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:

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.

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.

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.

3) A few small things.

Line 209: this is rather vague: “meaningful results” how?

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)

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).

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.

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).