Resilience of UK crop yields to compound climate change

Responses to reviewer comments

August 2022

Editor decision: Publish subject to minor revisions (review by editor)

21 Jul 2022
by Gabriele Messori
Comments to the author:
Dear Authors,

While all Reviewers recognise the improvements in your revised text, Reviewer #1 still raises some concerns on the appropriateness of the statistical analysis. Ensuring this analysis is sound is key to supporting the conclusions drawn in the paper.
I am returning the paper to you for minor revisions, but view a detailed and convincing reply to Reviewer #1 as essential before the paper may be accepted for publication. Please contact the editorial office should you need more time than that automatically allocated by the editorial system.

Best Regards,
Gabriele Messori

We would like to thank the Editor for his evaluation of our paper. We also thank the Reviewers for their additional comments, which have helped further improve the manuscript. Please find below a detailed reply to all Reviewers, describing how we have addressed their comments.

Report 1 (Anonymous referee #3)

In the revised manuscript, the description of statistical methods is greatly improved and a section on assumptions and limitations helps the interpretation of the results and addresses many concerns raised in the reviews. The added section on the statistical approach as well as the section on assumptions and limitations are a great improvement of the manuscript. The concern of reviewer 1 about the lack of statistically significant correlations between climate variables and yields however remains and the newly added multi linear regression does not dispel this concern.

We are glad the Reviewer finds the manuscript greatly improved, and are grateful for the additional comments. Regarding the correlations between climate variables and yields, we agree with the importance of stating which associations are statistically significant and which are not. To this end, we have added sentences explicitly emphasising which correlations are significant and which are not, in our revised manuscript (as described in the next point).

One aim of the study is to investigate “(1) whether statistically significant associations exist between observed temperature/precipitation metrics and historical wheat yields” (L73-74). My reading of table 2 would be that such statistical associations only exist for the production

phase and for region EMYH and NAT. In the conclusions it should be declared that for the two other phases and for the other regions there is no statistical association.

We agree and further elaborate this point in the Conclusion. Weakly significant correlations are found also for some variables in SEE (min_meanT and min_minT) and SNE (var_dailyT), so we write:

> *"Significant statistical associations are found principally in the Foundation and Production phases and principally for regions EMYH and NAT." (see Conclusions)*

Furthermore, the authors do not provide sufficient evidence for the trustworthiness of the multi linear regression model and for many interpretations drawn from it. As an example: From table 4 it appears that the multi linear regression model for the region SFE has a p-value of below 0.05 (I assume that this is the p-value of some goodness of fit test but it would be good to clarify this). However, from table 2 we know that none of the used climate variables is significantly correlated to yields. Could it be that this low p-value for the model is a result of over-fitting? A 5 parameter model fitted to roughly 30 years of data might be a bit much especially if none of the input variables is significantly correlated to the fitted variable (yields).
One way of excluding the risk of over-fitting would be to separate training testing and validation data, but this would require even more data. In my view such a model could be applied to the national yield anomalies as displayed in Figure 1c with detrended estimates of temperature and precipitation if the yield data and climate data before 1980 can be trusted.

The p-value is that of the multiple linear regression model; specified in the Table caption. The variables (max_minT and total_P in the Foundation phase and max_maxT and total_P in the Production phase) are chosen because of the consistent sign of the associations found with yields across regions and nationally (shown in Table 2). The strength of the association does vary regionally, as discussed previously.

The Reviewer makes a valuable point regarding the reliability of a multiple linear regression model trained with only a few decades' worth of available data. To address the Reviewer's suggestion, we computed the "Predicted R2", a metric which evaluates the ability of a linear model to predict responses for new observations (this metric removes a data point from the dataset, calculates the regression equation, evaluates how well the model predicts the missing observation, and repeats this process for all data points in the dataset) for the national model and find that it is 0.091, which suggests there is some predictability, although limited over the 31-year period, using this metric. In the revised manuscript, we discuss the low predictability and emphasise that our model is a proof-of-concept that could be refined once longer data becomes available in the future. Section 2.5 now includes the following statement:

> *"Although the model is significant ($p<0.05$) in EMYH, SEE and NAT, the predictability is relatively low. Alternative metrics could also be selected to improve the model, such as var_dailyT or var_maxT in the Production phase, or days_P>10mm in either phase, or additional variables reflecting e.g. precipitation intensity. These variables have not been tested and should be evaluated in future research, further developing the statistical crop model. Our model is a proof-of-concept that could be refined to improve the predictive skill if further data becomes available."*

Additionally, as highlighted by Reviewer 2, the multiple regression has an improved R2 compared to the pairwise correlations. We have thus included the following text as recommended by Reviewer 2:

".. the strongest associations between climate and yield anomalies may occur during years with cumulative climate impacts across growth stages. Cumulative impacts can be seen in the improved R2 in the multiple regression compared to the pairwise correlations. In other words, the model is capturing something individual variable correlations do not, and this could be that compensation across phases. That said, whether this added explanatory power is from inter-stage compensation, or compensation between variables within a single stage, is not clear from the regression results alone."

Table 4 has also been revised as suggested by Reviewer 2.

My suggestion would be to include a paragraph in the beginning of section 3.3 that states that most of the correlations are not statistically significant. Than one could explain potential reasons for this, including the "combined resilience of the wheat plant (i.e. physiological reproductive mechanisms) and the husbandry skills of farmers and agronomists" but also the length of the observational record. The interpretation of the lacking statistically significance should be that based on the time series of one region one can not exclude that the correlation occurs by chance. As the authors already suggest by marking some parts in table 2 in bold, the fact that correlation coefficients go in the same direction in all three regions could however be an indication for a link between the variables. In combination with a plausible explanation this link can be interpreted, but should not be over-interpreted.

We agree and have updated the text in section 3.3 accordingly:

"Most of the correlations in the historical data are not statistically significant (Table 2). The often relatively weak association between climate anomalies and wheat yields at the level of individual growth stages can be explained partly by the shortness of the observational record, the combined resilience of the wheat plant (i.e. physiological reproductive mechanisms) and the husbandry skills of farmers and agronomists in mitigating these impacts by adjusting to climatic extremes"

In that case, the prior knowledge on the mechanisms that lead from climate variables to yields plays an important role. Without this prior knowledge one could not exclude that the discussed associations between climate variables and yields might just be random. I would therefore suggest to include this prior knowledge in a structured way in the introduction instead of introducing it throughout the results section.

We agree with the Reviewer that prior knowledge plays an important role in the choice of climate variables and their interpretation, in relation to crop yields. This prior knowledge is summarised succinctly in Table 1, and is highlighted throughout the manuscript. We add an additional sentence when introducing Table 1, which states:

"Prior knowledge on the effects of climate in different growth stages guides our choice of climate variables in the study (Table 1)."

I also have a question about your interpretation of figure 4 and figure 5: Do these figures really "reveal the important connection between temperature and precipitation" as you state in line 273 or do they mostly show that precipitation and temperature are not independent from each other. For instance, it is to be expected that in the mid-latitudes wet summer periods are cooler than dry summer periods. This does not necessarily imply that for wheat growth both temperature and precipitation are important.

We have modified the statement, which now reads:

> *"The figures also indicate that temperature and precipitation are not independent from one another"*

Minor comments:

L174-178: I would rather write the climate indicators in words than in formula notation. Without table 2 this is difficult to read.

The climate indicators are explained above in section 2.2 and are very long, which we feel would make the paragraph even harder to read. Therefore, we prefer to leave the formula notation here.

L219-220: I would drop the "RCP8.5 is still a plausible scenario" as it is not fully correct. Furthermore, the next sentence gives the justification for the use of the RCP8.5 scenario.

Agreed. The text has been updated as:

> *While the likelihood of such high on-going emissions is now considered low (Chen, D. et al., 2021; Hausfather and Peters, 2020), the RCP8.5 scenario is commonly used to facilitate detection of climate signals in future projections above natural variations in the climate (due to the large changes projected), and was deliberately chosen as the configuration for UKCP Local simulations to maximise the signal to noise.*

L230-236: Where did you get this infomration from CMIP5 from? This should be referenced somewhere. I just checked here: https://interactive-atlas.ipcc.ch/regional-information

It is not entirely clear which information the Reviewer is referring to as a reference is provided in the text (Kendon et al. 2020). However, we think this URL could be helpful for the readers and have added it in the manuscript, which now reads:

> *"UKCP Local projections also project relatively high temperature changes compared with other climate models (see e.g. https://interactive-atlas.ipcc.ch/regional-information)."*

L234-236: This observation appears to be true for CMIP6 as well. And I think this is really important for the further interpretation of the projected future climate conditions. I think this observation should be acknowledged and discussed in the results section (3.4 or 3.5) conclusions as well.

The sentence has been updated as:

> *"Changes in summer precipitation show a considerable drying in the UKCP Local projections, whereas the CMIP5 and CMIP6 simulations indicate that outcomes with more modest reductions or small increases should also be considered."*

We have updated the results section (in the paragraph that discusses future projections):

> *"It is important to note that the UKCP Local projects stronger drying than CMIP5-6 models."*

And also the Conclusions:

> *"the projected significant decreases in future rainfall (which are stronger in UKCP Local than in CMIP5 and 6) could equally be beneficial to wheat yields"*

L374-378: This list of previous research does not fit to well in the results section in my opinion. To me this reads more as a discussion that helps to put your results into perspective and should therefore not come before your results.

We agree these sentences fit better elsewhere and have moved them to the second paragraph of the introduction.

We greatly thank the Reviewer for their help in improving the manuscript!

Report 2 (Referee #2: Corey Lesk)

Thanks for your efforts revising the paper. I think the revisions add some needed caveats and statistical grounding to the conclusions. I just have a few minor clarifications to suggest:

We would like to thank Corey Lesk for his additional helpful comments on our manuscript.

1) The word 'escape' which now appears first in abstract
It's not clear what this means in the abstract, so this should probably be clarified a bit. Also, I'm not sure this it is a well supported conclusion that the effects you are showing are indeed due to bad weather in multiple growth stages exceeding the ability of farmers to adapt. It could be purely due to plant physiology, and you don't show data on farmer management responses in response to weather. This point on 'escape' seems like more of a speculative discussion point, so maybe try to frame it more as such.

This comment about "escaping" farmers' ability to adapt has now been removed from the abstract and the short summary to avoid any speculation.

A related point is that, on line 349, it's not clear how this inter-stage compensation conclusion is drawn.
One piece of statistical evidence you could note on this front is the improved r2 in the multiple regression compared to the pairwise correlations. e.g. nationally, pearson r is about 0.46 at most, equivalent to r2 of 0.21, but the model adjusted r2 is about 0.3 nationally (~10% more variance explained, or a 50% improvement over the pairwise correlations). In other words, the model is capturing something individual variable correlations do not (and this could be that compensation). That said, whether this added explanatory power is from inter-stage compensation, or compensation between variables within a singe stage, is not clear from the regression results alone. That should at a minimum be acknowledged as a limit on your conclusions, and something worth following up on (could fit well around line 455). Further, it's adds to my questioning your prominent conclusion about multi-stage weather anomalies 'escaping' farmer adaptive capacity.

We have modified the statement on line 349 by including the Reviewer's suggestions:

> *"Thus, the strongest associations between climate and yield anomalies may occur during years with cumulative climate impacts across growth stages. Cumulative impacts can be seen in the improved R2 in the multiple regression compared to the pairwise correlations. In other words, the model is capturing something individual variable correlations do not, and this could be that compensation across phases. That said, whether this added explanatory power is from inter-stage compensation, or compensation between variables within a single stage, is not clear from the regression results alone."*

Additionally, in the conclusions, we acknowledge the limitation, also following the suggestions above:

> *"However, it is unclear whether the added explanatory power of the regression model is from inter-stage compensation, or compensation between variables within a single growth stage. This would be an area for further research. This data-driven regression approach could additionally be refined by including various thresholds …"*

2) Variable selection for regression
I more or less buy this variable selection, but most studies using these methods test a variety of potential models and variable combinations. So simply stating on Line 178-179 that "Alternative metrics could also be selected, such as var_dailyT or var_maxT in the Production phase, or days_P>10mm in either phase, but these are likely to show similar relationships" makes me a bit uneasy. Especially since you talk a lot about change in precipitation extremes in your projections. So maybe simply mention somewhere that future research should further develop this statistical crop model.

Thank you. We agree with this point. The sentence has been revised to:

> *"Alternative metrics could also be selected, such as var_dailyT or var_maxT in the Production phase, or days_P>10mm in either phase, or additional variables reflecting e.g. precipitation intensity. These variables have not been tested and should be evaluated in future research, further developing the statistical crop model."*

3) A few small things.

Line 209: this is rather vague: "meaningful results" how?

By this we meant stronger statistical associations. However, this point does not add value to the manuscript and so for the sake of clarity, these sentences have been removed.

Table 4: would be helpful to see significance (SE or p-value) of coefficients (can use star scheme as in other tables), and units of coefficients (I think t/ha for intercepts, t/ha/°C or t/ha/mm for slopes)

We have added the significance of the coefficients and their units.

Line 380: good point, maybe worth mentioning that this interaction depends on how the link between precipitation and soil moisture change in the future (a topic drawing increasing

attention both in climate and crop science, enabled by the rise of satellite-derived soil moisture observations).

Thank you. We have added this point in the revised text (it is now in the introduction, as Reviewer 1 felt that it didn't fit well in the Results section).

Line 382: contrary to what expectations? Actually, your projections make sense given UK wheat's climate sensitivity. Maybe clarify that you mean contrary to global expectations of declining yields under climate change. Yield gains due to reduced frost risk in cool climates are widely expected. The drying effect of warming temperatures in places where often crops get hit by sodden conditions is perhaps less widely appreciated.

Thank you. We agree and the text has been updated accordingly ("Contrary to global expectations of declining yields under climate change").

A final thing occurred to me reading the revised paper, which is the irony or injustice that the country that initiated the rise of fossil fuels (and gained wealth and adaptive capacity doing so) ultimately ends up benefitting from the consequences of climate change (at least in terms of agriculture). If you want to note the wider implications of your study, you could note this point in the conclusions (and its implications for the UK's ethical obligations to finance adaptation in places that did not get wealthy off of fossil fuels, but where climate change will lower yields).

Some of the authors of this paper do have sympathy towards this view. However, we do have to be broadly independent of statements about international affairs. There is also an issue of impartiality for some authors who work in a government-owned setting. We accept that this is an interesting point, but we request to not discuss it in the revised manuscript, to avoid any suggestion of presenting opinions.

Report 3 (Anonymous referee #1)

I would like to congratulate the authors on an analysis well done and manuscript well written. Thank you for taking serious my concerns and confusion in places. In particular I'd like to mention the new section on assumptions, which is great both in terms of clarity and completeness.

We are thankful to the Reviewer for their positive evaluation of our manuscript and their previous comments, which greatly improved the work!