Review of ***Seasonal crop yield prediction with SEAS5 long-range meteorological forecasts in a land surface modelling approach***

The authors evaluate the use of seasonal (up to 7 months lead) and sub-seasonal (up to 4 months lead) weather forecasts for inputs into a land surface model to predict crop yields and other land surface features. Specifically they use the ECMWF SEAS5 forecasts with the Community Land Model (CLM) model to predict crop yields, soil moisture, leaf area index, and evapotranspiration. They evaluate forecast skill from 2017-2020 over two regions, one in Western Europe and another in Australia. Overall, forecast crop yields were in-line with official statistics but were less successful in capturing inter-annual yield variations. They also find systematic bias in soil moisture estimations when compared to two other observed Soil Moisture products((SMAP L3 and ESA CCI).

This is a well written and executed study that is appropriate for HESS. I have relatively few substantive concerns and several editorial recommendations and questions.

Substantive Concerns

My primary substantive concern is that this is a very short time period (2017-2020) and very limited spatial domain (1 in part of Germany another in part of Australia) from which to draw meaningful conclusions. I understand the limitations on the temporal extent of the study (SEAS5 is a relatively new product that, as far as I know, has not been back cast). However I do not understand the limited spatial scope. If the authors have agricultural stats for those regions in Germany and Australia, could they not conduct them for other regions? Or do national level comparisons using stats from either FAO, the USDA foreign ag-service (USDA-FAS), or another established source of crop yield/production data. Given that the authors do find different levels of accuracy across the two regions, I would recommend adding a least one or two more regions or look at national level accuracy using FAO/USDA-FAS or another established source. A comparison with national level statistics many countries in different biomes and production regimes (rainfed, irrigated, non-commercial, et cetera) would strengthen the paper.

Maybe expanding the spatial scope is not possible given the nature of the products in which case the authors can clarify this in their response to the reviewers. Regardless, in the paper the authors should clarify that forecast experiments over a relatively short period (and in a limited spatial domain) limit the applicability of the study for future use. This should not prevent publication, but the authors should clarify this and use analyses of long-term climatic and production trends in the study regions to elucidate what type of events (climate events, crop failures resulting from non-climate factors) this study might have missed and how that would impact using the results of this study either in the field for further research. Again, expanding the spatial scope (and scales -ie national level) of the test sites could help alleviate some of the concerns that come from such a short temporal testing window.

Editorial Recommendations

- This paper uses A LOT of acronyms. A simple table describing the main products and models (ie acronym, full name, resolution, source, et cetera) would really help the reader understanda and follow the results.
- Question and outline of paper not reached unitl page (around line 105)
- Line~126 Why not include human components among dynamic nonlinear interactions- specifically, deliberate choices made by humans about resource allocation, investment, et cetera
    - Related on line 160, mention that farming decisions are a result of early season weather conditions- and for that matter seasonal and sub-seasonal weather forecasts similar to those used in this paper. To what extent to farmers in the study regions take advantage (and make decisions based on) regional outlooks that use similar forecasts? To what extent might this bias your results?
    - Lines 531-540 Why do you assume that inter-annual variability is driven only by climate factors (it may be, but you should clarify that assumption)? Perhaps the model does not capture inter-annual variability precisely because it is driven by not climate factors (storage, global prices, costs of labor/land inputs, crop-disease, pests). In general the paper and discussion would be stronger if you account for the fact that grain yields are not a strictly bio-physical phenomenon (especially in the commercially farmed and irrigated regions you are studying).
- **Section 2.3-** This paper assumes a lot of familiarity with CLM5. I'm not sure if that holds with all readers of HESS (certainly not with this reviewer). Either in the main text or appendix, can you clarify what the key assumptions of the CLM5 are and how the parameters (ie production relative to input) are estimated or assumed.
- Line 255- the summary of weather patterns in 2017-2020 would be better captured in a figure
- Results—**Section 3.1** The title of the paper and much of the introduction focuses on Crop Yields. However, the first 2 pages of the results focus on soil moisture estimations. This is confusing to the reader. If these results are important (and it seems they are) both the title and intro material should also emphasize the soil moisture estimations as much as the crop yields.