Xue and Ullrich, 2022 "Sensitivity of the pseudo-global warming method under flood conditions: A case study from the Northeastern U.S."

Summary:

This study evaluates the sensitivity of the PGW method to regionally uniform vs. grid-point 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.

General:

- 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.
- 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 ther- modynamic environments in convection-permitting regional climate simulations over the United States. Climate Dyn., 55, 383–408, https://doi.org/10.1007/S00382-017-4000-7.
- 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 spin-up 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.

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.
- 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.
- Line 55: Please add Liu et al. 2016 to this list of PGW studies.
- Lines 85–86: Please add Dougherty and Rasmussen (2020,2021), Prein et al. (2017), and Rasmussen et al. (2020) to this list.
- Lines 89–91: I am curious why you decided not to perturb moisture in this study–can you please explain this in your methods?
- 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.
- Line 103–105: Please briefly list these parameterizations in the text.
- 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.
- 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.
- 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?
- Table 1: How were the start and end dates of the flood determined?
- Lines 138–140: Do you also vary the 2D 10-m winds, 2-m temperature in ERA5?
- 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.
- Figure 3: Did you regrid these data to all be at the same grid-spacing?
- 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.
- 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?
- 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?
- 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 <u>all</u> 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).
    - O'Gorman, P. A., 2015: Precipitation extremes under climate change. Curr. Climate Change Rep., 1, 49–59, https://doi.org/ 10.1007/s40641-015-0009-3.
    - Trenberth, K. E., 1999: Conceptual framework for changes of ex- tremes of the hydrological cycle with climate change. Climatic Change, 42, 327–339, https://doi.org/10.1023/A:1005488920935.

- 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?
- Line 226: Are you referring to the purple cross in this figure? If so, please state it as such.
- Line 229: Is the reduction over the sea compared to the historical simulation or T-only experiment?
- 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
- Line 235: Again, please be clear which marker you are referring to in Figure 6 when discussing these results.
- 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.
- Line 266–267: The colder temperatures and reduced precipitable water appears to be true only over the sea surface.
- 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.
- Figure 14: Is this just the 2056 May mean temperature from LE CESM1 without running any PGW simulations?
- Line 290–292: I would add “stronger synoptically-forced systems” after frontal systems.

Technical comments:

- Table 1: Please replace “huge” with “large”, since the former is too colloquial.
- 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.
- 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)”.
- 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.
- Figure 7 should be the last figure given that the first time it is mentioned is on Line 278.

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