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Earth System Dynamics Discussions

Interactive comment on "Agricultural management effects on mean and extreme temperature trends" *by* Aine M. Gormley-Gallagher et al.

Anonymous Referee #1

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This paper looks at the effects of prescribed representations of conservation agriculture and irrigation on mean annual 2m and maximum daytime temperature in CESM. There is some interesting analysis and potential for results that could be useful for the community. There are aspects of the sub-grid scale vs grid scale analysis, and possibility for critique of whether more processes enhance model skill, that are intriguing. The figures are generally well presented. However, there are several issues that need to be addressed.

The paper reads like a combination of previously published results (specifically, the ensembles used are already published in Theiry et al. (2017) and Hirsch et al. (2018)). That might be unfair, but the regression analyses is simple and it seems unlikely it wasn't done separately for CA and irrigation, and much of the explanatory analysis

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references these two papers. It is the responsibility of the authors to show clearly why this is novel compared to what has come before.

The results (as shown in Table 1 especially) are difficult to reconcile with the statements made in the abstract and conclusions. Looking at Table 1, if the smallest RMSE (or the anomalies closest to zero) are considered, the Control simulation is better $\sim$ 2/3 of the time. The abstract says, “our results underline... the need to account for land management in climate projections”. Surely the opposite is true, as the Control scenario does better by the measure most used to assess model skill. Even within the results, there appear to be contradictions. Line 218: “the impact of irrigation and CA on the modeled spatially averaged temperatures... is an overall cooling effect”. Line 223: “for the IRR and CA models... the spatially averaged T2m and TXx warming rates are higher than those of the CTL model”.

Some of the results are presented in such a way as to be somewhat misleading. For instance, the values in Figure 2 (% change in RMSE) with the colored categorization (which, being visual, is much stronger evidence to the reader than a table) can be compared with the equivalent anomaly in Table 1 (RMSE). For irrigation (IRR-CTL) the RMSE T2m (CRU) difference is -0.002, and the figure categorization is - 5-10%. For irrigation (IRR-CTL) the RMSE TXx GHCNDEX difference is +0.004, and the figure categorization is 0-5%. i.e. a difference in RMSE that is twice as big, is categorised as half the size in terms of color. This means that it looks as though the CA and IRR simulations are doing much better than if the simple RMSE is considered.

Some of the results are inconsistent with each other. For instance, on line 182, the range of temperature anomaly compared to observations is given as 0.007 - 0.03 in the text, but in Table 1 it is 0.007 – 0.024 (usually a number is rounded down when the last value is below 5). Or line 179 where the text says 0.004 for the Control, but Table 1 says 0.006. Figure 4b shows TXx HadEX2 on top of IRR and CNT much higher, but Table 1 shows CTL and IRR with differences from HadEX2 of 0.008 and 0.012 respectively.

The introduction does not do a good job of introducing the main point of the paper. The first paragraph sets up the issue that observations show less warming in TXx than T2m, but models get it the other way around (TXx warms more than T2m). But we basically don't hear about this issue again. Subsequent paragraphs in the introduction are brief summaries of key papers (by the authors) and do not provide the cohesive overview of each topic a reader needs in an introduction, instead being based around a particular reference.

The abstract doesn't do a good job of summarizing the paper. It goes straight to "we did things" without explaining why the reader should be interested, or what the relevance is, then goes to "it's important" without presenting the evidence of why it is important. It provides no context for what was done, then the emphasis on the "pulse cooling" in the abstract is not followed through in the results. The subgrid scale aspect seems to be the most novel part of the paper, but since the rationale for it isn't explained clearly in the methods or results, it's a minority of the results section, there's no comparison for the scale of the water vapor feedbacks, and no clear explanation of why the water vapor feedbacks are responsible for the differences between the grid scale and subgrid scale, it's not convincing.

The results section has paragraphs where the numbers in Tables are repeated, (with some unexplained deviations, as discussed above) with little attempt to give a view of what the results mean for the model's performance. There is no point in having a table if the text repeats what it says, and vice versa. There is absolutely a place for straightforward analysis and simple statements, but it needs to enhance clarity, not just be a list.

The Tables and Figures all need more detail in the captions, to help explain what they are and why they differ (and why those differences were deemed necessary). For instance: Table 1 – which years go towards the values? Table 2 – what are the "impact of irrigation and CA on various climatological values", because 0.026 K yr-1 for T2m doesn't make any sense as a number for the Control if it's supposed to be IRR – CTL as

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described in the caption. Figure 4 – presumably when it says “average” it’s the mean, but then line 378 says “median”, which is a notably different average.

The results section has times where it would benefit from showing more evidence. For instance, the two paragraphs starting line 228 speculate that latent and sensible heat partitioning and changes in ET are responsible for the differences between CA and IRR, and the Control. But instead of exploring these and showing how the latent heat changes, there is just references to previous papers. I.e. it is not a result, it is a summary of previously published research. Similarly, the paragraphs line 289 – 315 contain a lot of speculation and references and not enough evidence.

The discussion section is missing, at the very least: cloud uncertainties (different models do them differently, and they are notoriously difficult to resolve, so that these results rely on them is problematic); uncertainties in the partitioning of latent and sensible heat when albedo changes (i.e. Bowen ratio); the fact that the CA increase in albedo is a huge assumption, as soil albedo is very heterogeneous and dependent partly on soil moisture (thus the CA modeled might be doing the wrong thing in many areas); the representation of transpiration in the model, and the fact that presumably the crop/vegetation cover is the same when in reality these changes would affect the LAI of the crop; the canopy interception and soil interception representation in the model, which affects the evaporation and thus how much the irrigation and CA affect the evaporation.

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