the subtitle will be PGW_T_WIND_SLP_gp - PGW_T_gp which is lengthy and redundant.*

Thanks for your suggestion on color bar. We have reversed it.

Figure 7 should be the last figure given that the first time it is mentioned is on Line 278.

We have relocated it to the correct position.

Figure 10: I think you mean hPa instead of "(mm/d)" in the figure caption, right?

We have corrected the caption.

REVIEWER COMMENTS - Referee Reviewer 2

This article deals with a hot topic that may suit HESS. This study examines the sensitivity and robustness of the PGW method over NEUS by conducting multiple PGW experiments. In addition, several PGW experiments are conducted to answer the three key questions related to the application of the PGW method. The results may help further understand the impact of different PGW simulations on climate projections. Overall, I think it is a pretty good job. However, some scientific or presentation issues need to be carefully addressed. Therefore, the reviewer recommends that this manuscript should be accepted after minor revision.

Dear reviewer, thank you very much for your appreciation and suggestions. Certainly, these suggestions are very helpful in improving the scientific rigor and presentation of this work. We modified the paper correspondingly and please check the point-to-point responses below.

Minor revision:

It is recommended to use consecutive line numbers.

The line numbering is provided by the HESS LaTeX template. In the posted PDF line numbers appear to be consecutively numbered.

It is suggested that some necessary statistical parameters should be provided to quantify the difference in precipitation and temperature simulation performance of different schemes.

Thank you for your suggestions. We employed the K-S Test, which aims to determine if two data have the same distribution, and Student's t-test, which aims to examine if two data have the same mean value among all PGW simulations during the total simulation period. We chose the PGW_T_gp as the baseline and compared it with other PGW simulations on the regional mean precipitation and 2-meter temperature. Results show that the p-values from both the K-S Test and t-test are much larger than 0.05 in all cases, which indicates we cannot reject the null hypothesis that the regional mean precipitation and 2-meter temperature of PGW_T_gp and other PGW simulations have the same distribution and mean values at the 95% confidence level.

Further we conducted similar statistical tests on the precipitation and 2-meter temperature at each gridpoint. The results show that, except PGW_T_ZG_gp, the p-values of both K-S Test and t-test between all other PGW simulations and PGW_T_gp are nearly zero (much less than 0.05), indicating evidence against the null hypothesis that the precipitation and 2-meter temperature at each gridpoint of PGW_T_gp and other PGW simulations (except PGW_T_ZG_gp) have the same distribution and mean values at the 95% confidence level. However, the p-values of these two tests on precipitation and 2-meter temperature at each gridpoint between PGW_T_gp and PGW_T_ZG_gp are still much larger than 0.05 (0.9997 and 0.7403 for K-S Test and t-test), indicating that, even at gridpoint scale, PGW_T_ZG_gp's precipitation and 2-meter temperature still have the same distribution and mean values as PGW_T_gp.

This lends evidence to our claim that modifying ZG (geopotential height) in PGW simulations makes little difference on the final simulation and is not necessary. Also, the statistical tests further confirm our conclusion that different PGW methods will displace the weather events (the precipitation and 2-meter temperature at each gridpoint) but have a few impacts on the regional mean meteorological fields (the regional mean precipitation and 2-meter temperature).

We have added the content above to the newly added discussion.

The figures legend/caption is not self-explanation, which should be improved. In addition, many subgraphs in Figures 2 and 3 are very similar, making their differences challenging to identify. It is suggested to adopt the form of Figs.8-12 or add the statistical parameters mentioned in question 2.

Thank you very much for pointing it out. We will improve the legends and captions to make them clearer. In Figures 2 and 3, we will plot the differences between our simulation and other datasets. Attached is the updated the Figure 3.

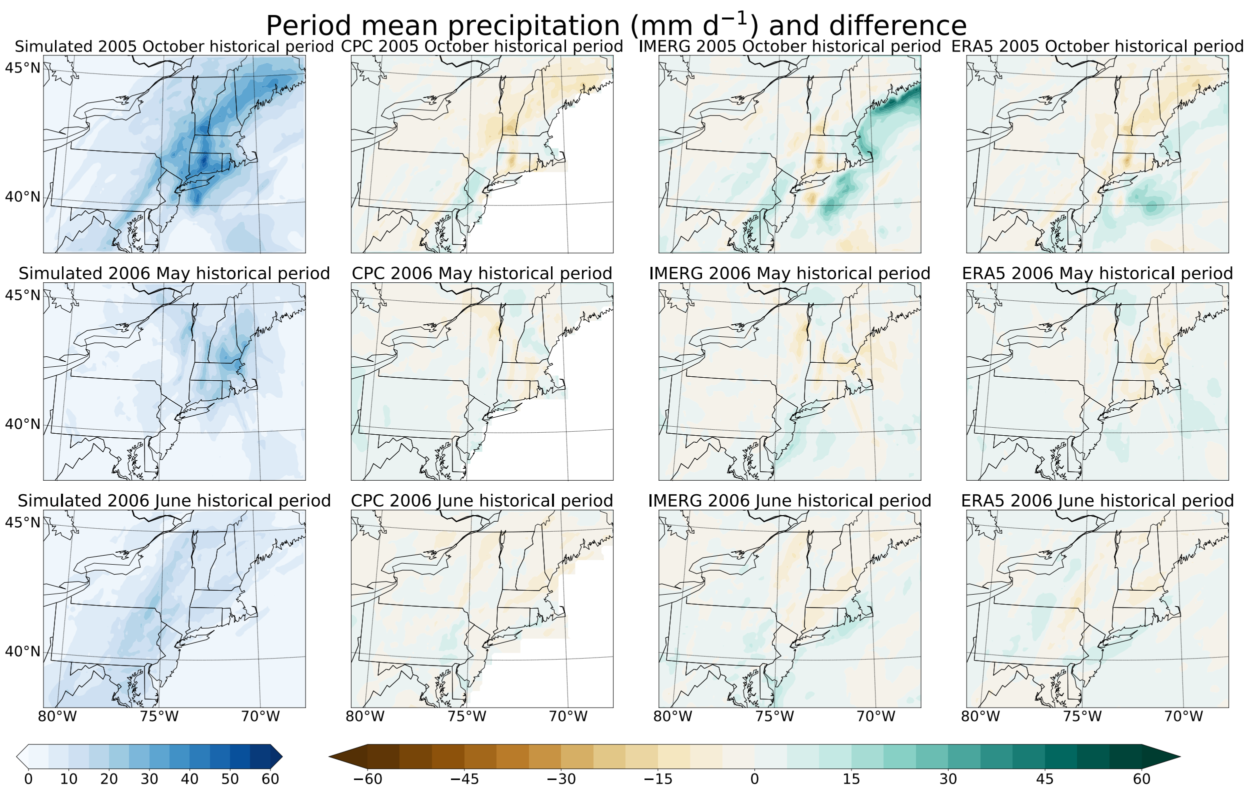


Figure R1 Period mean precipitation (mm/d) and difference (the difference is calculated as the difference between each reanalysis/observational data and our simulation)

Figure 1 recommends that the macro location comes from the continent to geologically locate. The locations of the three regions should be marked in Figure 1

Sorry, we're not sure if we understand this suggestion clearly. The three storms pass through our domain, along with their associated precipitation, as shown in the references in Table 1. In Figure 5 and Figure 8 (in the main paper), we can observe the distribution of precipitation during three storms and speculate about their centers; however, because they are noisy and spread out, it's difficult to directly specify the locations of these three storms and associated flooding on the domain figure.

Please clarify the "returned" flood period (2055 April to 2056 July). Is this the 50-year return period for floods considered? Why not consider using other periods?

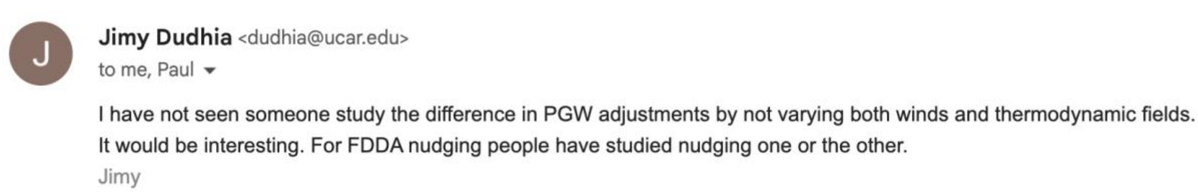
The "returned" flood period (2055 April to 2056 July) in this paper means the scenario when the same larger-scale circulation and dynamic fields of the historical 2005 April to 2006 July flood period reoccurred under the RCP8.5 warming scenario in the middle of this century. The historical 2005 April to 2006 July flood period includes three major extreme precipitation / flood events over the NEUS. By simulating this consecutive period, we can reduce the spin-up period needed. We choose the middle of this century (2050s) as the simulated future period because it's widely used and studied.

Please check whether Figure 13 is incorrect. Also, please adjust the color bars in Figures 11, 3, and S1, as some of them are not valid.

Thanks a lot for pointing this out. Figure 13 should be correct and we changed its first column's color bar to improve its appearance; however, the caption of Figure 13 has one problem – it should be "over the inner domain" instead of "over the whole domain." We also adjusted the color bars in Figures 3, 11 and S1 as you suggest.

The authors should rearrange the structures of the manuscript. The discussion is missing, maybe the result should change to result and discussion. You must buy the results from other similar studies in the discussions section. It is worth completing comparisons or differences with similar studies in other regions of the country or the world with related studies.

Thank you for the suggestion. We will restructure the paper and add a discussion section. However, because our PGW method sensitivity study is novel, it is difficult to contextualize it in the literature. In fact, we haven't found any similar papers (we even consulted with Dr. Jimy Dudhia – one of the authors of WRF and he told us that this study is very novel). But we will try to elaborate the discussion with enough reference papers.


Jimy Dudhia <dudhia@ucar.edu>
to me, Paul

I have not seen someone study the difference in PGW adjustments by not varying both winds and thermodynamic fields. It would be interesting. For FDDA nudging people have studied nudging one or the other.
Jimy

The authors should further clarify the shortcomings and limitations of the study.
Please check the format of the references to meet the journal's requirements.

Agreed. We have identified the several shortcomings or limitations of the study and will discuss these in the text: for example, our simulation has a coarse resolution and uses cumulus scheme which reduce the model performance; the number of flood events studied is limited.

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