Referee 1: Corey Lesk

Author response

This paper examines the dependence of county level historical soybean yields on a suite of climate variables, with a particular eye to the influence of compound extremes. The paper is well motivated by recent climate and crop science literature, and nicely illustrates the particular relevance of hot and dry extremes for soybean yields. It further outlines historical trends in such extremes in relation to their univariate temperature and soil moisture components (using proxies to extend the time series). The manuscript is well written and the discussion raises a lot of interesting points. Overall, I think the authors did a great job and the paper should be considered for publication with revisions.

To me, the weaknesses of the paper are a 1) that there are a few methodological concerns and 2) that the paper doesn't extend that much beyond what is already fairly well established, even though it could using its data and methods. I elaborate below on these critiques and suggest some ways the authors could make the paper more compelling in revision.

RESPONSE: We thank the reviewer for the positive feedback on our manuscript. We are grateful for the critiques and suggestions on how our manuscript can be improved. We respond to the comments given in the text below (in bold and italics text).

General comments

Methodological concerns:

The statistical modeling framework is quite detailed and meticulous and there is much attention paid to many sources of confusion, error, or interpretability issues. I commend these solid methods. However, I think the contradiction in scale between the model calibration (pooled national data) and its application (county-level) data greatly limits the soundness of the otherwise meticulous method. This to me is one of the main things to address in revisions.

The use of pooled national data to calibrate the model and then the subsequent use of that model to assess yield variability at county scales seems a bit inconsistent. All county-year pairs of yield values were combined into a larger dataset as in the Troy et al. (2015) paper referenced, but then the resulting model was applied for individual counties, whereas Troy et al. ran all their analysis at national scale. It doesn't seem common or intuitive to me to calibrate and run models at such different scales, and this isn't justified, discussed, or acknowledged in any detail. What the authors did here is an expedient way to make their analysis applicable at a county level, but leverage a wider dataset to increase degrees of freedom and enable the testing of more complex models. But the cost is that the mismatch in scales raises questions on whether some results are a result of the mismatch, or truly robust results.

A few ideas on how this might be influencing results: First, it could be that the particular relevance of concurrent low August soil moisture and high Tmax in Illinois (nicely illustrated in Fig. 5) is a result more of the suitability of the nationally fitted model at that location, rather than the relevance of compound extremes more generally. This concurrent temperature-moisture result does not strongly agree with the results of Mourtzinis et al. (2015) which found larger temperature impacts in southern states, nor Zipper at al. (2016) which found stronger drought impacts on soy in southern states. It could very well be that the compound impact is larger in Illinois, but the ambiguity induced by the contradiction in scales in the modeling makes the result not as robust as it could be.

Second, there is strong variation in the significance and model r2 across counties (Fig. 3). Generally, this itself raises the question of whether models should be calibrated locally (indeed we wouldn't expect nationally-consistent model to be optimal everywhere, even if parameters of said model are estimated locally). More specifically, the model performs quite strongly in Illinois, which to me raises the question of whether the strong compound Tmax-SM impact is really just because the national model works very well in Illinois (i.e., a result determined by methods, so not very robust).

The data-driven approach also is attractive because it allows the 'most important' months to be identified. However, I have doubts about the methods for this around the selection of the earlier among collinear climate signals (see detailed comments). Further, it's not only collinearity in time, but among variables at the same time, that matters, as you discuss in lines 280-90. Leaf- to field-scale experiments show that light is very important for crops, so that it is excluded from the modeling is a methodological choice that requires careful interpretation. Another example is the idea that temperature is a strong predictor because it encapsulates many moisture and heat related stressors, as in many of the cited papers. Point is, as a result of these assumptions regarding variable exclusion in the methods, the model specification is actually not as data-driven in the end, so worth considering other approaches with their own strengths/weaknesses:

I think an alternative approach could be to compromise a bit on data-driven model specification and simply prescribe the model structure a priori. This is appropriate because you cite much literature in your introduction on why and how compound extremes should matter, so you have a prior to base the specification on. You can then run the stepwise model selection on a smaller set of predictors for each county and see if results are robust, e.g. Illinois/August still pops out. I understand there is a compromise in this alternative, but it might complement and add confidence given concerns about the original approach. It might also add some confidence to run the panel regression for the full national model (i.e. what is the national value of the coefficients in Fig. 4a).

RESPONSE: We thank the reviewer for generally commending the statistical framework and agree with his main criticism with regards to the mismatch in scale between predictor selection at national scale and model fitting that took place at county scale. Our initial intent was to conclude one unique set of predictors for all counties to facilitate interpretability and avoid potential overfitting at the local level. Nevertheless, we agree that such approach means that predictors are not optimal for each county which can introduce ambiguity when interpreting results. Furthermore, we do agree that the selection of the earlier among collinear climate signals needs further justification. In order to address these issues, we re-ran our analysis with a modified methodology in line with the reviewer's suggestions (see Figure R1). The predictor selection and model fitting is now set to run strictly at county scale.

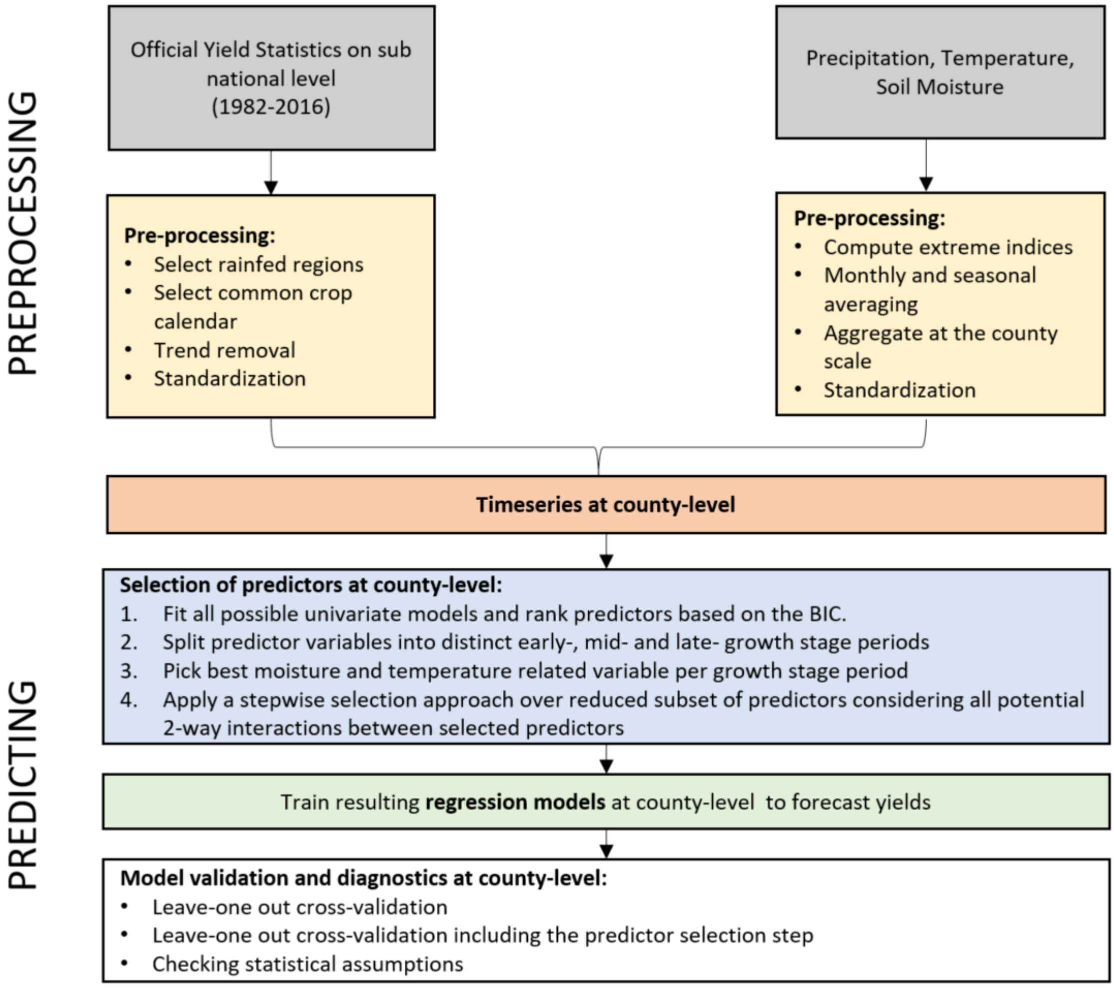

Figure R1. Overall modelling workflow applied for this study linking US yields to weather and climate variables

In order to reduce overfitting concerns, we ran a model one out of sample cross-validation that includes a predictor selection step (Robust-OOS) and limited the number of potential predictors to: Minimum Temperature, Maximum Temperature, Root Zone Soil Moisture and Excessive precipitation. These predictors are supported by main findings in prior literature that highlight the damaging effects of chilling conditions, high temperature, water stress and excessive rainfall on crops grown in the US (Carter et al., 2018; Gu et al., 2008; Li et al., 2019; Mourtzinis et al., 2015, 2019; Ortiz-Bobea et al., 2019; Zipper et al., 2016). The choice to exclude actual evapotranspiration and shortwave radiation from the modeling step is motivated by our intent to particularly focus on the effects of temperature and moisture climate variables on soybean yields. Shortwave radiation and actual evapotranspiration are important variables with respect to physiological mechanisms in crop growth. Nevertheless, shortwave radiation is often highly correlated with temperature in the summer and actual evapotranspiration is influenced by soil moisture, temperature and crop growth making it particularly tricky to study all these variables together in a simple regression framework. This is not to say that there is no benefit in including these variables alongside temperature and soil moisture in a data-driven framework but such exercise requires further careful analysis that is not the focus of this work.

With respect to the selection of the earlier among collinear climate, we no longer intervene manually with predictor selection and only monitor multicollinearity concerns

using the variance inflation factor (VIF). In the latter case, a flag is raised if the VIF exceeds a value of 3 for any variable used to fit the final model at county scale (Carter et al., 2016; James et al., 2013). The resulting general model performance, county scale coefficients, selected predictors and associated timing within the season are represented in Figures R2, R3, R4 and R5 respectively. Further robustness plots with respect to selected predictors and associated timing are represented in Figures R6, R7 and R8. These plots display the most frequently selected predictors and associated timing in addition to how frequently they've been selected across the robust one out of sample cross validation (Robust-OOS). Figures R1, R2 and R3 will replace the initial figures in the preprint main text. Figures R4 and R5 will be added to the appendix. Figures R6, R7 and R8 will be added to the supplementary material.

Finally, in the submitted preprint, Illinois was highlighted for being a major soybean producing state in addition to the relevance of summer moisture-temperature interaction terms within the state. Southern states still showed in previous results and in updated ones presented here strong sensitivity to temperature and moisture generally in line with Mourtzinis et al. (2015) and Zipper at al. (2016) (see adjusted Figure R3).

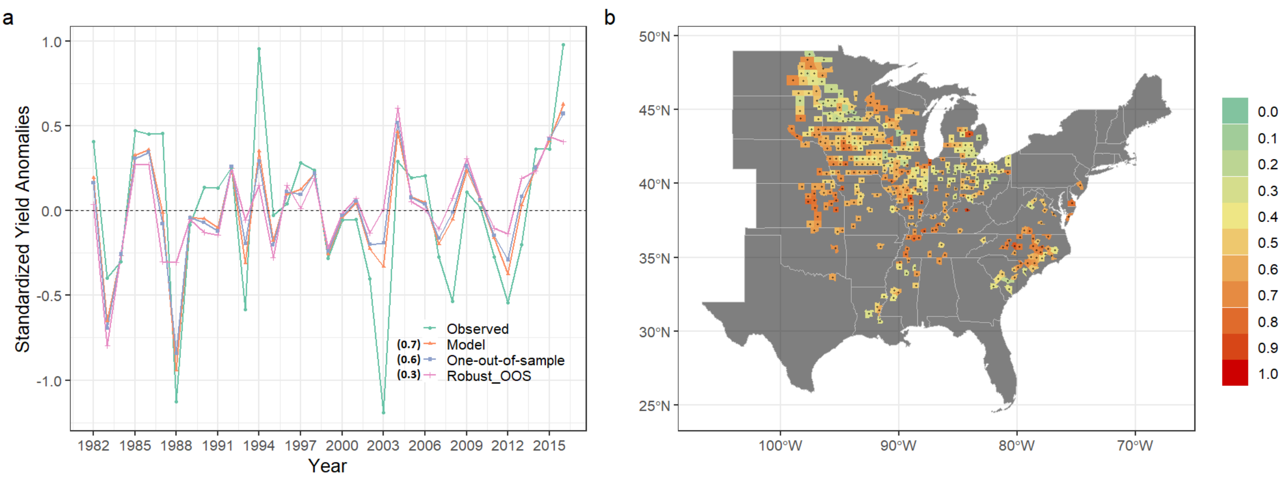

Figure R2. Similar to figure 3 in initially submitted preprint. Only difference relates to the addition of a robust out of sample prediction time-series (Robust_OOS) constructed by a cross-validation step that includes a predictor selection step at every iteration. R-squared values are 0.7, 0.6 and 0.3 for modeled, one out of sample and robust one out of sample predictions respectively.

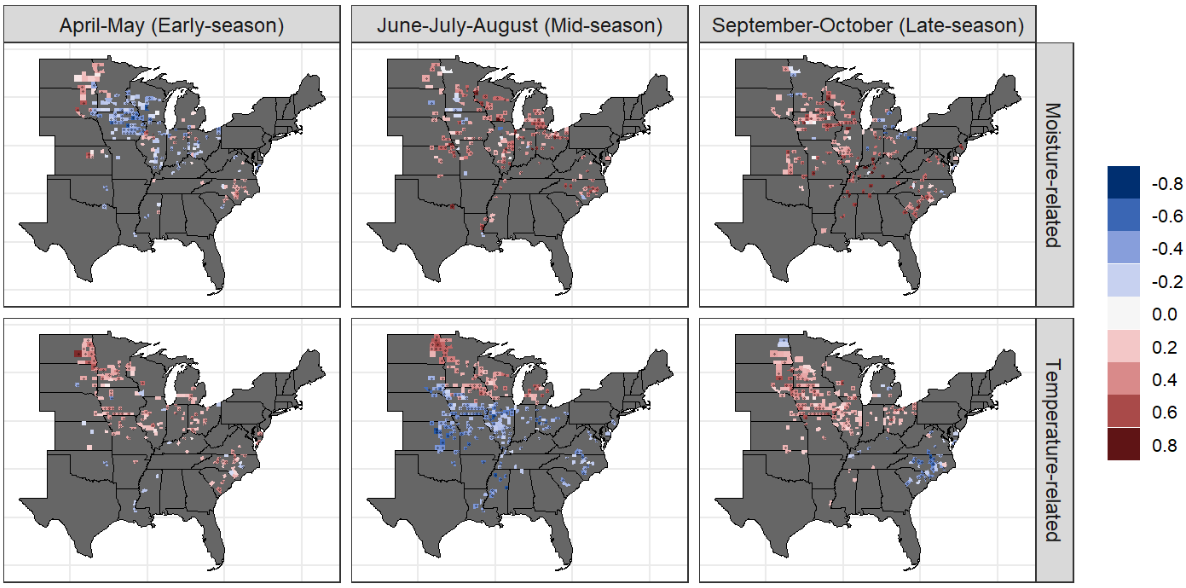

Figure R3. Region- and season- specific estimated sensitivity coefficients for soybean yields to moisture and temperature related predictors. Stippling indicates statistical significance from a t-test at 95% confidence level. Values of coefficients are interpreted as the change in soybean yield standard deviation from a one-standard deviation change in the considered predictor.

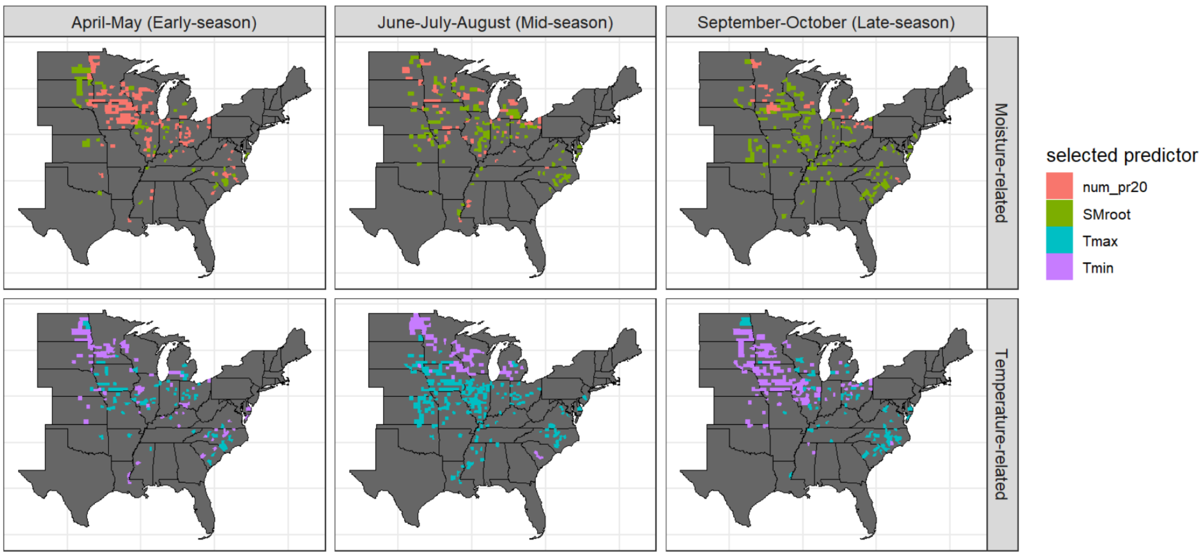

Figure R4. Region- and season- specific selected temperature and moisture related predictors.

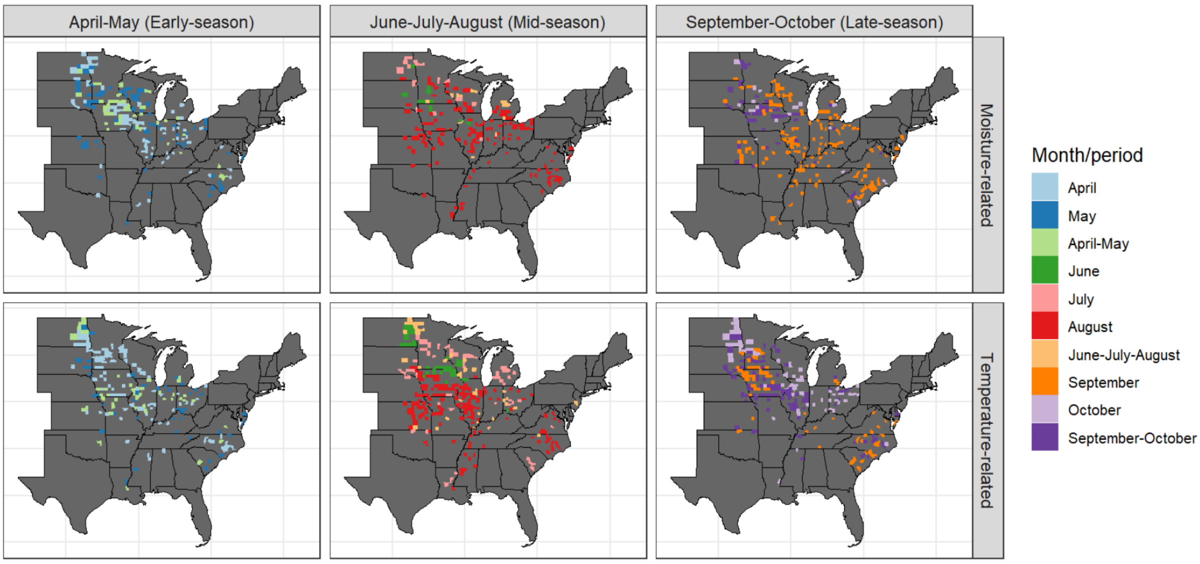

Figure R5. Month or period selected for predictors shown in Figure R4.

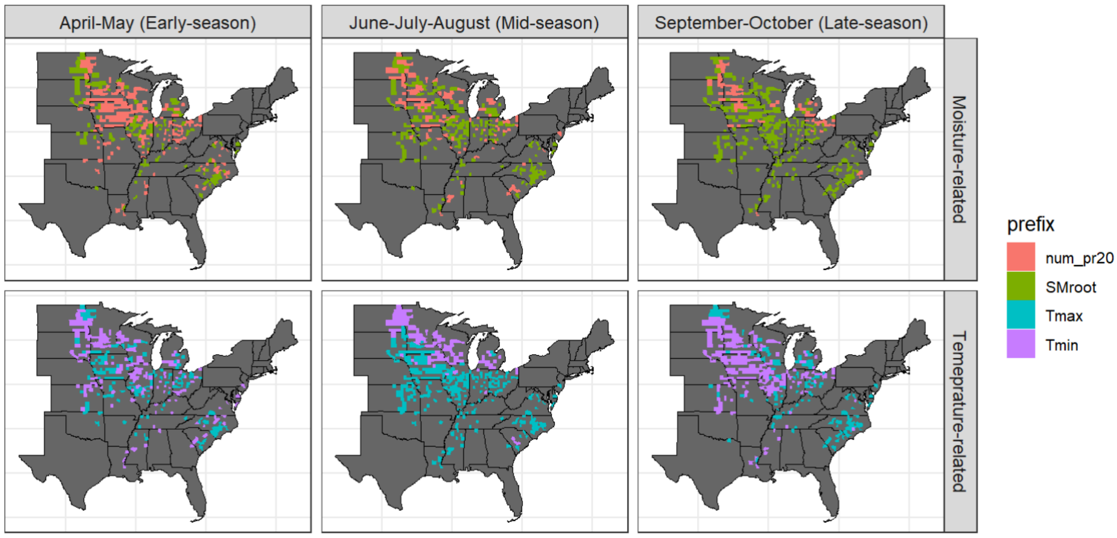

Figure R6. Most frequently selected predictors via the robust-OOS

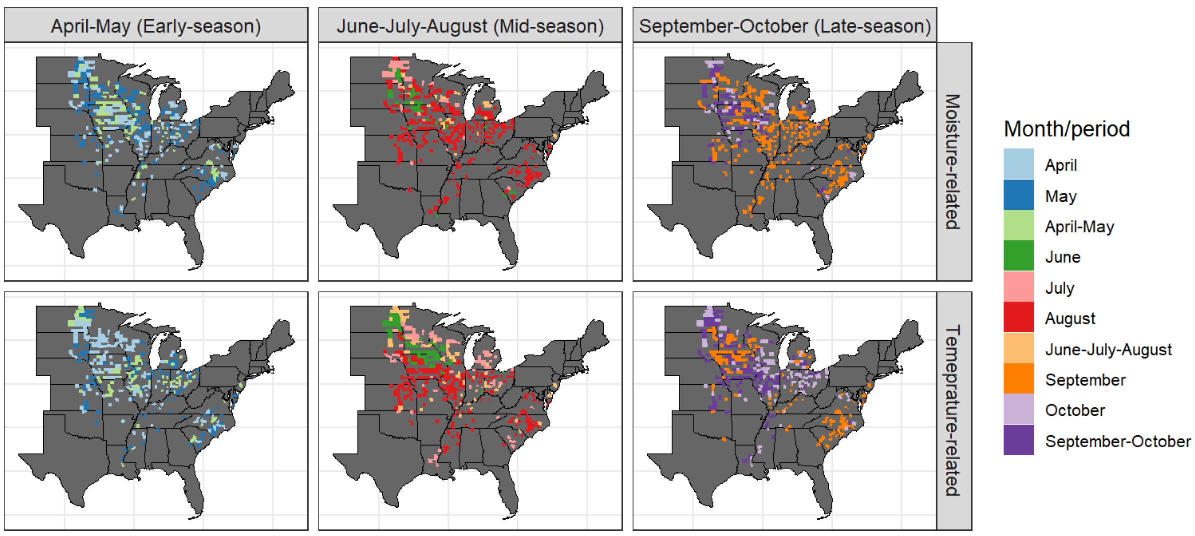

Figure R7. Most frequently selected timing via the robust-OOS

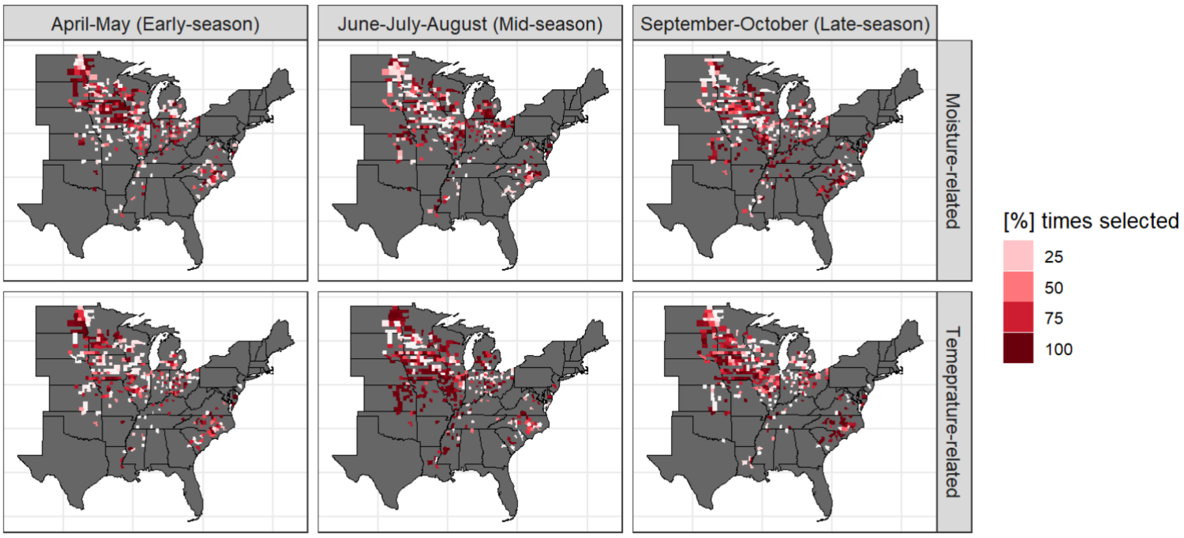

Figure R8. Frequency of the most selected predictor and timing via the robust-OOS

Novelty and advancing understanding:

I really like how this paper clearly puts data and nice visualization to the idea and existence of examples of compound extreme impacts on crops. It also goes into some detail on soybean, a crop for which there is somewhat less attention on the topic. However, I think the core conclusions of this paper have essentially already been established. For instance, the Illinois case study in Figure 5 is an excellent visualization, but its message essentially quite similar to Kent et al. (2015, Fig. 2c, a great paper on maize which might be a handy reference to include) combined with what was published in Matiu et al. (2017), namely that such compound impacts do occur in places. It’s useful that this paper points out that this occurs for this particular crop and location, but a similar point has been made in Ortiz-Bobea et al. (2019, also a great paper that probably needs to be referenced in this paper). Examining trends in concurrent heat-drought is also a useful topic, but has been covered in some detail in e.g. Sarhadi et al. (2019) and Lesk and Anderson (2021). Overlap with past research is a great contribution, but I think it does demand that this study go a bit deeper.

For instance, I think the study could choose one result to go into some more detail on to really gain some new insight. One could be how exactly these extremes are impactful in some places, less so in others, and some of the uncertainties and challenges around understanding this (see minor comments). If the particular importance of compound impacts in Illinois turns out to be a robust finding, why exactly might this be? The authors hypothesize a link to a reversal of the crop induced land-surface cooling during dry episodes, leading to compound impacts (as suggested by Mueller et al. 2016). Many papers have recently speculated about this, and you have the data to examine this in great detail for this location (e.g. compositing and examining coevolution of AET, SM and Tmax timeseries over hot-dry events) and add valuable concreteness to the speculation. Another direction could be to assess drivers of the trends in Figure 6 more concretely, possible roles of agriculture itself in influencing those trends, roles of modes of climate variability and aerosols (e.g. Fig. 6c probably shows some dependence of on the changepoint selection as visible in Fig. 6d, why that might be, and does it say anything meaningful about future change?).

References:

Kent, C., Pope, E., Thompson, V., Lewis, K., Scaife, A. A., & Dunstone, N. (2017). Using climate model simulations to assess the current climate risk to maize production. Environmental Research Letters, 12(5), 054012.

Lesk, C., & Anderson, W. (2021). Decadal variability modulates trends in concurrent heat and drought over global croplands. Environmental Research Letters, 16, 055024.

Ortiz-Bobea, A., Wang, H., Carrillo, C. M., & Ault, T. R. (2019). Unpacking the climatic drivers of US agricultural yields. Environmental Research Letters, 14(6), 064003.

Sarhadi, A., Ausín, M. C., Wiper, M. P., Touma, D., & Diffenbaugh, N. S. (2018). Multidimensional risk in a nonstationary climate: Joint probability of increasingly severe warm and dry conditions. Science advances, 4(11), eaau3487.

RESPONSE: We thank the reviewer for highlighting the visualization component and are grateful for suggestions to further expand the analysis to include more insight into our work. We are also thankful for the suggested papers that we will include in our reference list. Focusing on the hypothesized reversal of the crop induced land-surface cooling effect during hot-dry summer years leading to extreme impacts is a particularly insightful and relevant section to add to our work. In an attempt to illustrate this, we've calculated over the period 1982-2016, the correlations across the month of the year between (AET,TMAX), (AET,SM) and (TMAX,SM) pairs subsetted into hot-dry years (i.e. 75^{th} August maximum temperature and 25^{th} August/September moisture percentiles respectively) and remaining years (Figure R9). The univariate time-series (i.e. TMAX, SM and AET) in Figure R9 are calculated by spatially averaging grids within Illinois. Correlation maps for the entire eastern US across month between actual evapotranspiration and maximum Temperature are presented in Figure R11.

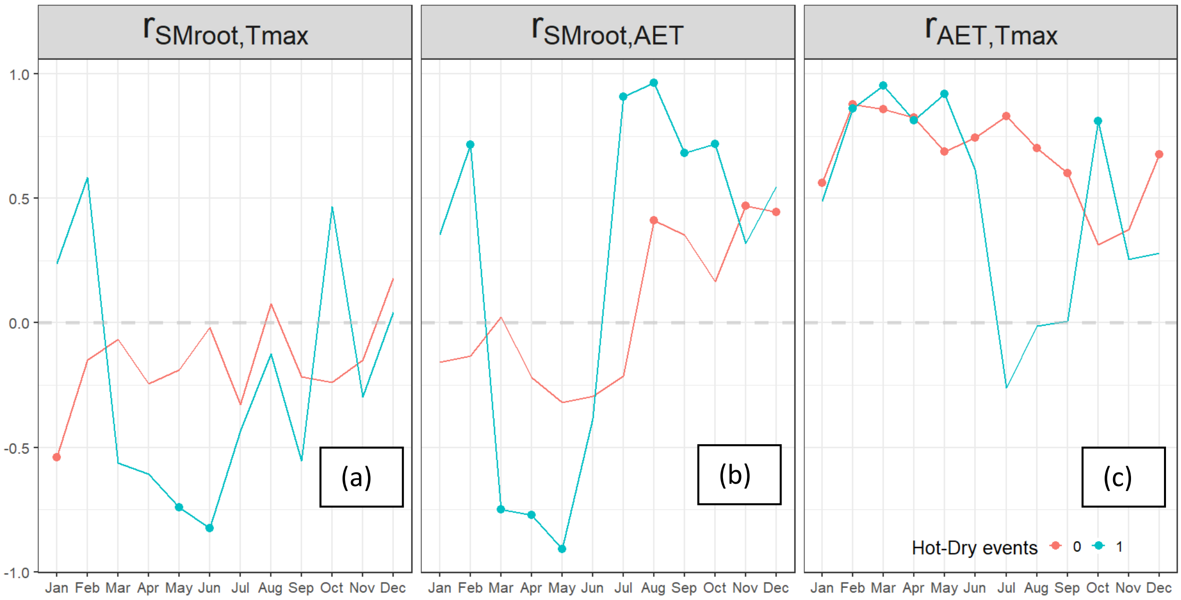

Figure R9. Correlation at monthly scale throughout the year for (SM,TMAX), (SM,AET) and (AET,Tmax) pairs subsetted into hot-dry years and remaining years. Hot dry years are represented by a sample of 10 constituted of years (1983, 1984, 1987, 1988, 1991, 1995, 2005, 2007, 2011 and 2012).

We observe that summer hot-dry years are characterized by a stronger soil moisture and temperature ($r_{TMAX,SM}$) coupling over spring compared to normal years (*Figure R9-*a). Hot-dry years also show a distinct coupling between soil moisture and actual evapotranspiration ($r_{AET,SM}$) when compared to normal years (*Figure R9-b*). The correlation in general between these two variables across the months of the year show negative coupling over spring (March-April-May) and positive coupling over the summer period (July-August-September). We understand the negative coupling in spring as reflective of non-moisture limiting spring conditions. The timing when the coupling sign changes reflects a critical moment in the system when soil moisture becomes limiting. During spring hot and dry years, (Tmax,SM) and (AET,SM) negative couplings are significantly stronger, leading to earlier and more intense depletion of soil moisture, and in turn, an abrupt reversal in the sign of the coupling between SM and AET going from May to June. This moment coincides with the reversal in the sign of the coupling between AET and Tmax (*Figure R9-c*) during hot-dry years. We understand this change in the sign of the coupling between AET and Tmax as an indicator of the reversal of crop induced land-surface cooling. In summary, hot-dry summer years can be characterized by a tendency to have more intense negative spring coupling between (AET,SM) and (SM,Tmax) leading to fast soil moisture depletion in spring and a reversal in the land-surface cooling mechanism over summer leading to important soybean impacts. Average soybean yield values in Illinois subsetted into hot-dry summers vs other years are presented in Figure R10. We will add a combined Figure R9-R10 to the main text in the revised manuscript while including in the discussion other works that addressed the impacts of a warm spring followed by summer hot-dry conditions in the US (e.g. (Sippel et al., 2016)). With respect to other drivers of the observed trends, we will add more information in the revised manuscript on works that investigated decadal variability, role of aerosols and the role of agriculture itself on temperature and moisture trends in the region (e.g. (Alter et al., 2018; Butler et al., 2018; Kumar et al., 2013; Lesk and Anderson, 2021; Mueller et al., 2016; Nikiel and Eltahir, 2019; Weaver, 2013)).

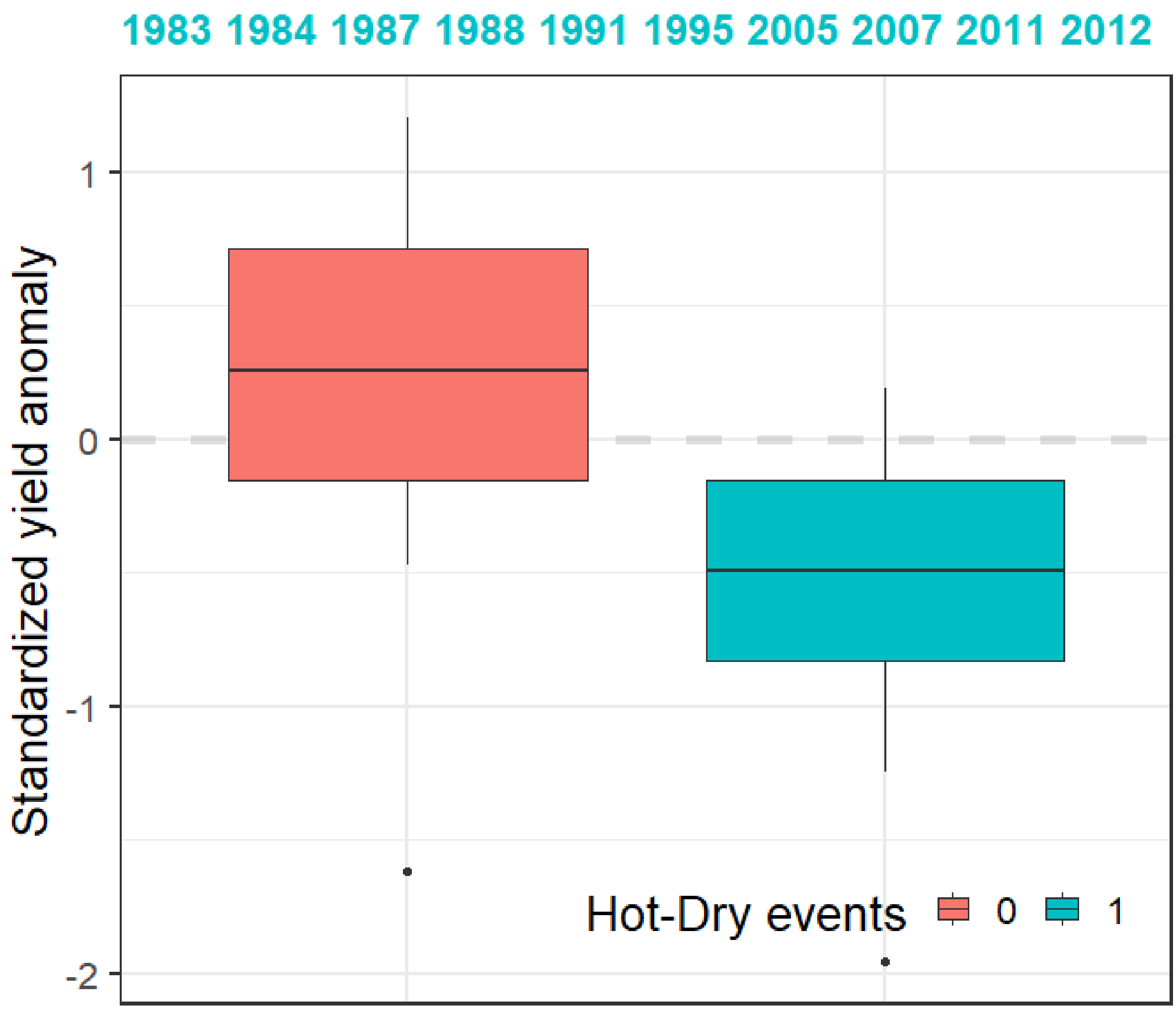

Figure R10. Average standardized soybean yield anomalies over Illinois subsetted into hot-dry years (years added in the header) and normal years.

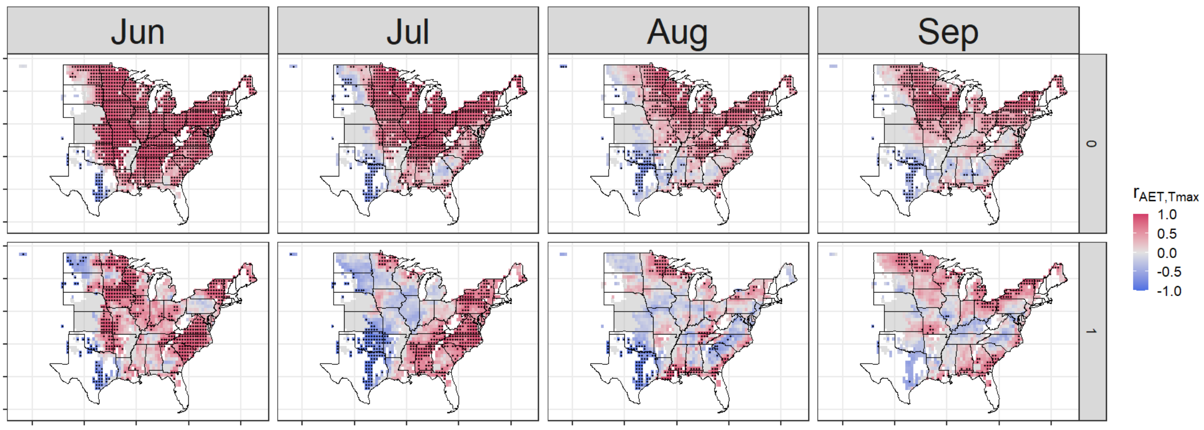

Figure R11. Correlation between actual evapotranspiration and maximum temperature for the month of June, July, August and September subsetted into normal years (0) and hot-dry years (1).

Detailed comments

Lines 25-30: Introduction has great context for why we should care about US soybean. I think it would give helpful context to readers to stay somewhere here that a large portion of soybean is produced for feed.

Cassidy, E. S., West, P. C., Gerber, J. S., & Foley, J. A. (2013). Redefining agricultural yields: from tonnes to people nourished per hectare. Environmental Research Letters, 8(3), 034015.

RESPONSE: Thank you for highlighting this important element. We will include this information in the revised manuscript.

Line 46: Agreed that more attention is needed especially for soy, but probably should cite a few missing studies that have be written on the topic, consider: Rigden et al. (2020), Ortiz-Bobea et al. (2019), Haqiqi et al. (2021)

Haqiqi, I., Grogan, D. S., Hertel, T. W., & Schlenker, W. (2021). Quantifying the impacts of compound extremes on agriculture. Hydrology and Earth System Sciences, 25(2), 551-564.

Rigden, A. J., Mueller, N. D., Holbrook, N. M., Pillai, N., & Huybers, P. (2020). Combined influence of soil moisture and atmospheric evaporative demand is important for accurately predicting US maize yields. Nature Food, 1(2), 127-133.

RESPONSE: Thank you for the suggested references. We will include these in the revised manuscript.

Line 91: There's some evidence that 30mm/day is not a high enough rainfall amount for negative impacts on soy yields in the US (Lesk et al. 2020). I wonder if heat/extreme rainfall would pop out as a compound (possibly positive/compensating) impact on crops if you used a higher threshold.

Lesk, C., Coffel, E., & Horton, R. (2020). Net benefits to US soy and maize yields from intensifying hourly rainfall. Nature Climate Change, 10(9), 819-822.

RESPONSE: Thank you for the reference. When comparing the two predictors: Number of days with precipitation above 20mm to number of days with precipitation above 30mm, we saw more often the selection of the earlier for the fitted models. Nevertheless, the predictors considered here are slightly different than the ones used in the reference study. We will still try out a predictor with a higher threshold. If this is found to be of higher relevance than what we are currently using, we will adjust the revised manuscript accordingly both for the predictor used and associated interactions if present.

Line 115: Selecting the earlier among collinear monthly predictors raises an interesting question of whether the signal for one variable preceding the others in time necessarily means that variable is the driver of the crop response. That is, the later signal could easily have caused the real impact on the crop, and the earlier one is predictive because of its correlation with the later. This is worth justifying more, or at least acknowledging as an important assumption (because it partly determines what variables ultimately can be considered drivers of compound impacts in your methodology). Could be an angle for going to deeper on why Illinois pops out for example.

RESPONSE: We acknowledge that the choice made to select the earlier among collinear variables was more motivated by a practical concern rather than a causal framing. In order to avoid arbitrarily omitting one of these two variables as this was also a concern raised by the second reviewer, we decided to exclude this manual selection step as discussed above and only monitor multicollinearity concerns with the VIF value.

Lines 123-5: Would be good to see more detail on which/why other interactions were left out, and exactly how much 'better' the selected interaction was than other candidates, as this is key to your conclusion. An weaker alternative could be to simply assert that this interaction is one you have a good reason to care about (i.e. the hypothesized interaction is the motivation of your analysis).

RESPONSE: In the revised methodology, all pairs of interactions between selected predictors are considered at county level and only dropped later via the stepwise approach. Figure R11 represent the location and pair of picked up interactions in the final model specification. Most interactions are related to hot-dry summer conditions except for the positive interaction picked up between August maximum temperature and September-October minimum temperature mainly around the state of Iowa. Although we don't focus much on the latter, this might be related to increased impact whenever conditions go from anomalously hot in August to anomalously cold in September-October further stressing crops and reducing the potential positive effects of crop temperature acclimation (see (Butler and Huybers, 2013; Carter et al., 2016) and references therein). Figure R12 will be added to the supplementary material.

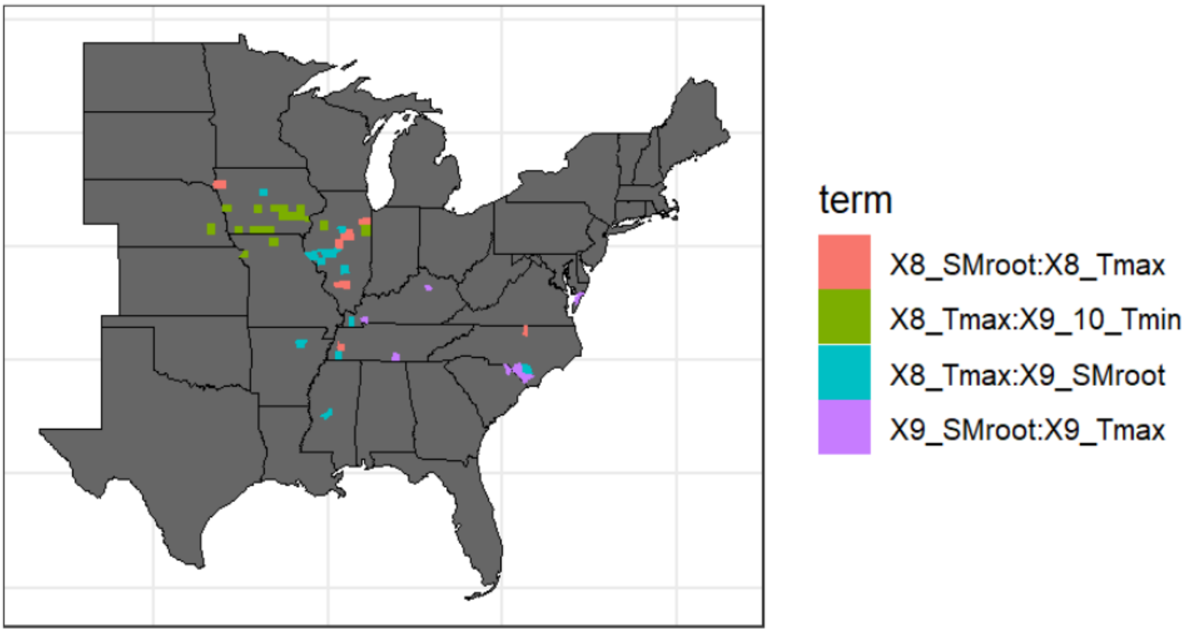

Figure R12. Interaction pairs kept in the final model specification after the stepwise selection approach.

Line 148: Ref needed for energy limited AET

RESPONSE: Actual evapotranspiration is now excluded from the initial model setup and therefore this reference is no longer needed in the text.

Lines 165-6: I'm surprised SM and Tmax are not more strongly collinear in August given the land-atmosphere feedbacks and their involvement in the compound extreme. This should be discussed more and possibly examined in depth. E.g. – are the feedbacks really setting up earlier in the season, so SM and Tmax are more collinear then, and thus get excluded from the analysis? If so, this raises questions of whether August then really is the most important for yield, or just popping up because of this methodological decision (although some other papers you cite do support August being important). There's something deeper to understand here.

RESPONSE: In the adjusted methodological setup, no additional selection steps are imposed besides the initial univariate BIC selection step followed by the stepwise regression approach. With regards to the land-atmosphere feedbacks, we hope the additional analysis presented in Figure R9 somehow illustrates how soil moisture, actual evapotranspiration and temperature correlation can evolve across the season leading up to summer damaging compound extremes. August still pops up as very important month for yields even after allowing predictor selection to run at county scale (see Figure R5). Robustness with regards to the selected predictors is further investigated with a cross-validation step that includes predictor selection at every iteration highlighting again the relevance of August temperature in predicting soybean yield variability (see Figures R7 and R8).

Lines 176-8: I don't see this result supported by data in Fig. 3A, please explain.

RESPONSE: Thanks for pointing this out. The reference was initially intended to Figure 4 where coefficients used to calculate those values are presented. We will adjust this accordingly in the revised text.

Line 185: interesting that model predicts yields better in south (as in Schauberger) – crops here not necessarily 'decoupled' from climate, as warmer seasons benefit yields…

RESPONSE: We agree with this point and this is now better represented in the manuscript by allowing predictors to be selected at the local scale. Northern states do show that a warmer season even during summer benefits yields.

Fig 3b: The question this raises for me is if the north-south gradient in r2 relates to a gradient in suitability of the nationally tuned model. Indeed, since Illinois is a major soybean producer, it's contribution of data to the pooled sample is particularly high (meaning the strength of the prediction could be because the national model fits best there, while other models would fit just as well if calibrated on smaller scales).

RESPONSE: We agree with this point and have adapted the methodology accordingly as discussed earlier. In line with the reviewer's concern, we indeed did find that the northern regions are actually more sensitive to cold conditions rather than hot conditions during the summer period and these are mostly around the earlier month of summer (i.e. June and July) (Figures R4 and R5). As predictor selection is now executed at the county scale, selecting such predictors for the northern states did improve R^2 for this area and reduced the north-south gradient in model performance (Figure R2).

Lines 192-3: how does this square with e.g. Li et al. 2019 who show both very high and very low soil moisture are damaging?

RESPONSE: In this revised model setup, both soil moisture and excessive precipitation are included as covariates of soybean yields with high levels of soil moisture predominantly positively influencing yields and excessive precipitation negatively affecting yields. This is to say that we do find evidence that both very low and excessive moisture are damaging for crops in line with Li et al. 2019. The fact that the RESEST test shows that most model fits would have not improved had we considered quadratic variables is possibly related to the following factors. First, the quadratic association between moisture and yields is more pronounced depending on the month considered. During August, it may be that most losses are related to drought conditions in line with (Li et al. 2019, Figure 1-d). Second, during initial exploratory data analysis we did, we noted that seasonal climatic averages compared to monthly averages showed much more clearly the quadratic relationship to yield. Finally, we also noted that soil moisture had a less pronounced quadratic relationship to yield when compared to average precipitation. All these factors combined might have contributed to the reported results in the preprint.

Line 220: Do you consider AET as a climate variable, or a plant/crop variable (because carbon gain comes with water loss necessarily).

RESPONSE: AET is indeed more of a crop variable as it is the direct result of plant growth which itself is driven by radiation, temperature and soil moisture. Due to the complexity of representing such relationships within a simple regression framework, we opted to leave out AET from the model fitting as discussed above.

Fig 5: very nice figure. Interesting that there is some tail dependence for hot and dry extremes, in that in this bottom-right quadrant you see very extreme joint temp/sm anomalies compared to the others. does this raise questions of causality around the fact that such extreme low SM values can only be reached with very high Tmax? in other words, is the yield impact especially severe because of the compound impacts of temp and moisture, or simply because of extreme moisture impacts that can only happen if T is also high?

RESPONSE: Thank you. We agree with regards to the remark highlighted by the reviewer. The plot indeed hints at some tail dependence which proposes that soil moisture and temperature coupling is stronger in that bottom-right quadrant (i.e. for extreme hot-dry conditions). This likely relates to circulation and land-atmosphere feedbacks discussed previously making it particularly difficult to disentangle moisture and temperature effects during hot-dry summers. Still, we are inclined to believe that impacts are particularly severe due to the compound nature of the stress rather than it being mainly related to extreme dry conditions that only happens to occur during very high temperature periods. This answer is motivated by leaf-scale experiments showing that drought and heat inhibit plant growth via different pathways resulting in more damage to crops when these stressors occur simultaneously (Rizhsky et al., 2002, 2004; Suzuki et al., 2014).

Also could clarify in panel b that the slopes of those lines are the tmax slope + interaction slope * 5-50-95 percentile soil moisture value. Also, given the low sample of hot and wet events, I wonder if it even makes sense to draw the blue line beyond 2sigma Tmax anomalies (there are no such events observed as you say, probably for an important climate reason).

RESPONSE: We will add the calculation clarification to the text and adjust the plot to limit it to physically plausible ranges.

Line 255: there is a strong role of relatively few years in these time series, and possible some signal of climate oscillations, that may be worth at least referring to (Lesk and Anderson 2021 ERL and/or refs therein might be useful)

RESPONSE: We agree with regards to the strong role of relatively few years. We particularly see a high frequency of hot-dry years during the 1980s followed by a reduced frequency afterwards as reported and discussed in the referenced paper (Lesk and Anderson 2021). We will add this to the text and qualitatively expand on the potential role of decadal variability influencing these trends.

Line 290: pun intended?

RESPONSE: Not really, we will use the term "produce" instead of "yield" in the revised text to avoid confusing readers.

Lines 300-310: It's also worth noting that Schlenker and Roberts (2009) and Schauberger et al (2017) too found that the crop damages beyond the ~30 degree threshold were mitigated when moisture was sufficient (either from irrigation or rain). So your findings are in loose agreement with those studies too, in addition to Carter et al. (2016), Siebert et al. (2017), and Troy et al. (2015). I also think it's worth acknowledging that wet conditions may simply prevent very high temperatures, thus reducing exposure rather than sensitivity to heat (see my comments on Fig. 5).

RESPONSE: We will include these references and add the suggested nuance to the revised text.

Lines 309-311: I have a paper in review showing evidence for this globally. If it is accepted in time, it would be a good reference.

RESPONSE: We will keep an eye on that. Thanks!

Lines 311-313: Again, I think you're overstating the lack of attention a bit, see suggested refs above.

RESPONSE: We will include suggested references above and tone down statements regarding the lack of attention in the revised paper.

Nice work thanks!

RESPONSE: Thank you, very insightful review!

References:

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