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Comment on esd-2021-92

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Referee comment on "Resilience of UK crop yields to compound climate change" by Louise J. Slater et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-92-RC2, 2022

This paper seeks to understand historical links between climate and variability in UK wheat yields, and examine the implications of future climate change as projected using a convection-resolving climate model. It considers differential yield responses across growth stages, and tries to then aggregate these stages to assess compensating or amplifying impacts. This latter aspect is the main novelty of this research, which I think is useful and timely. I also appreciate the effort to consider compensating effects between growth stages and between heat and water via this aggregate climate scoring approach, and in Figures 4-5. There is increasing attention to these joint affects, so this paper has the potential to add some clarity here as well.

I have two main critiques that should be addressed. First, the statistical analysis is not adequately described, and based on what I can surmise from the sparse detail, it is probably not the strongest approach. Second, the assessment of future impacts is only driven by data on the climate side, and the crop impacts are only qualitatively discussed. This sells the historical climate-yield relationships short: why not use your historical results for a data-driven estimate of future impacts? Further, your results and other research show how multivariate climate variation/change could lead to compensating or compounding impacts on crops, the potential for which could be more robustly and objectively assessed through a more quantitative approach.

On statistical analysis: The methods is missing any description of the statistical analysis, justification for model specification, etc. This makes it fairly hard to assess the reliability of the results, and what they mean. I gather that the analysis is pairwise two-variable Pearson correlations (yield vs. each climate variable). The authors then use these results to develop a scoring system to combine variables/growth stages, which is not necessarily a bad approach, and the results in Figure 6 seem pretty strong. But this is not a widely used approach, and given lack of detailed methods, it is hard to assess. Rather, multivariate regression (i.e., temperature and precipitation variables for each growth stage all included in one yield model) is what is typical. There are both benefits and pitfalls to it, but it would improve confidence to try this more widely-vetted method and see if results are consistent, and would enable a more self-consistent way to assess compensations. Further, this multivariate regression approach is more suited to then actually projecting yield based on multivariate projections from climate models. You may also consider non-linear yield responses. Finally, only p-values are mentioned in the text, which only provide limited information. I see Pearson coefficients in a table, but their relative magnitudes are not discussed. And the effect size (i.e. slope coefficient) underlying these correlations also provide useful information (steepness of yield response to climate variable), so may be helpful to discuss.

Another methodological issue is reliance on interpreting specific years relative to statistical results, which often lead the paragraphs in the results. I actually really like this for its concreteness, but it is not a super robust method and seems prone to cherry-picking years that fit the narrative. I think this can be remedied by trying to frame these claims more as discussion points and reducing their prominence in the results. Alternatively, you could formalize your method for selecting key years, and describe it in the text.

Another important limitation of this research is its use of only one climate model under only one climate forcing scenario. This leaves important uncertainties in emissions trajectories and climate responses unquantified. The RCP8.5 scenario also is falling out of favor in some circles, as it assumes implausibly high emissions – the authors acknowledge this late in the paper, but don’t strongly justify why we should nevertheless be focusing on an unlikely future. It would probably be useful to include RCP2.6 or 4.5, or at very least acknowledge that the paper doesn’t address emissions uncertainty. The implications of using one climate model should also be justified – is the HadGEM3/HadREM3 nested model particularly useful for the region? The use of a 12 member ensemble helps, but I notice that some years (often with important yield impacts) in Figures 4-5 fall outside the whiskers of the historical model data, raising questions of whether this model can reproduce these conditions (historically or in the future). We know models have such deficiencies –using more than one can help at least partly constrain uncertainty.

Small comment: impacts of rising CO_2 on crop water use will be important in the future, as you mention in the intro. It’s a huge uncertainty and hard to model, but should probably discuss its relevance for your projections.

Finally, I think you could consider in a bit more depth the interactions between temperature and precipitation both in the climate and for crops. For instance, very hot conditions in the UK can often only be reached with a dry land surface (visible as apparent negative temp-precip correlations during production, Fig’s 4-5). Miralles et al. 2019 is useful reference on these processes. Cool and wet conditions could also be linked physically, with implications for crop impacts. This raises questions about the independence of heat and moisture impacts, which is a problem here since they are only assessed one-at-a-time using Pearson’s correlations (multivariate regression could help capture the interaction). Further, joint impacts of changes in temp and precip in the future could be discussed more, see line comments.

Thanks for the nice paper! I think it will be a useful publication once some issues are addressed.

Line comments:

Line 36: Could cite more recent papers on this: Ray et al. 2019, Ortiz-Bobea et al. 2021

Line 55: Ainsworth and Long 2021 would be a useful reference here

Line 56: Soil moisture, precipitation intensity/distribution ref?

Line 80: Could be helpful to motivate this step. Presumably, you do this to remove long-term yield trends (due to technology, climate, co2) and isolate annual anomalies relative to this.

Line 100: This threshold for heavy rainfall should be justified and/or its influence should be tested. For instance, Lesk et al. 2020 found extreme rainfall impacts only at high intensities >50mm/hr for US maize and soy (how this maps to daily scale is unclear, but a 10mm/hr threshold would preclude these damaging intensities). Others have used more holistic distributional measures like the daily rainfall GINI coefficient (Shortridge 2019). I'm not aware of equivalent studies for wheat, but these could be good references to add to Zampieri et al. 2017 in line 56 to bring in studies in sub-seasonal rainfall distribution.

Line 137: I think "1989-1960+1" was not intended to be included in text

Lines 159-161: I think the connection between temperature and precipitation is an issue worth discussing. The wet years with poor yields also tend to be relatively cool (especially during foundation). The dry years tend to be hot.

Line 191: I don't see 1976 on the figure, and 2013 and 2018 don't seem particularly extreme.

Line 200: This somewhat undercuts your preceding results. You do find climate-yield relationships so I don't see strong basis for claiming they are masked by inputs. Further, it is not clear which inputs these would be. I do not know of any short-term adaptive solutions to excess moisture (farmers can improve drainage and soil texture over time, but not within a single season). Further, the usual adaptive management for heat or drought is irrigation, which is not widespread in the UK. Instead, what might be more important/interesting is analyzing (or at least speculating on) the role of inputs in raising mean yields (over decades), and how that may influence yield variability (which you are trying to attribute differentially to climate).

Line 205: This claim is interesting and usefully motivates the next section, but needs work, and here's one place using multivariate regression may be useful. In this more standard method, multiple climate variables together usually explains less than half of yield variation (full-model adjusted $r^2 < 0.5$). Using individual pairwise correlations is less common, and so it's unclear what would be high or low correlation. If the correlations are indeed low in a more robust assessment, it could be because of the myriad other environmental or social factors contributing to yield (climate explains less than half of yield variability).

Line 257-259: Here's a place you could mention multivariate change. Cool and wet foundation phases have been linked to poor yields, and these are connected because it is hard to warm up the surface when soils are wet, and hard to dry out wet soils when it is cool. The projected warmer and wetter conditions are orthogonal to this connection, and some of that warming may help dry out waterlogged soils. Question is whether the warming will suffice to offset the increased precipitation, and this is the kind of question that a multivariate regression model could help answer. See for instance Rigden et al. 2020, Lesk et al. 2021, Ortiz-Bobea et al. 2019.

Line 265: Precipitation may not change much, but there is still warming, which will increase atmospheric vapour demand (all else being equal). So here's a place where some acknowledgement or analysis of multivariate change would probably lead to more robust conclusions about the future. Zampieri et al. 2017 touches on some of this multivariate influence. See also Lobell et al. 2013 with detail on the evaporative role of temperature (it's for U.S. maize, but relevant to interpreting future warming).

Line 277-279: Yes, especially since temperature could have non-linear impacts, see Barlow et al. 2015, a useful reference for frost effects too.

Line 285-288: Great, this offsetting is coming to light as an important mechanism/uncertainty, I just think it could be discussed in more depth.

Line 300-301: Consider using term 'compound extremes' here and in the intro to link to emerging literature on this topic. E.g. Zscheischler et al. 2020

Figures 4-5: I like that this shows the bivariate temperature-precipitation distributions. It is hard to differentiate the grey circles from diamonds, however. It may be easier to see if the 95% confidence ellipses are removed – I'm not sure what they add and could be replaced by simple dots showing point-estimates of mean yield. Otherwise, perhaps the climate model data should be presented on separate axes.

Figure 6: this is a pretty convincing figure notwithstanding my concerns above, but it's hard to understand why the black data are showing y-axis values and an increasing trend, as I don't see yield projection results or methods anywhere in the paper. I assume the points are different years, and aggregate climate scores evolve over time. If so, this data should probably be separate time axes. The black data also seem visually like trendlines on the yield/climate score scatters, but I don't think they are so this may mislead readers.

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