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Peer Review File

Moisture from US Corn Belt Fuels More Intense Convective Storms

Corresponding Author: Dr Zhe Zhang

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Zhang,

Your manuscript titled "Moisture from US Corn Belt Fuels More Intense Convective Storms" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised.

We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns. Specifically, the revised manuscript must meet the following editorial requirement: "Make full discussions on factors influencing simulations of mesoscale convective storm tracks, particularly the length of the tracks, and the reliability of the improved model for simulations in comparison to observations". We will assess a revised manuscript addressing these points before making a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Min-Hui Lo, PhD

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript by Zhang et al. adds to the understanding of managed land-atmosphere interactions and their impact on convective storms within the US Corn Belt. The methodological approach combining a high-resolution convection-permitting model with an advanced WVT and land surface scheme including MMF, dynamic crop and irrigation management represents a substantial advancement in the field of Earth system modeling. The findings provide compelling evidence for the direct influence of human agricultural activities on storm characteristics and intensity. However, several aspects warrant revisions, e.g., a more nuanced interpretation of causality within the regional modeling framework, and a precise discussion of the broader implications.

Specific comments:
• The manuscript quantifies the combined role of irrigation, crop, and shallow groundwater. It's worth noting that only a small fraction (~10–15%) of the Corn Belt is actually irrigated, with most areas relying on rainfall and shallow groundwater. Given that there are other experiments (GW-only, GW-Crop), it would be helpful to discuss what each component contributes. The paper mostly treats GW-Crop-Irr as a whole, but the title and implications often refer to 'agriculture' or more generally. Is it mainly crop and irrigation or does shallow groundwater alone play a major role? For instance, crop growth may just shift ET timing to July, improving moisture supply during peak MCS season. Groundwater might sustain soil moisture in early summer, while irrigation adds water in the late-season, especially in the western Corn Belt. A comparison/clarification of which component matters most would help generalize these findings beyond this region.
• The use of a regional model means that its domain boundaries significantly influence the climate system within, potentially generating spurious moisture convergence near the edges. This could lead to an overestimation of convective activity and limit the ability to fully capture large-scale climate feedback. From Fig. 1f, it appears much of the increase in MCS frequency is right over Iowa/northern Illinois (the core Corn Belt) where Stage IV had a hotspot that the Baseline simulation missed, implying the GW-crop-Irr simulation successfully helped to simulate storms in this central hotspot. However, there are also some overestimations in the far north and southeast of the domain. The authors attribute the northern overestimation to a combination of missing Stage IV data and model boundary effects. The overestimation in the Southeast US is also visible. Is this again a boundary issue? A discussion on this in the paper would help, otherwise, it could give the impression of systematic over-estimation due to the model's inherent limitations.
• While the study quantifies how Corn Belt agriculture intensifies local convective storms, it does so within a regional model domain. Recent work (e.g., McDermid et al., 2023, Nature Reviews Earth & Environment) has shown that irrigation can affect atmospheric circulation and precipitation far downstream, even across continents. This raises the possibility that agricultural impacts may extend well beyond the Corn Belt. It would be helpful if the authors briefly acknowledged this spatial limitation and noted that future studies using global convection-permitting models could explore such remote or tele-connected effects.
• The study does not appear to consider tile drainage, which is widespread across the US Corn Belt and significantly alters soil moisture, ET, and surface fluxes. The omission of this practice represents a significant gap, as tile drainage fundamentally changes the soil moisture conditions that are central to the study's proposed feedback mechanism. Since the paper focuses on how land surface processes shape storm development, it would be helpful to acknowledge that subsurface drainage could counteract some of the irrigation effects or shift the seasonality of ET, particularly in wetter periods. Including or at least discussing tile drainage would improve the completeness of the land-atmosphere representation. Along with a recommendation for future studies, to consider incorporating tile drainage to provide a more complete and realistic representation of the agricultural landscape.

Minor comments:
• Line 20-21: an extra word 'influence'
• Line 149: missing a 'to'
• The simulation setting is in the supplementary, however, it could be clearer, e.g., Irrigation management means no irrigation?
• Review use of strong causal language, such as 'directly intensifying' and consider if a slightly more nuanced phrasing, like 'significantly amplifying' might be more appropriate. This refinement would acknowledge the complex interplay of various atmospheric and land-surface factors as well as the limitations.
• The statement that agriculture 'directly intensifies convective storms... potentially leading to cascading hazards (hail, tornadoes, flooding)' should be more cautiously worded. The study clearly supports increased storm duration and rainfall intensity, which strengthens the case for flood risk. But since wind shear doesn't change (as shown in Supplementary Fig. S23), and hail or tornadoes weren't directly analyzed, those hazard links are speculative. It would be more appropriate to focus on the well-supported flooding implications and rephrase the conclusion to better match the study's actual scope.

Overall, the conclusions are likely to influence thinking around land-atmosphere feedbacks and agricultural impacts on extremes, particularly within regional climate modeling and earth system modeling communities. The statistical analysis is robust and the methodology appears reproducible given the level of detail provided in the paper and supplement.

Reviewer #2 (Remarks to the Author):

This paper provides a thorough explanation of the mechanisms by which irrigation promotes the development of MCS, addressing a gap in previous studies that primarily focused on the increase in MCS without detailing the formation mechanisms or moisture sources. The application of a comprehensive irrigation system to convective-permitting resolution is commendable. However, I have a few concerns:

1. The study clearly illustrate the irrigation promotes the MCS growth, but how well it can captures the MCS or whether it can "more realistically" capture the MCS. Figure 1 shows it can better capture the lifetime and speed, and the supplementary files shows it can well capture the land processes (ET, Td, etc.), but I still have no idea about the general (convective) precipitation pattern. And even when seeing the improvement in frequency (Figure 1e-f), the underestimation becomes overestimates with less significant dots, but I'm not sure about the overall bias. If I look at Figure S4 for each year, I feel like

the GW-Crop-Irr model has worse performance in 2011 normal year, and it doesn't have significant changes in 2010/2012. This let me raises doubt about the reliability of the improved system about whether it can really reflect the MCS mechanism.
2. It seems like the improvement mainly improve the lifetime in wet years and speed in dry years? Any potential explanation for that?
3. I'm not sure about the main intension of showing the case study in Figure 2. Key information here is irrigation contributes more along the MCS track but as you mentioned, this MCS is not very "well captured". So why this specific case but not other cases is chosen?
4. Is it possible to show the density map of MCS?
5. For experiments design, 4 experiments are introduced, while only two is used in this study, so I wonder the implications of mentioning the other two cases?
6. Since you are using Crop-Irr-Groundwater, I wonder is pumping incorporated?
7. In figure 5, (b) shows a stronger upper level divergence, but based on Figure S11, wind anomaly is only to one direction but not a divergence.
8. A followup is, if the low-level wind moves further straightforward and high-level wind moves backward, like a shear force, is the convective cell strentched and lean more?
9. I wonder x-axis numbers in Figure 5, it's used for showing the exact position of the convective cell and the structure scale?
10. For the top 20% cases, is it the top 20% of each experiment? In that case, the selected experiments might be different between the experiments and each experiment may even containing different number of selected cases? Then is it a still fair comparison?
11. The reference label to the supplementary figures should be rechecked. Many of them are wrong, such as Line 188 may be you are referring to Figure S22(?), Line238 referring to Figure S11(?), and Line 457 referring to Figure S14(?). Please recheck the order and even reorder if possible.
12. Please check the details of all the figures, including axis label for both x and y, units, etc. For instance, Figure 1c y-axis is halfly missing, Figure S2 "^-3" could be missing. And keep using one format between "NoGW" or "Baseline" or "NoGW Baseline" in the figures and the subcaptions.
13. The meaning of some supplementary figures are not that clear. Like the Fig S22 , the dot I assume it should be vector differences?

Reviewer #3 (Remarks to the Author):

Review of “Moisture from US Corn Belt Fuels More Intense Convective Storms” by Zhang et al.

This is a very interesting modelling paper about the role of soil moisture on atmospheric convection, which is a long-lasting research topic in the field of climate and weather science, as well reminded in the introduction. The study focuses on the US Corn Belt where soil moisture, evapotranspiration, and their evolution, are controlled by complex interactions between human agricultural activities (crop expansion and irrigation) and the natural supply from groundwater. More specifically, the paper compares two regional land-atmosphere simulations differing by the way the land surface is parametrized: either allowing complex interactions between soil moisture, crop development, groundwater, and irrigation; or using a baseline configuration in which crop LAI is prescribed and does not respond to soil moisture, itself independent from groundwater and irrigation.

The novelty is twofold: firstly, in the use of a convection-permitting atmospheric model, at a high enough resolution to permit the explicit description of convective processes, without simplified sub-grid parametrizations required at coarser scales; secondly, in the use of an Eulerian water vapor tracing algorithm, to quantify which part of the precipitation in the Corn Belt originates from local evapotranspiration. The resulting simulation are extremely CPU and storage intensive, and were only run in May-June-July-August (growing season with frequent mesoscale convective events), for three consecutive years. This very advanced setup has already been used in a paper recently published in the PNAS by the same group of authors (Zhang et al., 2025), focusing on the quantification of moisture recycling within the Corn Belt, and on the relative contribution from groundwater, crop dynamics and irrigation.

The present paper offers a very important complement by focusing on mesoscale convective systems (MCSs), for which the convection-permitting feature is particularly relevant. These MCSs are important for both the weather and climate communities, as they are (i) major drivers of land-atmosphere coupling, (ii) causing major hazards, and (iii) usually enhanced by global warming. Consistently with the results of their previous study, the authors find that the additional moisture flux provided by the groundwater-crop-irrigation system results in more intense convective storms, making the simulation more realistic compared to precipitation observations from the StageIV database. This result is well expressed by the paper’s title, and well supported by the proposed figures, which are clearly explained by the text. This makes the paper worth publishing in Communications Earth & Environment, but I would suggest one addition (optional), a strengthening of the discussion, and a few minor revisions, as detailed below.

My main recommendation is to develop what relates to the length of the MCS tracks. Figure 2 provides a very powerful analysis of this feature for two days in June 2012, with a comparison to the StageIV data. Both simulations exhibit one event, which is longer and more intense with the GW-Crop-IRR than the Baseline setup. What I find very interesting is that this makes the GW-Crop-IR simulation closer to the StageIV observation, if one lumps its two tracks into one. Here, I first wonder if Fig. 2a-c could not be moved into Figure 1. Then, I am curious about the MCS track length, regardless of the number of events: are they statistically longer in GW-Crop-IRR? Hence closer to the mean track length that can be extracted from

StageIV? If this analysis makes sense for an atmosphere physicist, which I am not, it would probably strengthen the conclusion that the additional moisture supply makes the simulated MCS closer to observation.

Other suggestions:

L133-136, “overestimation of MCS frequency”: three good reasons are given here, including uncertainties in the MCS tracking algorithm. Can the authors expand a bit? Do these uncertainties include interactions between the window used to track the MCS and the boudaries of the simulated domain? Would it be possible to define a buffer zone along these boundaries in which MCS are uncertain? If so, how do the MCS statistics compare if calculated inside the full domain, or in the domain minus the buffer zone?

This overestimation should be reminded in the Discussion, like the fact that all model results are model-dependent and need confirmation by other models and observation-based evidence.

L139-141, “Additional process-based model validation”: this part is interesting but is uniquely supported by figures from the supplementary, which would deserve to be referenced by their number in the main text. I suggest replacing “surface humidity” by “near-surface atmospheric humidity” to prevent from confusion with soil humidity. The caption of Fig. S16 and S17 should read “same as Fig. S15” instead of S14. Regarding groundwater (Fig. S14), the caption does not explain which simulation is depicted in panels b-d. And since there is not comparison between the match of the two simulations with the GGMN simulation, this figure might be omitted. Finally, I am not sure « process-oriented » is relevant here, at least without further elaboration in the text.

L172-174: since the Corn Belt moisture contribution to the local MCS is stronger in GW-Crop-IRR than in the Baseline simulation, can we conclude (and write here) that GW-Crop-IRR is more realistic? Do we have an observation-based estimation of the Corn Belt contribution to extreme precipitation?

L191-193: similarly, does the paper provide observation-based estimates of “moisture contribution to MCS precipitation” and “MCS precipitation proportion in total precipitation”? If not, the end of the sentence about improved performances should be rephrased to avoid overstatement.

L212: the increase of rainfall intensity near the MCS center is not particularly sharper in GW-Crop-IRR than in the Baseline, so the sentence should be rephrased. It might even underline that the intensity differences are quite small, in contrast to the Corn Belt contribution to MCS precipitation.

Discussion:

L283, end of first paragraph summarizing the main results: it would be important to add that the intensification of MCS makes GW-Crop-IRR more realistic with respect to StageIV precipitation.
L300-305: I wonder if 1) and 2) are really two different impacts, and if they are, my feeling is that 2) causes 1) so should be mentioned first.

L306, “cold pool dynamics”: not being familiar with this concept (also mentioned in L254), I would appreciate a short explanation, using Fig. 5 if relevant.

L311-313: I wonder if it would not be clearer to insist on the fact that a low roughness length is an important driver of tornado activity.

L316: please specify if you speak of differences in crop height or roughness length.

L317: a difference of 1 to 10 m between grassland and tropical forests seems too small if we speak of crop height, and may be too high for roughness length.

L319-321: this sentence is not grammatically correct and not particularly clear. I suggest the following rephrasing: “This suggests the land surface contribution mostly concerns the local boundary conditions.”

Figure 5: It would be useful to give indications about the orientation of the diagrams (West and East). If the color shading is based on simulated MCS, please specify which ones (20% composite?).

L338-341: It would be useful to add here which driver, among groundwater, dynamic crops, and irrigation, is the most impactful based on ref 22. This could lead to discuss how realistically these three drivers are parametrized in the study, and if other important moisture sources may be missing, such as the Great Laurentian Lakes.

L338-342: One sentence to discuss the limitations of such a complex modelling exercise is not enough. The effect of the lateral boundary conditions would be usefully discussed with a reference to Fig S1. In particular, I wonder if it is well suited to study the Corn Belt in a domain where the Corn Belt is not at the center. There are many more limitations, regarding both the land and atmosphere components, and they should be reminded as they generate uncertainties, and call for confirmation of the present results using other models.

The last paragraph of the discussion (L346-366) refers to some studies that concur with the present one. It would be useful to specify if they are all targeted on the US Corn Belt. I also recommend to include other important references, which rely on field campaigns designed to study the atmospheric impact of irrigation: the ones of the GRAINEX field campaign on the influence of irrigation on convective precipitation in Nebraska (at least 5 papers since 2022); the paper of Lunel et al. (2024), which compares simulations with a convection-permitting model to observations from the LIAISE experiment in Spain.

L355: The reference to the land-atmosphere coupling hotspots of Koster et al. (2004) is interesting to hint at some generalization, and could be developed a bit. In particular, one important common point of these hotspots is to be transitional areas with respect to soil moisture. Yet, soil moisture is higher in GW-Crop-IRR than in the Baseline: does the Corn Belt remain a transition area in GW-Crop-IRR or does it become too wet for this? Does the higher convection in irrigated areas relate to stronger land-atmosphere coupling (in the sense of higher correlation between anomalies of soil moisture and anomalies of precipitation, as used by Koster et al., 2004), or in contrast to weaker land-atmosphere coupling, which does not prevent from a larger quantitative effect on precipitation?

L355-359: Here starts the opening of the Discussion, which could deserve a specific paragraph with a line break. The first two sentences are about future crop expansion and irrigation, but this remains very general. Yes, increased irrigation can be supplied by deep fossil aquifers, but they are massively depleted and their volume or pressure may become limiting (e.g. Scanlon et al., 2012; Hilton and Jasechko, 2023). Climate change will also affect groundwater availability and influence future irrigation prospects (Wu et al., 2020 ; Arboleda-Obando et al., 2025). These elements could be worth mentioning, as they introduce strong uncertainties on future storms in the Corn Belt.

Typos and technical comments:

Fig. 1: the label of the y-axis of panel c is truncated ; I couldn't find where "The number of MCS is printed in parentheses" ; the caption should state that the thick black contour delineates the US Corn Belt ; L153 : "shows" instead of "showing".

L162: "shows" instead of "illustrates"

L164: "indicates" instead of "indicate"

174: "presents" instead of "present"

L213-218: the percent changes written here are not the same as the values printed in Fig. 3, can you explain why you use ranges in the text and how they were obtained? The   in L213 should probably be a -.

Fig. 2d-f, Fig. 3, Fig. 4: I suggest reminding in the caption what MCS are considered to draw these figures? I understood they are based on the 20% most extreme MCS, at least for the composites of Fig. 3, but I would like to be sure. And I would also like to know if Fig. 4 is based on the composite MCS?

Fig. 4, L266-267: I couldn't find how the surface temperature gradient is calculated, can it be added, either in the caption or in the Methods?

Fig. 4: if space allows, I would be interested by the panels showing the differences c-a, and d-b.

L289: "What is" instead of "What's"

Methods:

The figures must be given a consistent notation: either Supplementary Fig. X, or Fig. SX.

Simulated domain and boundary conditions: do the Great Lakes provide some moisture?

Irrigation: where is water taken from to irrigate? Can water be limiting for irrigation?

L467-468: what does « wet soil moisture » refer to? The soil moisture of the water year 2010? If so, please rephrase.

Cited references:

Arboleda-Obando, P. F., Ducharne, A., Cheruy, F., and Ghattas, J.: Joint evolution of irrigation, the water cycle and water resources under a strong climate change scenario from 1950 to 2100 in the IPSL-CM6, Earth Syst. Dynam. Discuss. [preprint], https://doi.org/10.5194/esd-2024-41, in review, 2025.

Hilton, A., and Jasechko, S. (2023). Widespread aquifer depressurization after a century of intensive groundwater use in USA, Science.

Lunel, T., Boone, A.A. & Le Moigne, P. (2024). Irrigation strongly influences near-surface conditions and induces breeze circulation: Observational and model-based evidence. Quarterly Journal of the Royal Meteorological Society, 150(762), 2798–2819. Available from: https://doi.org/10.1002/qj.4736

Scanlon, B. R., et al. (2012). Groundwater depletion and sustainability of irrigation in the US High Plains and Central Valley, PNAS.

Wu, WY., Lo, MH., Wada, Y. et al. Divergent effects of climate change on future groundwater availability in key mid-latitude aquifers. Nat Commun 11, 3710 (2020). https://doi.org/10.1038/s41467-020-17581-yds

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Decision Letter:

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Dear Dr Zhang,

Your manuscript titled "Moisture from US Corn Belt Fuels More Intense Convective Storms" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Mengjie Wang
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability
Bluesky: @commsearth.nature.com; @commssustain.nature.com

Min-Hui Lo, PhD
Editorial Board Member
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have addressed all my previous concerns thoughtfully and thoroughly. Each of my comments has been resolved either through clear explanations in the rebuttal or through appropriate revisions in the manuscript. I find the current version significantly improved and scientifically sound. I would also like to acknowledge that the comments raised by the other reviewers were insightful and have been incorporated effectively. I appreciate the authors' diligent efforts and thank them for their careful attention to the feedback. I am satisfied with the changes made and support the manuscript's publication.

Reviewer #2 (Remarks to the Author):

The manuscript is well-revised, and I have no further concerns.

Reviewer #3 (Remarks to the Author):

All my comments were convingcingly addressed by the authors, whom I thank for their answers.
The paper is important and well written, and I think it can now be published in COMMSENV.

Reply to Reviewers' comments

Reviewer #1 (Remarks to the Author):

The manuscript by Zhang et al. adds to the understanding of managed land-atmosphere interactions and their impact on convective storms within the US Corn Belt. The methodological approach combining a high-resolution convection-permitting model with an advanced WVT and land surface scheme including MMF, dynamic crop and irrigation management represents a substantial advancement in the field of Earth system modeling. The findings provide compelling evidence for the direct influence of human agricultural activities on storm characteristics and intensity. However, several aspects warrant revisions, e.g., a more nuanced interpretation of causality within the regional modeling framework, and a precise discussion of the broader implications.

We thank the reviewer for this concise summary of our study, recognizing the novelty of our study and giving us suggestions for revision. We have expanded the discussion and interpretations related to the causality in the modeling framework and broader implications of our findings. Please see our point-by-point replies in the blue text below.

Specific comments:

• The manuscript quantifies the combined role of irrigation, crop, and shallow groundwater. It's worth noting that only a small fraction (~10–15%) of the Corn Belt is actually irrigated, with most areas relying on rainfall and shallow groundwater. Given that there are other experiments (GW-only, GW-Crop), it would be helpful to discuss what each component contributes. The paper mostly treats GW-Crop-Irr as a whole, but the title and implications often refer to 'agriculture' or more generally. Is it mainly crop and irrigation or does shallow groundwater alone play a major role? For instance, crop growth may just shift ET timing to July, improving moisture supply during peak MCS season. Groundwater might sustain soil moisture in early summer, while irrigation adds water in the late-season, especially in the western Corn Belt. A comparison/clarification of which component matters most would help generalize these findings beyond this region.

We thank the reviewer for the insightful comment regarding the individual contributions of the shallow groundwater, dynamic crop growth, and irrigation processes. We fully agree that this is a valid and important point.

As the reviewer noted, our primary goal was not to isolate the effect of each component, but rather to study the integrated impact of this complex land-atmosphere interactive system on intense convective storms. Our previous works had (22, 30-32) identified the individual roles of irrigation, crop, and groundwater on land-atmosphere interactions. In this work, our GW-Crop-Irr simulation represents a more holistic and realistic representation of land surface physics in the Corn Belt. The title "moisture from the Corn Belt" refers to the geographical region as a whole, which includes both irrigated and rainfed areas. We recognize that the contribution of each process may vary geographically. For example, irrigation's influence is more prominent in the western Corn Belt, where it is more common, while shallow groundwater effects may be stronger in the eastern Corn Belt. We have added a new sentence to the Discussion (L288-294; see also below for the added text) to reflect this important nuance.

We have also revised the manuscript's main text to more clearly state that our analysis focuses on the combined effect of these three processes. This work builds upon previous studies that have separately investigated shallow groundwater (Rasmussen et al., 2023), dynamic crops (Trevor et al., 2019), and irrigation (Yang et al., 2019; Qian et al., 2022) in WRF. Our primary contribution is the first coherent integration of these three processes and their combined impact. Our previous work (Zhang et al., 2025) laid the foundation for this integrated model, and this current study extends that work by focusing specifically on the integrated system's effects on convective storms.

We revised the main text to address your comment:
In the Introduction (L93-101):
"Here, we use a high-resolution, convection-permitting climate model coupled with an advanced Eulerian water vapor tracer (WVT) algorithm to quantify the integrated effects of shallow groundwater, dynamic crops, and irrigation on convective storms over the US Corn Belt (See Methods and Supplementary Fig. S1). Our modeling framework integrates detailed representation of shallow groundwater (30), dynamic crop growth (31), and irrigation practices (32) within the Noah-MP land surface model (33, 34), coupled with the Weather Research and Forecasting (WRF, 35) model. While we recognize that each process contributes uniquely to the system's overall moisture budget, our analysis focuses on their combined impact as a holistic land-atmosphere system."

In the Discussion (L288-294):
"Please note that while our analysis treats the groundwater, dynamic crop, and irrigation processes as an integrated system, we recognize that their individual contributions may vary by subregion and season. For instance, irrigation is largely confined to the 10-15% of the western Corn Belt, while shallow groundwater effects may be more pronounced in the eastern Corn Belt. Similarly, dynamic crop transpiration peaks with the crop's phenological cycle in late July to early August, whereas the effect of shallow groundwater on soil moisture is more persistent throughout the summer (22)."

• The use of a regional model means that its domain boundaries significantly influence the climate system within, potentially generating spurious moisture convergence near the edges. This could lead to an overestimation of convective activity and limit the ability to fully capture large-scale climate feedback. From Fig. 1f, it appears much of the increase in MCS frequency is right over Iowa/northern Illinois (the core Corn Belt) where Stage IV had a hotspot that the Baseline simulation missed, implying the GW-crop-Irr simulation successfully helped to simulate storms in this central hotspot. However, there are also some overestimations in the far north and southeast of the domain. The authors attribute the northern overestimation to a combination of missing Stage IV data and model boundary effects. The overestimation in the Southeast US is also visible. Is this again a boundary issue? A discussion on this in the paper would help, otherwise, it could give the impression of systematic over-estimation due to the model's inherent limitations.

We thank the reviewer for this insightful comment. While our GW-Crop-Irr simulation successfully addresses the underestimation of MCS frequency in the core Corn Belt, it also shows some overestimation near the northern and southeastern boundaries of the domain.

We agree that these overestimations are partially caused by model boundary effects. During the warm season, the dominant moisture flow over the Corn Belt is from the southern Great Plains to the northeast, driven by the Great Plains Low-Level Jet (GPLLJ). The additional moisture from our GW-Crop-Irr scheme enhances this convergence within the domain. This is precisely why we exclude the outermost 100 km (25 grid points) from our MCS analysis to mitigate these boundary effects. We added a description in the Method section Statistical analysis (L521-524):
"To mitigate the influence of model boundaries on the analysis convective systems, we define the outmost 100-km (25 grid points) of the simulation domain as a buffer zone, excluding the effect of moisture convergence near outflow boundaries from all MCS track calculations."

Furthermore, in Fig. 1e-f, the overestimation in the southeastern domain is actually less pronounced in our GW-Crop-Irr simulation compared to the Baseline. This indicates that our model's enhanced physics do not systematically lead to increased overestimation in these regions. The shift of MCS hotspots in the GW-Crop-Irr simulation (as shown in Supplementary Fig. S4) to the north and west aligns better with the observed Stage IV data, further demonstrating the model's overall improvement in simulating the observed spatial pattern.

We have also revised in the Discussion to address this important point about model boundaries, moisture convergence, and their influence on MCS frequency. This discussion will clarify that while some boundary overestimation occurs, the overall performance in replicating observed MCS hotspots is significantly improved.

In Discussion (L361-369):

"Nonetheless, we recognize several limitations in our current approach. One of them is the omission of tile drainage, an important and widespread practice in the eastern Corn Belt that is essential for managing excessive soil moisture and enabling spring planting. Not accounting for this process may lead to an underestimation of runoff and overestimation of soil moisture and its associated feedback, particularly during the early growing season (54). Furthermore, our model does not simulate the specific source of irrigation water, instead assuming that irrigation demand can always be met. Future research should seek to incorporate a physically-based pumping scheme to provide a more complete and realistic representation of the agricultural hydrological cycle and its feedback to the atmosphere. The use of a regional model also introduces inherent spatial limitations, as domain boundaries may influence the simulation, potentially leading to moisture convergence and overestimation of MCS frequency near outflow boundary (Fig. 1e-f and Supplementary Fig. S4). More importantly, the regional domain restricts the ability to fully capture large-scale climate feedback and the potential influence of Corn Belt moisture on downstream weather system and global atmospheric circulation (51). Future studies aiming to deepen our understanding of agricultural feedback to the atmosphere on a large spatial scale, using a global model with regional refinement to convection-permitting scale (55, 56), such as Model for Prediction Across Scales (MPAS, 57), will be of significant importance."

• While the study quantifies how Corn Belt agriculture intensifies local convective storms, it does so within a regional model domain. Recent work (e.g., McDermid et al., 2023, Nature Reviews Earth & Environment) has shown that irrigation can affect atmospheric circulation and precipitation far downstream, even across continents. This raises the possibility that agricultural impacts may extend well beyond the Corn Belt. It would be helpful if the authors briefly acknowledged this spatial limitation and noted that future studies using global convection-permitting models could explore such remote or tele-connected effects.

We appreciate this excellent suggestion. We agree that the use of a regional model presents a key limitation in representing potential remote or tele-connected effects of agricultural land use. The reviewer's reference to McDermid et al. (2023) is highly relevant, as it highlights the critical role of agricultural activities in modulating atmospheric circulation and precipitation far downstream.

We acknowledged this spatial limitation in the Discussion section (see also below) and cited the suggested work (McDermid et al., 2023) to contextualize our findings within the broader, global implications of agricultural impacts. We also noted that our study, while focused on regional effects, serves as an important step toward understanding these interactive processes. We further suggested that future studies, utilizing next-generation global convection-permitting models such as MPAS-A, would be well-suited to explore these important non-local effects.

In Discussion (L364-369):
“More importantly, the regional domain restricts the ability to fully capture large-scale climate feedback and the potential influence of Corn Belt moisture on downstream weather system and global atmospheric circulation (51). Future studies aiming to deepen our understanding of agricultural feedback to the atmosphere on a large spatial scale, using a global model with regional refinement to convection-permitting scale (55, 56), such as Model for Prediction Across Scales (MPAS, 57), will be of significant importance.”

• The study does not appear to consider tile drainage, which is widespread across the US Corn Belt and significantly alters soil moisture, ET, and surface fluxes. The omission of this practice represents a significant gap, as tile drainage fundamentally changes the soil moisture conditions that are central to the study's proposed feedback mechanism. Since the paper focuses on how land surface processes shape storm development, it would be helpful to acknowledge that subsurface drainage could counteract some of the irrigation effects or shift the seasonality of ET, particularly in wetter periods. Including or at least discussing tile drainage would improve the completeness of the land-atmosphere representation. Along with a recommendation for future studies, to consider incorporating tile drainage to provide a more complete and realistic representation of the agricultural landscape.

We thank the reviewer for this insightful comment. We agree that tile drainage is a key component of the agricultural landscape in the Corn Belt, especially in the wetter eastern regions, where it is crucial for managing soil moisture during the spring planting season. We acknowledge that the absence of tile drainage in our model could lead to an overestimation of soil moisture, especially at the beginning of the growing season.

We revised our manuscript's Discussion section to explicitly acknowledge the omission of tile drainage and its potential implications for our findings, particularly regarding the spring and early summer soil moisture (353-357):

“Nonetheless, we recognize several limitations in our current approach. One of them is the omission of tile drainage, an important and widespread practice in the eastern Corn Belt that is essential for managing excessive soil moisture and enabling spring planting. Not accounting for this process may lead to an underestimation of runoff and overestimation of soil moisture and its associated feedback, particularly during the early growing season (54)."

We also cited the relevant dataset by Valayamkunnath et al. (2020) to provide a clear path forward:
Valayamkunnath, P., Barlage, M., Chen, F. *et al.* Mapping of 30-meter resolution tile-drained croplands using a geospatial modeling approach. *Sci Data* 7, 257 (2020). https://doi-org.cuucar.idm.oclc.org/10.1038/s41597-020-00596-x

Minor comments:

• Line 20-21: an extra word 'influence'

Thanks for pointing this out. We have removed this extra word in the revised manuscript in L21.

• Line 149: missing a 'to'

Thanks for pointing this out. The missing "to" has been added in the revised manuscript.

• The simulation setting is in the supplementary, however, it could be clearer, e.g., Irrigation management means no irrigation?

We thank the reviewer for this question. Indeed, we have revised the caption for Supplementary Table 1 to clarify the simulation design. In Table S1, "No" means irrigation practice is not turned on in the model, while "Yes" means it is turned on. We changed these words to "On" and "Off" to avoid ambiguity.

• Review use of strong causal language, such as 'directly intensifying' and consider if a slightly more nuanced phrasing, like 'significantly amplifying' might be more appropriate. This refinement would acknowledge the complex interplay of various atmospheric and land-surface factors as well as the limitations.

We thank the reviewer for this insightful comment. We agree that our original phrasing, such as "directly intensifying," may be overly strong given the complex nature of land-atmosphere interactions and the inherent limitations of our model. We have gone through the manuscript and revised our language to use more nuanced terms, such as "significantly amplifying," "enhancing," or "contributing to." These changes better reflect the intricate interplay of various atmospheric and land-surface factors and acknowledge the limitations of our study.

• The statement that agriculture 'directly intensifies convective storms... potentially leading to cascading hazards (hail, tornadoes, flooding)' should be more cautiously worded. The study clearly supports increased storm duration and rainfall intensity, which strengthens the case for flood risk. But since wind shear doesn't change (as shown in Supplementary Fig. S23), and hail or tornadoes weren’t directly analyzed, those hazard links are speculative. It would be more appropriate to focus on the well-supported flooding implications and rephrase the conclusion to better match the study’s actual scope.

We thank the reviewer for this critical feedback on our language and its connection to specific hazards. We agree that our initial wording, which linked agricultural intensification to hazards like hail and tornadoes, was speculative and went beyond the scope of our analysis. Accordingly, we have removed these speculative claims. The revised text now focuses on the well-supported link between enhanced agricultural moisture and increased rainfall intensity and storm duration, thereby emphasizing the implication for flooding. This adjustment ensures that our conclusions are fully consistent with the evidence presented in the paper.

Overall, the conclusions are likely to influence thinking around land-atmosphere feedbacks and agricultural impacts on extremes, particularly within regional climate modeling and earth system modeling communities. The statistical analysis is robust and the methodology appears reproducible given the level of detail provided in the paper and supplement.

Thank you so much for this recognition. We are very grateful for your time and effort in reviewing our manuscript and providing very valuable suggestions.

Reviewer #2 (Remarks to the Author):

This paper provides a thorough explanation of the mechanisms by which irrigation promotes the development of MCS, addressing a gap in previous studies that primarily focused on the increase in MCS without detailing the formation mechanisms or moisture sources. The application of a comprehensive irrigation system to convective-permitting resolution is commendable. However, I have a few concerns:

We thank the reviewer for this constructive feedback and for recognizing the novelty of our study. Our primary goal was twofold: (1) to develop and improve land surface processes by integrating groundwater-crop-irrigation schemes, and (2) to investigate how these integrated processes collectively impact land-atmosphere interactions, particularly the frequency and characteristics of mesoscale convective systems (MCSs).

As the reviewer pointed out, accurately simulating MCS frequency is a significant challenge due to the inherent year-to-year variability and the complexity of these systems. Please see our point-by-point reply below.

1. The study clearly illustrate the irrigation promotes the MCS growth, but how well it can captures the MCS or whether it can "more realistically" capture the MCS. Fig. 1 shows it can better capture the lifetime and speed, and the supplementary files shows it can well capture the land processes (ET, Td, etc.), but I still have no idea about the general (convective) precipitation pattern. And even when seeing the improvement in frequency (Fig. 1e-f), the underestimation becomes overestimates with less significant dots, but I'm not sure about the overall bias. If I look at Fig. S4 for each year, I feel like the GW-Crop-Irr model has worse performance in 2011 normal year, and it doesn't have significant changes in 2010/2012. This let me raises doubt about the reliability of the improved system about whether it can really reflect the MCS mechanism.

We thank the reviewer for recognizing that the GW-Crop-Irr simulation improves the representation of land surface processes, including soil moisture, ET, and dewpoint temperatures. The first goal of our study is to enhance land surface representation by incorporating process-level mechanisms that have been missing in previous modeling studies.

We also acknowledge the GW-Crop-Irr simulation improves the MCS frequency in the hotspot regions (Iowa, Illinois, Indiana, Kentucky), while slightly overestimating near the northern and southeast boundaries. We want to clarify the dots in Fig. 1e-f represent areas where the model's performance is significantly different from Stage IV observations. Therefore, a reduction in these dotted areas in the GW-Crop-Irr simulation indicates performance closer to the observations and, thus, a better overall representation of MCS.

In fact, the overestimation of MCS frequency in the Southeast domain is less pronounced in the GW-Crop-Irr simulation compared to the Baseline, as shown in Fig. 1e-f. This suggests that the GW-Crop-Irr simulation is not systematically overestimating precipitation in the region.

This improvement can be explained by the prevailing wind pattern. The Great Plains Low-Level Jet (GPLLJ) transports moisture from the Gulf Coast to the Corn Belt, following a southwest-northeast pattern (See Supplementary Fig. S24). In the Baseline run, the underestimated surface moisture in the Corn Belt leads to fewer MCSs locally and a larger overestimation of precipitation in the downstream Southeast. In contrast, the enhanced moisture in the GW-Crop-Irr simulation facilitates more frequent MCSs within the Corn Belt, resulting in less moisture being transported downstream and, consequently, less overestimation in the Southeast. This is supported by the clear shift of MCS frequency hotspots further north and west into the core Corn Belt region, as shown in Supplementary Fig. S4, which matches better with the observed pattern.

We also included a new figure below that presents the total and MCS precipitation from StageIV observations and two simulations. The general patterns of both total and MCS precipitation are similar. The Baseline run substantially underestimates precipitation in the Corn Belt and overestimates it in the Southeast, whereas the GW-Crop-Irr run shows a significant improvement by reducing these biases.

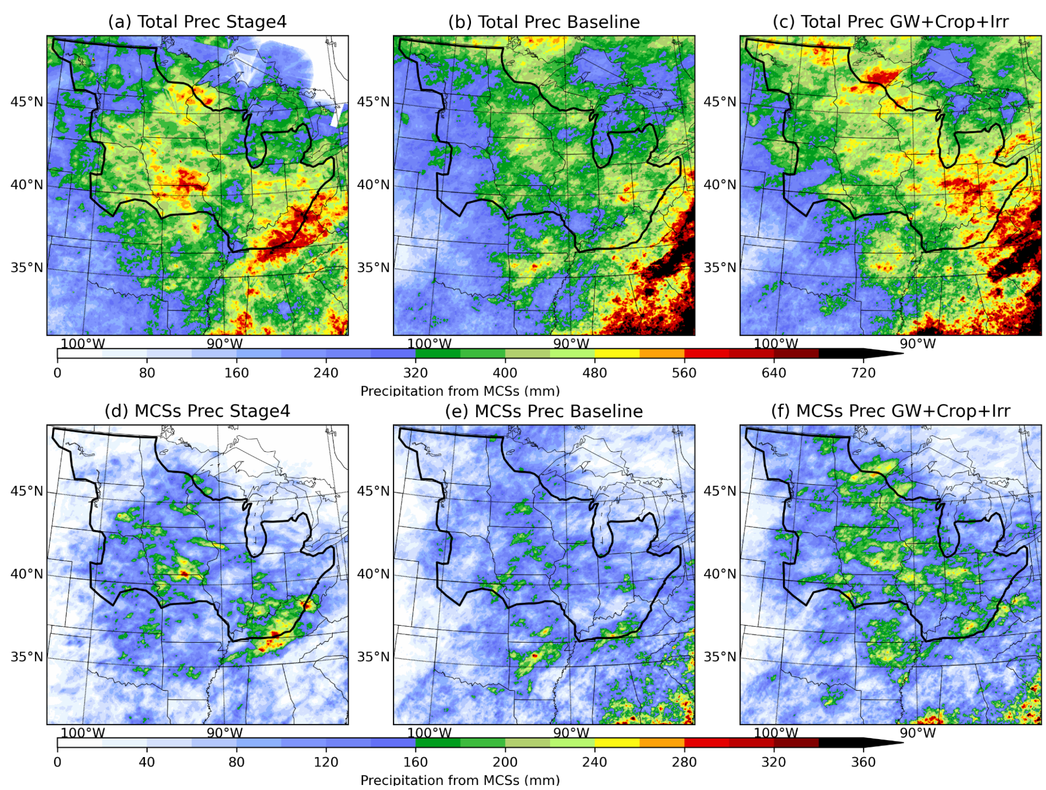

Fig. S5. Total (a-c) and MCS (d-f) precipitation (3-year average, from May to Aug) from StageIV observation, Baseline and GW-Crop-Irr simulation.

Finally, we acknowledge that model performance is influenced by multiple factors beyond land-surface processes, including the planetary boundary layer scheme, microphysics, and model boundary conditions. Our future work will address these issues by using a global-to-regional modeling framework, such as the Model for Prediction Across Scales (MPAS), which can mitigate boundary issues and allow for a more comprehensive analysis of large-scale interactions. We have added additional discussion regarding the limitations of our study in the Discussion section (L353-369):

"Nonetheless, we recognize several limitations in our current approach. One of them is the omission of tile drainage, an important and widespread practice in the eastern Corn Belt that is essential for managing excessive soil moisture and enabling spring planting. Not accounting for this process may lead to an underestimation of runoff and overestimation of soil moisture and its associated feedback, particularly during the early growing season (54). Furthermore, our model does not simulate the specific source of irrigation water, instead assuming that irrigation demand can always be met. Future research should seek to incorporate a physically-based pumping scheme to provide a more complete and realistic representation of the agricultural hydrological cycle and its feedback to the atmosphere. The use of a regional model also introduces inherent spatial limitations, as domain boundaries may influence the simulation, potentially leading to moisture convergence and overestimation of MCS frequency near outflow boundary (Fig. 1e-f and Supplementary Fig. S4). More importantly, the regional domain restricts the ability to fully capture large-scale climate feedback and the potential influence of Corn Belt moisture on downstream weather system and global atmospheric circulation (51). Future studies aiming to deepen our understanding of agricultural feedback to the atmosphere on a large spatial scale, using a global model with regional refinement to convection-permitting scale (55, 56), such as Model for Prediction Across Scales (MPAS, 57), will be of significant importance."

2. It seems like the improvement mainly improve the lifetime in wet years and speed in dry years? Any potential explanation for that?

We thank the reviewer for this insightful observation regarding the year-to-year variability in our results. However, with only three years of data, it's difficult to draw a statistically significant conclusion about how our model's performance on MCS statistics varies with different climate years. Our analysis focuses on the three-year average (shown in Fig. 1), which demonstrates that, on average, the GW-Crop-Irr simulation improves both MCS lifetime and speed compared to the Baseline. We acknowledge that more extensive, multi-decadal simulations would be needed to robustly determine how our model's improvements vary under different hydroclimatic conditions, which however requires substantial computational resources and is outside the scope of this study.

3. I'm not sure about the main intention of showing the case study in Figure 2. Key information here is irrigation contributes more along the MCS track but as you mentioned, this MCS is not very "well captured". So why is this specific case but not other cases chosen?

We appreciate the reviewer's question regarding the choice of the case study in Fig. 2. We included this specific case for two main reasons:

(1) It provides a direct and intuitive visual representation of the concepts we're analyzing. The figure shows the MCS storm track, the storm precipitation, and the water vapor transport (WVT) contribution to total precipitation from the Corn Belt. We believe this visual approach is more effective and straightforward for readers to understand the analysis we are doing compared to using text descriptions alone.

(2) This case was chosen because it represents a typical west-to-east MCS propagation and, despite minor tracking discrepancies, the model captures the precipitation amount and swath well. While our simulation is not intended for reproducing every individual storm, it's crucial to show that our model can plausibly represent the physical processes governing real-world storms. This case demonstrates that even though our study focuses on multi-year averaged statistics (as in many previous MCS studies), the underlying physics are sound. The discrepancy of one versus two in the tracking algorithm is a known issue, which we acknowledge as a limitation of the tracking method rather than a flaw in the model's physical simulation.

4. Is it possible to show the density map of MCS?
We thank the reviewer for this question. We have added the Supplementary Fig. S6 below for the MCS frequency density plot in the study domain (3-year average, MJJA). The MCS density map is essentially similar to the MCS occurrence frequency maps shown in Fig. 1d-f. The frequency density is the MCS frequency divided by the total time period (May-August, 3-year average).

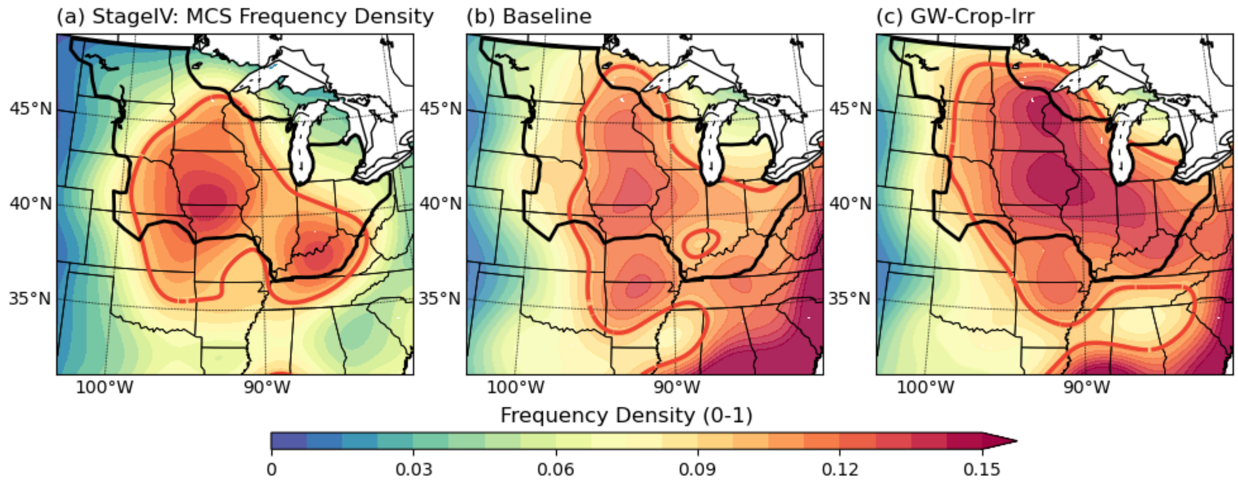

Fig. S6. Frequency density of MCS occurrence in the Corn Belt (3-year average, from May to Aug), from StageIV, Baseline, and GW-Crop-Irr. The red contours in a-c show the frequent MCS hotspot for occurrence > 12 cases in three datasets.

5. For experiment design, 4 experiments are introduced, while only two are used in this study, so I wonder the implications of mentioning the other two cases?

We thank the reviewer for this question. We included the four-experiment design in the methods section to provide context and continuity with our prior work (22, Zhang et al., 2025). While that study quantified the contribution of each individual process (Groundwater, Dynamic Crop, and Irrigation) to regional precipitation recycling, the focus of the present manuscript is fundamentally different. Here, we aim to understand the integrated impact of these three processes on convective storms. To avoid potential confusion, we have rephrased our experiment design part to introduce only the Baseline and the integrated GW-Crop-Irr simulations.

We chose to compare the GW-Crop-Irr simulation with the Baseline because the GW-Crop-Irr run represents the most realistic and comprehensive representation of the Corn Belt's land-atmosphere system. This approach allowed us to directly study the effects of the full, integrated system, which is the primary contribution of this work. We have also provided this rationale in the Introduction (L99-101):

“While we recognize that each process contributes uniquely to the system's overall moisture budget, our analysis focuses on their combined impact as a holistic land-atmosphere interactive system.”.

6. Since you are using Crop-Irr-Groundwater, I wonder if pumping is incorporated?

We thank the reviewer for raising this important question. We did not incorporate pumping in this study. Our model assumes that the water source can always meet the simulated irrigation demand. We acknowledge this limitation in the Discussion section (L355-358). "Furthermore, our model does not simulate the specific source of irrigation water, instead assuming that demand can always be met. Future research should aim to incorporate a physically-based pumping scheme to provide a more complete and realistic representation of the agricultural hydrological cycle and its feedback to the atmosphere"

7. In figure 5, (b) shows a stronger upper level divergence, but based on Figure S11, wind anomaly is only to one direction but not a divergence.

We thank the reviewer for carefully reviewing Fig. 5 and the original Supplementary Fig. S11 (now Fig. S13 in the revised manuscript). Fig. 5 is a schematic representation of the key physical mechanisms at play, with the arrows, lines, and text included for illustrative purposes. Our intention was not to imply a statistically significant increase in upper-level divergence as a result of the GW-Crop-Irr simulation, as confirming this would require a more detailed analysis beyond the scope of this paper. To avoidany misunderstanding, we also clarify in the figure caption that the arrows are for illustrative purposes. This adjustment ensures the schematic is consistent with the data presented in the supplementary materials and our overall conclusions.

8. A followup is, if the low-level wind moves further straightforward and high-level wind moves backward, like a shear force, is the convective cell strentched and lean more?

We appreciate this insightful follow-up question. Diagnosing wind speed and direction in storm environments is extremely difficult, because every storm is unique and it's hard to find a "representative storm" or to rotate and composite storms such that they align well like the classic leading-line/trailing-stratiform model shown in Fig. 5. That said, it is not robust to look at the wind profile of two storm environments from Baseline and GW-Crop-Irr and arrive at a conclusion that wind shear is profound and distorts the convective cells, causing them to become stretched and lean instead of vertical.

Based on our analysis, as presented in Supplementary Fig. S25, there is no statistically significant difference in either the low-level (0-3 km) or deep-layer (0-6 km) wind shear between the Baseline and GW-Crop-Irr simulations. Therefore, we conclude that the GW-Crop-Irr simulation primarily influences MCSs through thermodynamic effects (such as increased CAPE and reduced CIN), rather than through dynamic factors like wind shear. However, a more comprehensive analysis of the dynamic contributions of the additional moisture could be an interesting area for future studies.

9. I wonder x-axis numbers in Figure 5, it's used for showing the exact position of the convective cell and the structure scale?

We thank the reviewer for this question. Yes, Fig. 5 is a composite pattern based on quantitative model data (x-axis, y-axis, color contour and color scale), with the lines, arrows, and cloud-top illustration added schematically to depict the idealized structure of the MCS and its interaction with the underlying land surface. This design helps to convey the abstract concept of the integrated groundwater-soil-vegetation-atmosphere continuum in a more accessible way.

10. For the top 20% cases, is it the top 20% of each experiment? In that case, the selected experiments might be different between the experiments and each experiment may even contain a different number of selected cases? Then is it a still fair comparison?

We thank the reviewer for this question. Indeed, different experiments can have different MCS case numbers. Thus, the top 20% in GW-Crop-Irr can contain more case numbers than in the Baseline. While the absolute number of identified MCS cases vary between the simulations, the selection of the top 20% of each experiment ensures that we are comparing the strongest storms in each simulated scenario. However, in Supplementary Figs. S8 and S9, the difference between GW-Crop-Irr and Baseline is significant across all the distributions.

See also our main text (L213-215):
“For statistical robustness, the top 20% strongest MCSs were selected but the conclusion does not change when varying the sample sizes from top 10% to all MCSs (see Methods and Supplementary Fig. S8-12).”

11. The reference label to the supplementary figures should be rechecked. Many of them are wrong, such as Line 188 maybe referring to Figure S22(?), Line238 referring to Figure S11(?), and Line 457 referring to Figure S14(?). Please recheck the order and even reorder if possible.

Thank you so much for this suggestion. We have checked and modified the reference in the main text.

12. Please check the details of all the figures, including axis label for both x and y, units, etc. For instance, Figure 1c y-axis is halfly missing, Figure S2 "^-3" could be missing. And keep using one format between "NoGW" or "Baseline" or "NoGW Baseline" in the figures and the subcaptions.

We thank the reviewer for this reminder. We have carefully reviewed the labels and captions for every figure to make sure they are complete and consistent throughout the manuscript.

13. The meaning of some supplementary figures are not that clear. Like the Fig S22 , the dot I assume it should be vector differences?

We thank the reviewer for this question. The new Supplementary Fig. S24g-i (formerly Fig. S22) show the differences of IVT vectors between Baseline and GW-Crop-Irr and the dots suggest the differences are very small. We have included these notes throughout the supplementary figures as needed.

Reviewer #3 (Remarks to the Author):

Review of “Moisture from US Corn Belt Fuels More Intense Convective Storms” by Zhang et al.

This is a very interesting modelling paper about the role of soil moisture on atmospheric convection, which is a long-lasting research topic in the field of climate and weather science, as well reminded in the introduction. The study focuses on the US Corn Belt where soil moisture, evapotranspiration, and their evolution, are controlled by complex interactions between human agricultural activities (crop expansion and irrigation) and the natural supply from groundwater. More specifically, the paper compares two regional land-atmosphere simulations differing by the way the land surface is parametrized: either allowing complex interactions between soil moisture, crop development, groundwater, and irrigation; or using a baseline configuration in which crop LAI is prescribed and does not respond to soil moisture, itself independent from groundwater and irrigation.

The novelty is twofold: firstly, in the use of a convection-permitting atmospheric model, at a high enough resolution to permit the explicit description of convective processes, without simplified sub-grid parametrizations required at coarser scales; secondly, in the use of an Eulerian water vapor tracing algorithm, to quantify which part of the precipitation in the Corn Belt originates from local evapotranspiration. The resulting simulation are extremely CPU and storage intensive and were only run in May-June-July-August (growing season with frequent mesoscale convective events), for three consecutive years. This very advanced setup has already been used in a paper recently published in the PNAS by the same group of authors (Zhang et al., 2025), focusing on the quantification of moisture recycling within the Corn Belt, and on the relative contribution from groundwater, crop dynamics and irrigation.

The present paper offers a very important complement by focusing on mesoscale convective systems (MCSs), for which the convection-permitting feature is particularly relevant. These MCSs are important for both the weather and climate communities, as they are (i) major drivers of land-atmosphere coupling, (ii) causing major hazards, and (iii) usually enhanced by global warming. Consistently with the results of their previous study, the authors find that the additional moisture flux provided by the groundwater-crop-irrigation system results in more intense convective storms, making the simulation more realistic compared to precipitation observations from the StageIV database. This result is well expressed by the paper’s title, and well supported by the proposed figures, which are clearly explained by the text. This makes the paper worth publishing in Communications Earth & Environment, but I would suggest one addition (optional), a strengthening of the discussion, and a few minor revisions, as detailed below.

My main recommendation is to develop what relates to the length of the MCS tracks. Figure 2 provides a very powerful analysis of this feature for two days in June 2012, with a comparison to the StageIV data. Both simulations exhibit one event, which is longer and more intense with the GW-Crop-IRR than the Baseline setup. What I find very interesting is that this makes the GW-Crop-IR simulation closer to the StageIV observation, if one lumps its two tracks into one. Here, I first wonder if Fig. 2a-c could not be moved into Figure 1. Then, I am curious about the MCS track length, regardless of the number of events: are they statistically longer in GW-Crop-IRR? Hence closer to the mean track length that can be extracted from StageIV? If this analysis makes sense for an atmosphere physicist, which I am not, it would probably strengthen the conclusion that the additional moisture supply makes the simulated MCS closer to observation.

We thank the reviewer for the insightful comments and for recognizing the value of our analysis on MCS track length. We agree that a statistical analysis of track length would provide a powerful, additional metric to support our main conclusions. Following this recommendation, we conducted an additional analysis of MCS track length for the Stage IV observations, the Baseline simulation, and the GW-Crop-Irr simulation. We calculated the track length for each storm by summing the hourly distance between the mass centers of the storm's precipitation field (see figure below):

Stage IV shows a mean track length of 513 km. In comparison, the Baseline simulation underestimates this value, with a mean track length of 445 km, more than 10% difference (p=0.0002). The GW-Crop-Irr simulation yields a mean track length of 490 km, representing a substantial improvement over the Baseline simulation. Moreover, this value is not statistically different from the Stage IV observations (p=0.2). This new analysis demonstrates that the GW-Crop-Irr simulation produces MCSs with more realistic track lengths, which strongly supports our central conclusion that the additional moisture supply from our integrated land-surface processes leads to more realistic storm characteristics.

This result is physically consistent with our findings on storm lifetime and storm speed. As shown in Fig. 1a–b, a longer storm lifetime combined with a faster storm speed leads to a longer total travel distance. We have included this new figure in the Supplementary Materials to provide additional evidence for our conclusions.

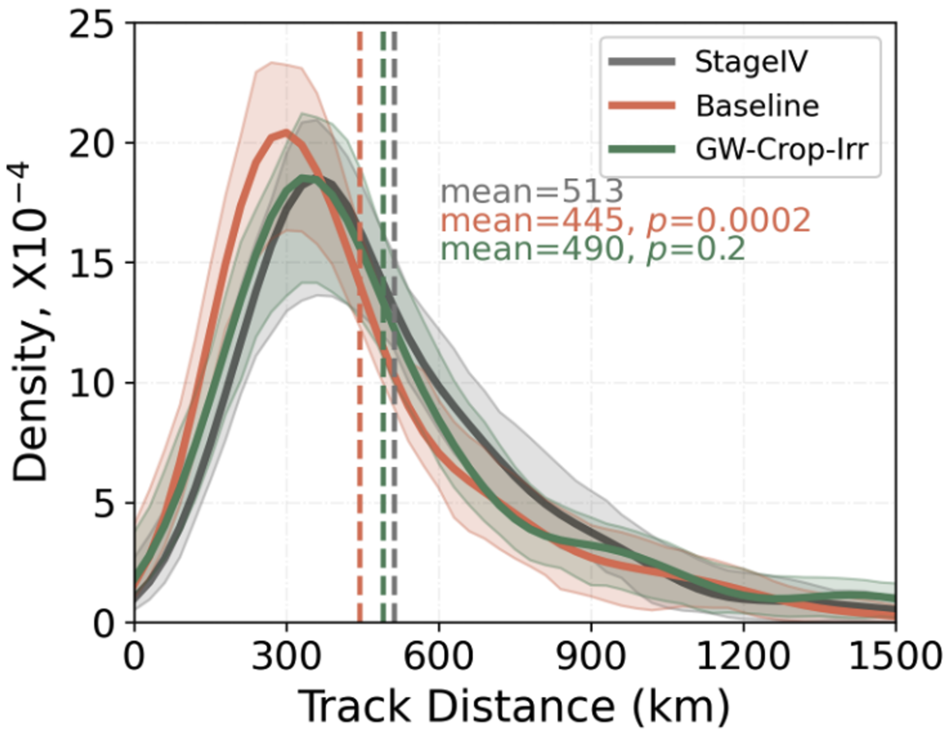

Fig. S26. GW-Crop-Irrigation interactions increase storm track distance, consistent with the increase in storm lifetime. Since storm track distance is the cumulative distance traveled each hour over a storm's lifetime, longer-lasting storms naturally result in longer tracks. The storm track in the Baseline is significantly underestimated compared to the StageIV precipitation, while GW-Crop-Irr shows a marked improvement, significantly increasing track distance relative to the Baseline.

Other suggestions:

L133-136, "overestimation of MCS frequency": three good reasons are given here, including uncertainties in the MCS tracking algorithm. Can the authors expand a bit? Do these uncertainties include interactions between the window used to track the MCS and the boundaries of the simulated domain? Would it be possible to define a buffer zone along these boundaries in which MCS are uncertain? If so, how do the MCS statistics compare if calculated inside the full domain, or in the domain minus the buffer zone? This overestimation should be reminded in the Discussion, like the fact that all model results are model-dependent and need confirmation by other models and observation-based evidence.

We thank the reviewer for this insightful question. The reviewer is correct that the interaction between the MCS tracking algorithm and the simulation domain boundaries is a key source of uncertainty.

To address this, all MCS statistics presented in our study were calculated exclusively within a domain that excludes a 100-km (25 grid cells) buffer zone along each boundary. We have added a sentence in the Methods section to explicitly state this (L520-524):

"We select the identified MCS cases within the Corn Belt mask to analyze MCS characteristics for StageIV precipitation data and two simulations, as shown in Fig. 1a-c. To mitigate the influence of model boundaries on the analysis convective systems, we define the outmost 100-km (25 grid points) of the simulation domain as a buffer zone, excluding the effect of moisture convergence near outflow boundaries from all MCS track calculations."

As such, our analysis already focuses on the most robust and reliable portion of the simulated domain. Ideally, we would like to use a larger domain in WRF or use MPAS-A global model with regional refinement to eliminate the model boundary effect in future studies.

We also expand in the Discussion section about the overestimation of MCS frequency near the domain boundaries (L361-369):

"The use of a regional model also introduces inherent spatial limitations, as domain boundaries may influence the simulation, potentially leading to moisture convergence and overestimation of MCS frequency near outflow boundary (Fig. 1e-f and Supplementary Fig. S4). More importantly, the regional domain restricts the ability to fully capture large-scale climate feedback and the potential influence of Corn Belt moisture on downstream weather system and global atmospheric circulation (51). Future studies aiming to deepen our understanding of agricultural feedback to the atmosphere on a large spatial scale, using a global model with regional refinement to convection-permitting scale (55, 56), such as Model for Prediction Across Scales (MPAS, 57), will be of significant importance."

L139-141, "Additional process-based model validation": this part is interesting but is uniquely supported by figures from the supplementary, which would deserve to be referenced by their number in the main text. I suggest replacing "surface humidity" by "near-surface atmospheric humidity" to prevent from confusion with soil humidity. The caption of Fig. S16 and S17 should read "same as Fig. S15" instead of S14. Regarding groundwater (Fig. S14), the caption does not explain which simulation is depicted in panels b-d. And since there is not comparison between the match of the two simulations with the GGMN simulation, this figure might be omitted. Finally, I am not sure « process-oriented » is relevant here, at least without further elaboration in the text.

We thank the reviewer for this suggestion and have implemented all the recommended changes.

We have changed the word “surface humidity” to “dewpoint temperature” to directly refer to the key land surface variables we are evaluating. We also modified the figure number in the main text as well as in the supplementary materials. We have removed the phrase "process-oriented" from the main text to keep the language concise and to avoid confusion.

L172-174: since the Corn Belt moisture contribution to the local MCS is stronger in GW-Crop-IRR than in the Baseline simulation, can we conclude (and write here) that GW-Crop-IRR is more realistic? Do we have an observation-based estimation of the Corn Belt contribution to extreme precipitation?

We thank the reviewer for this excellent question. Unfortunately, directly quantifying the contribution of a specific regional moisture source to extreme precipitation is extremely challenging to validate with observations. Most observational methods, such as water isotope analysis from stations or remote sensing, are better suited for quantifying broad climatological patterns rather than the source of moisture for individual extreme events.

However, we can infer the realism of our GW-Crop-Irr simulation based on its overall performance. As demonstrated throughout the manuscript, our GW-Crop-Irr simulation substantially improves the representation of key land-surface variables (e.g., evapotranspiration, soil moisture, and dewpoint temperature). This, in turn, leads to a significant improvement in simulating observed precipitation patterns and MCS frequency within the Corn Belt. Therefore, while a direct observational estimation of the Corn Belt's contribution is not feasible, the enhanced physical realism of our model and its improved simulation of observed storm characteristics support our conclusion that the moisture contribution quantified by our water vapor transport (WVT) analysis is more reliably represented in the GW–Crop–Irr simulation.

L191-193: Similarly, does the paper provide observation-based estimates of “moisture contribution to MCS precipitation” and “MCS precipitation proportion in total precipitation”? If not, the end of the sentence about improved performances should be rephrased to avoid overstatement.

We thank the reviewer for this question. While we cannot provide direct observation-based estimates of moisture contribution to MCS precipitation, we have revised the text to remove the claim of “improved performance” at the end of the sentence.

L212: the increase of rainfall intensity near the MCS center is not particularly sharper in GW-Crop-IRR than in the Baseline, so the sentence should be rephrased. It might even underline that the intensity differences are quite small, in contrast to the Corn Belt contribution to MCS precipitation.

We thank the reviewer for this suggestion. We have revised the description in the manuscript (L217-218):

"…the rainfall intensity near the maximum precipitation center (P_max) is higher in the GW-Crop-Irr simulation (Fig. 3c), with an increase of 2.7 mm/h (a 6% rise) over the Baseline simulation."

Discussion:

L283, end of first paragraph summarizing the main results: it would be important to add that the intensification of MCS makes GW-Crop-IRR more realistic with respect to StageIV precipitation.

Thank you so much for this helpful suggestion. We have added this comment to the discussion. Please see the main text in L286:

"This leads to increased frequency, lifetime, intensity, and propagation speed of MCSs, making the GW-Crop-Irr simulation more realistic compared to the StageIV precipitation data."

L300-305: I wonder if 1) and 2) are really two different impacts, and if they are, my feeling is that 2) causes 1) so should be mentioned first.

We thank the reviewer for the thoughtful suggestion. We agree that the two points are closely related. However, we have structured the discussion in this order to first address the thermodynamic changes in the storm environment (CAPE, CIN, LCL), which are key drivers of convective instability, and then to explain the moisture source of that instability—the enhanced moisture from the Corn Belt's integrated agricultural system. These two major impacts are also corresponding to the order of Fig. 4a-d. Therefore, we would like to keep the current order to best serve the logical flow of the discussion and help readers understand the physical mechanism.

L306, "cold pool dynamics": not being familiar with this concept (also mentioned in L254), I would appreciate a short explanation, using Fig. 5 if relevant.

We thank the reviewer for this question. "Cold pool" is a key component of an MCS, that is a region of cool, dense air that forms beneath the storm due to the evaporation of falling rain. As rain falls from the storm's updraft, it evaporates into the drier air below the cloud. This process cools the air, making it denser than the surrounding environment. This cold, dense air sinks to the surface as a downdraft, spreads out horizontally, and forms a dome-like "cold pool". This cold, dense downdraft and warm, moist updraft at the front together create a balanced, self-sustaining circulation, where new convection is continuously triggered along the leading edge, sustaining long-lived storms and driving their forward propagation.

A fundamental study on cold pool dynamics and their interaction with environmental wind shear and lifting mechanisms is RKW (1988), which theorized storm propagation speed as a function of the temperature gradient between the cold pool and the surrounding warm air.

Using Fig. 4h and Fig.5 as an example, the GW-Crop-Irr simulation shows a stronger (negative) temperature gradient between the storm center and 100km ahead in the inflow region, suggesting a more intense cold pool. This larger temperature gradient is consistent with Fig.1, which shows that GW-Crop-Irr produces faster propagation speeds and longer lifetimes. Please also see our discussion in L315-320 and theoretical calculation in Supplementary Text, supporting our modeling results.

Reference:
Rotunno, R., Klemp, J. B. & Weisman, M. L. A Theory for Strong, Long-Lived Squall Lines. *J. Atmos. Sci.* **45**, 463–485 (1988).

L311-313: I wonder if it would not be clearer to insist on the fact that a low roughness length is an important driver of tornado activity.

We thank the reviewer for this suggestion. Here, we compare our results with those of Li et al. (2024), who investigated a different aspect of surface roughness length. Their study compared two environments, the South Great Plains grassland (low roughness length) and the Amazon rainforest (high roughness length) and found low roughness length was associated with stronger wind shear and more frequent tornadoes in the SGP. In contrast, our study focused primarily on the thermodynamic effects of enhanced moisture source from the Corn Belt, and the dynamic crop model doesn’t sufficiently reflect the change of crop height and instead uses a constant roughness length. We provided further analysis in Supplementary Fig. S25, showing the low- and deep-level wind shear from Baseline and GW-Crop-Irr are not sufficiently different.

L316: please specify if you speak of differences in crop height or roughness length.

Thank you for this question. In this sentence, we are speaking of the variation in crop height. For the US Corn Belt, the crop height is prescribed to be 0.1-2m, and for Amazon rainforest, the tree canopy height is prescribed to be 8-20m, an order of magnitude larger than in the Corn Belt.

L317: a difference of 1 to 10 m between grassland and tropical forests seems too small if we speak of crop height and may be too high for roughness length.

Thanks for this comment. Following on from the last response, vegetation canopy height for cropland should be 0.1-2m and for tropical rainforest between 8-20m. We made this correction in the main text (L326-328):
"However, the variation of crop height is minimal (0.1-2 m) compared to the differences between grassland in South Great Plains and tropical rainforest in Amazon (8-20 m)"

L319-321: this sentence is not grammatically correct and not particularly clear. I suggest the following rephrasing: "This suggests the land surface contribution mostly concerns the local boundary conditions."

Thank you so much for this suggestion. We will rephrase this sentence in the main text (L330-333):
"This suggests the land surface contribution mostly concerns thermodynamic environment at local to regional scale, while the large-scale wind shears are dominated by model lateral boundary conditions."

Figure 5: It would be useful to give indications about the orientation of the diagrams (West and East). If the color shading is based on simulated MCS, please specify which ones (20% composite?).

We thank the reviewer for this question. MCSs in the central US generally propagate from west to east. However, not all storms move in the same direction, and the actual movement is a complex interplay, see Supplementary Fig. S2 for the storm tracks.

Therefore, to construct Fig. 4 and Fig. 5, we align the storm propagation with the strongest gradient of equivalent potential temperature (θ_e). Therefore, in these figures, the storms are shown as propagating from left to right and consistently showing the strongest updraft inflow region in the front. This ensures that the schematic and composite analysis accurately reflect the internal structure of the storms.

Thank you also for this reminder, we used the top 20% storm composite for color shading of the moisture flux (green upwards and red downwards) in Fig. 5. We also added this information in the Method section and in Fig. 3-5.

L338-341: It would be useful to add here which driver, among groundwater, dynamic crops, and irrigation, is the most impactful based on ref 22. This could lead to discuss how realistically these three drivers are parametrized in the study, and if other important moisture sources may be missing, such as the Great Laurentian Lakes.

We thank the reviewer for this suggestion. Based on our previous study (Zhang et al., 2025a), we have revised the discussion to clarify the distinct contributions of groundwater, dynamic crops, and irrigation. Our updated text now reads (L286-292):
"Please note that while our analysis treats the groundwater, dynamic crop, and irrigation processes as a whole, we recognize that their individual contributions may vary by subregion and season. For instance, irrigation is largely confined to the 10-15% of the western Corn Belt, while shallow groundwater effects may be more pronounced in the eastern Corn Belt. Similarly, dynamic crop transpiration peaks with the crop's phenological cycle in late July to early August, whereas the effect of shallow groundwater on soil moisture is more persistent throughout the summer."

We also acknowledge the importance of other moisture sources, such as the Great Laurentian Lakes. However, both the Baseline and GW-Crop-Irr simulations use the same open-water parameterization for lakes, which means the treatment of this moisture source is identical in both runs. This allows us to isolate the impacts of our land surface processes without interference from other large-scale moisture sources.

L338-342: One sentence to discuss the limitations of such a complex modelling exercise is not enough. The effect of the lateral boundary conditions would be usefully discussed with a reference to Fig S1. In particular, I wonder if it is well suited to study the Corn Belt in a domain where the Corn Belt is not at the center. There are many more limitations, regarding both the land and atmosphere components, and they should be reminded as they generate uncertainties, and call for confirmation of the present results using other models.

We thank the reviewer for this great suggestion. Indeed, there are several limitations in this study regarding a complex modeling system, many of which are also mentioned in this reply, including regional model boundary that potentially leads to moisture convergence and overestimation over model outflow boundary, missing tile drainage process before the planting dates, and not limiting irrigation supply to meet the demands, and limited regional model study with three-year growing seasons. As we are now transitioning from regional convection-permitting study in WRF to running global models with high-resolution regional refinement in MPAS, we hope to address some of these regional model boundary issues in the future.

Please see the corresponding additional discussions in the revised manuscript (L353-369):

“Nonetheless, we recognize several limitations in our current approach. One of them is the omission of tile drainage, an important and widespread practice in the eastern Corn Belt that is essential for managing excessive soil moisture and enabling spring planting. Not accounting for this process may lead to an underestimation of runoff and overestimation of soil moisture and its associated feedback, particularly during the early growing season (54). Furthermore, our model does not simulate the specific source of irrigation water, instead assuming that irrigation demand can always be met. Future research should seek to incorporate a physically-based pumping scheme to provide a more complete and realistic representation of the agricultural hydrological cycle and its feedback to the atmosphere. The use of a regional model also introduces inherent spatial limitations, as domain boundaries may influence the simulation, potentially leading to moisture convergence and overestimation of MCS frequency near outflow boundary (Fig. 1e-f and Supplementary Fig. S4). More importantly, the regional domain restricts the ability to fully capture large-scale climate feedback and the potential influence of Corn Belt moisture on downstream weather system and global atmospheric circulation (51). Future studies aiming to deepen our understanding of agricultural feedback to the atmosphere on a large spatial scale, using a global model with regional refinement to convection-permitting scale (55, 56), such as Model for Prediction Across Scales (MPAS, 57), will be of significant importance.”

The last paragraph of the discussion (L346-366) refers to some studies that concur with the present one. It would be useful to specify if they are all targeted on the US Corn Belt. I also recommend to include other important references, which rely on field campaigns designed to study the atmospheric impact of irrigation: the ones of the GRAINEX field campaign on the influence of irrigation on convective precipitation in Nebraska (at least 5 papers since 2022); the paper of Lunel et al. (2024), which compares simulations with a convection-permitting model to observations from the LIAISE experiment in Spain.

Thank you so much for this suggestion about adding references from the GRAINEX and LIAISE field campaign. Indeed, these field campaigns add observational evidence to our modeling study. Please see L373-377:

“In fact, previous observational and modeling studies have investigated the role of land use change (29), soil moisture (58), and irrigation signals (59). In particular, the GRAINEX field campaign in Nebraska (60, 61), LIAISE field campaign in Northeast Spain (62), and satellite-detect rainfall (20), underscore the significant role of land surface processes and locally source moisture (63) in influencing boundary layer properties and in precipitation development.”

L355: The reference to the land-atmosphere coupling hotspots of Koster et al. (2004) is interesting to hint at some generalization, and could be developed a bit. In particular, one important common point of these hotspots is to be transitional areas with respect to soil moisture. Yet, soil moisture is higher in GW-Crop-IRR than in the Baseline: does the Corn Belt remain a transition area in GW-Crop-IRR or does it become too wet for this? Does the higher convection in irrigated areas relate to stronger land-atmosphere coupling (in the sense of higher correlation between anomalies of soil moisture and anomalies of precipitation, as used by Koster et al., 2004), or in contrast to weaker land-atmosphere coupling, which does not prevent from a larger quantitative effect on precipitation?

We thank the reviewer for this insightful question. We cited (Koster et al. 2004) for reference to the “global hotspots for land-atmosphere interactions”, where irrigation could have large impacts to precipitation, and these regions are consistent with the results reported by other studies around the world.

In response to the reviewer’s question, we speculate that GW-Crop-Irr processes won’t change the soil moisture regime in irrigated regions from transitional to wet. Usually, irrigation is triggered when soil moisture is below climatological soil moisture limit, fulfilling a management allowable threshold as defined in the Method section, to encounter an adversary environment in arid and semi-arid regions. This amount of irrigation can help relieve dry conditions but may not be sufficient to move to a wet regime. While we didn’t look into the land-atmosphere coupling strength and regime analysis in this study, we agree it would be an interesting analysis for future study.

L355-359: Here starts the opening of the Discussion, which could deserve a specific paragraph with a line break. The first two sentences are about future crop expansion and irrigation, but this remains very general. Yes, increased irrigation can be supplied by deep fossil aquifers, but they are massively depleted and their volume or pressure may become limiting (e.g. Scanlon et al., 2012; Hilton and Jasechko, 2023). Climate change will also affect groundwater availability and influence future irrigation prospects (Wu et al., 2020; Arboleda-Obando et al., 2025). These elements could be worth mentioning, as they introduce strong uncertainties on future storms in the Corn Belt.

We appreciate this suggestion from the reviewer. We separated this long paragraph and added more discussion on future irrigation expansion, potentially limited by groundwater availability and climate change, as suggested by the reviewer. This information strongly enriches the depth and broadness of this study (L380-383).

“Last but not the least, it is anticipated that global food demand will rise in the future, in conjunction with climate change (67-69) and evolving dietary patterns. To satisfy this growing demand, a significant portion of irrigation would be sourced from deep “fossil” aquifers (70), but they have been massively depleted, and their volume may become a limiting factor (71, 72), exerting pressure intensifying the global groundwater crisis (73).”

Typos and technical comments:

Fig. 1: the label of the y-axis of panel c is truncated ; I couldn’t find where “The number of MCS is printed in parentheses” ; the caption should state that the thick black contour delineates the US Corn Belt ; L153 : “shows” instead of “showing”.

Thank you so much for this suggestion. We re-created Fig. 1 with case numbers printed in the parentheses and modified the caption.

L162: “shows” instead of “illustrates”
Thanks for this suggestion. We changed this word in the main text.

L164: “indicates” instead of “indicate”
Thanks for this suggestion, we modified it in the main text.

174: “presents” instead of “present”
Thanks for this suggestion, we modified it in the main text.

L213-218: the percent changes written here are not the same as the values printed in Fig. 3, can you explain why you use ranges in the text and how they were obtained?
We thank the reviewer for this question. Fig. 3c,f are composed by sampling the rainfall intensity (mm/h) with the distance to maximum precipitation center as the radius across all cases. Thus, the range represents the variability across different cases and spatial variability. We have modified the main text to be consistent with the figure (L217):

“… the rainfall intensity near the maximum precipitation center (P_{max}) is higher in the GW-Crop-Irr simulation (Fig. 3c), with an increase of 2.7 mm/h (a 6% rise) over the Baseline simulation”

The in L213 should probably be a -.
Thanks for this suggestion, we have changed the symbol “~” to “-” in the main text.

Fig. 2d-f, Fig. 3, Fig. 4: I suggest reminding in the caption what MCS are considered to draw these figures? I understood they are based on the 20% most extreme MCS, at least for the composites of Fig. 3, but I would like to be sure. And I would also like to know if Fig. 4 is based on the composite MCS?

Yes, that's right. We added in the Method section as well as in the text that Figs.3-5 MCS composite are derived from the top 20% strongest MCS cases (L538):
"For the analysis of spatial composite of convective storms in Figs. 3-5, we select the top 20% of the strongest MCS cases (~60 cases) by their precipitation maximum …"

Fig. 4, L266-267: I couldn't find how the surface temperature gradient is calculated, can it be added, either in the caption or in the Methods?

Thank you so much for this question. The surface temperature gradient for each storm is calculated by the integrated temperature difference (from surface to 3-km height) between 100-km ahead of the storm center and 100-km behind the storm center (25 grid cells ahead and behind). In this way, we can quantify the temperature difference along the storm track environment, in particular the warm-moist air ahead of the storm and cold-dry air behind the storm, thus the unit is in (K/100-km). We have added this clarification to the Methods section (L570-574).

Fig. 4: if space allows, I would be interested by the panels showing the differences c-a, and d-b.
Thank you so much for your interest and suggestion. However, Fig. 4 is already very busy, with 8 subplots. We have added the differences (c-a and d-b) in Supplementary Fig. 13 and provided a reference in the main text.

L289: "What is" instead of "What's"
Thanks for this suggestion, we modified it in the main text.

Methods:

The figures must be given a consistent notation: either Supplementary Fig. X, or Fig. SX.
Thank you so much for this suggestion, we have used a consistent notation for all figures (Fig. X in the main text and Supplementary Fig. SX in Supplementary Information).

Simulated domain and boundary conditions: do the Great Lakes provide some moisture?
We thank the reviewer for this question - Yes, both simulations (Baseline and GW-Crop-Irr) use the same treatment for lake evaporation (treated as open water) and they should be the same between the Baseline and GW-Crop-Irr. However, Great Lakes are not part of the Corn Belt land mask, therefore, the moisture contribution from the Great Lakes are not included in the WVT contribution in Fig. 2-5.

Irrigation: where is water taken from to irrigate? Can water be limited for irrigation?

Thank you for this question. At this stage we don't specify the source of irrigation, whether it's from surface water or groundwater, assuming the irrigation water source can always meet the demand, which is not a very realistic assumption. However, at least in the US Corn Belt, pumping is not a very significant factor, as compared to in the California central valley. Our team is actively working on implementing the pumping scheme in the land model and we are looking forward to this implementation and testing its impacts in future studies.

We have added in the Discussion on this limitation (L357-361).
"Furthermore, our model does not simulate the specific source of irrigation water, instead assuming that irrigation demand can always be met. Future research should seek to incorporate a physically-based pumping scheme to provide a more complete and realistic representation of the agricultural hydrological cycle and its feedback to the atmosphere."

L467-468: what does « wet soil moisture » refer to? The soil moisture of the water year 2010? If so, please rephrase.
We thank the reviewer for this question. "Wet soil moisture" in this context refers to soil moisture wetter than 0.25 m3/m3. In Supplementary Fig. S17-19, the biggest difference between Baseline and GW-Crop-Irr and the biggest improvement from GW-Crop-Irr related to ISMN occurs after soil moisture > 0.25 m3/m3. We have rephrased this word to "soil moisture > 0.25 m3/m3" in the Method section.

Cited references:

Arboleda-Obando, P. F., Ducharne, A., Cheruy, F., and Ghattas, J.: Joint evolution of irrigation, the water cycle and water resources under a strong climate change scenario from 1950 to 2100 in the IPSL-CM6, Earth Syst. Dynam. Discuss. [preprint], https://doi.org/10.5194/esd-2024-41, in review, 2025.

Hilton, A., and Jasechko, S. (2023). Widespread aquifer depressurization after a century of intensive groundwater use in USA, Science.

Lunel, T., Boone, A.A. & Le Moigne, P. (2024). Irrigation strongly influences near-surface conditions and induces breeze circulation: Observational and model-based evidence. Quarterly Journal of the Royal Meteorological Society, 150(762), 2798–2819. Available from: https://doi.org/10.1002/qj.4736

Scanlon, B. R., et al. (2012). Groundwater depletion and sustainability of irrigation in the US High Plains and Central Valley, PNAS.

Wu, WY., Lo, MH., Wada, Y. et al. Divergent effects of climate change on future groundwater availability in key mid-latitude aquifers. Nat Commun 11, 3710 (2020). https://doi.org/10.1038/s41467-020-17581-yds

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have addressed all my previous concerns thoughtfully and thoroughly. Each of my comments has been resolved either through clear explanations in the rebuttal or through appropriate revisions in the manuscript. I find the current version significantly improved and scientifically sound. I would also like to acknowledge that the comments raised by the other reviewers were insightful and have been incorporated effectively. I appreciate the authors' diligent efforts and thank them for their careful attention to the feedback. I am satisfied with the changes made and support the manuscript's publication.

Reviewer #2 (Remarks to the Author):

The manuscript is well-revised, and I have no further concerns.

Reviewer #3 (Remarks to the Author):

All my comments were convincingly addressed by the authors, whom I thank for their answers. The paper is important and well written, and I think it can now be published in COMMSENV.

Reply to reviewers:

We sincerely thank all the three reviewers for their time and effort on reviewing our manuscript and providing valuable and insightful questions and comments.