

Interactive comment on “Changes in crop yields and their variability at different levels of global warming” by Sebastian Ostberg et al.

Anonymous Referee #1

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General comments:

The manuscript “Changes in crop yields and their variability at different levels of global warming” tests an emulator approach to estimate crop yield impacts for arbitrary CO₂ emission scenarios based on ISIMIP multi-impact model projections. From a topical point of view, I think this study falls within the general scope of Earth System Dynamics journal. Clearly, the novelty of this paper lies in designing statistical methods to emulate crop yield impacts described in terms of dGMT, going beyond the existing emission scenarios included in the ISIMIP data-cube. Such emulator could provide a shortcut for fast assessment of yield impacts for a range of mitigation targets, thus extending the capacity of ISIMIP ensemble. The authors demonstrate that both mean yields and yield variability are, to a certain degree, directly scalable across emission scenarios, without

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additional explicit RCP-GCM-GGCM simulations. I believe that the ISIMIP data cube alone, as described by the authors, already provides a powerful tool for linking crop yield changes with arbitrary dGMTs, but has limitations with respect to arbitrary CO₂ levels. This is definitely a valuable work, with clear and sound conclusions, and after a moderate revision it would be an original contribution to community of crop impact modellers and for Integrated Assessment Models in general.

Overall, the manuscript is of high technical standard. However, I must say that I had problems with following the methods at some places since various methodical aspects are scattered across the whole document, which makes data reproducibility more difficult. The main “weakness” comes with using the linear-regression (or weighted-average) emulators at a grid basis without further testing its significance. Since the emulators for individual dGMT are based on intercept (a_0) and regression slope (a_1) of the fitted linear models (determining climate- and emission-induced yield changes, respectively), it would be useful to add a step of testing whether or not these values and the fit overall are statistically significant, and exclude those grids for which it is not so. One could expect that yield response along a narrow range of CO₂ increase (such as for a low dGMT) may be quite noisy, and not necessarily captured by linear models in a significant way. This might be the case for some GGCMs and regions where crop yield changes in a response to shifts in T and CO₂ are limited by other constrains, such as nutrient deficiency for example.

Specific comments:

P3, paragraph 115: soya is not a cereal crop

P4, p.150: "...generated for four RCPs..." which RCP scenarios were used here? It would be nice to list the emission scenarios here as this is the first time the scenarios are mentioned in the Methods section.

P4, p.165: "...were forced by climate change projections from HadGEM2-ES, RCP8.5..." I wonder if this statement is correct. The authors refer to "YnoCO₂" also

for other RCPs in Figure 5, if I am not mistaken.

P5, Table 1: I agree that the model-related assumptions have to be short, but some statements in Table 1 are jargon difficult for others to understand. For example, for LPJ-GUESS, is the thermal time needed for maturity adjusted over time? It is not clear from the explanation in Table 1...if so, there is probably no shortening of the growing season effect in this model, which may explain more positive effects of warming on wheat in Figure 2, in contrast to other models. Next, what does “decadal adjustment of winter and spring wheat sowing areas based on temperature” mean? Then, do LPJ-GUESS and LPJmL use no information on fertilization, meaning there is no limitation by nutrients at all? If the same assumption applies for both LPJ-GUESS and LPJmL, why not using the same description for both? Please, use harmonized and clearer descriptions that would allow readers to understand fundamental differences in the models, and which implications it might bring. Overall, I understand that the differences in yield projections due to unique specifications of individual GGCMs are not topic in this manuscript, but it would be very useful to discuss some results with a deeper insight into individual models where appropriate (such as that some models simulate nutrient-unlimited yields while the others not).

p.7: It is obvious from Fig 1 that different dGMT bins would bring different number of years into analysis (1) and (2). Time intervals for some dGMT bins may be quite short (e.g. 10 years), and probably not sufficiently long to smooth possible short-term inter-annual fluctuations and anomalies in simulated yields. In other words, the mean yield change could be very sensitive to anomalous years. A concept of climatic normal is usually used to eliminate anomalous fluctuations. I am not sure about the implications for this approach, but the authors should consider and possibly discuss this aspect.

p.12, Figure 5: please add information whether the linear regressions are statistically significant. Free scaling of y-axis may work better here to see the data scatter more clearly.

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p. 16, Fig. 7: as discussed in the manuscript, negative impacts of CO₂ are contrain-
tuitive, even though they may make sense from the statistical point of view. Maybe a
rule that all negative impacts of increased CO₂ are reverted to zero would make more
sense.

P17. P. 405 and elsewhere: "...change on potential yields..." This statement here (and
also elsewhere) is confusing for me. Do the authors estimate yield potentials or yields
under historic management?

P17.420: (ao(dGMT)) should be (dYCLIM(dGMT))

Section 5, p.23: In my opinion, de-trending of simulated yields within a dGMT bin by
RCP-specific yield averages provides only an artificial variability, since it combines dif-
ferent RCP projections together. I am not an expert in climate modelling, but I assume
that each radiative forcing scenario in a GCM generates a unique temporal variability
of meteorological variables, and I have my doubts that mixing RPC-specific simula-
tions together makes sense in terms of dGMT-specific variability. I am not sure that I
understand the concept in Section 5 correctly though.

p. 25: "...implies that projected impacts at different dGMT levels are not substan-
tially dependent on the choice of emissions pathways." This might be true unless lead
time is considered. Time horizons associated with individual dGMT levels are of high
importance for adaptation and economic processes.

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