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Earth System Dynamics
Discussions

Interactive comment on "Ocean-atmosphere interactions modulate irrigation's climate impacts" *by* N. Y. Krakauer et al.

Anonymous Referee #3

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1) The question that motivates the study is very interesting, but I think that the authors overstate their case too much to be convincing in their conclusions. My overall opinion is that this paper, although dealing with a novel and interesting question, is too modest in its present state. The simplicity of the numerical design and the performed analyses suggest the authors overlook the complexity arising from coupling land, atmosphere and ocean in climate models, which is problematic for publishing in "Earth System Dynamics".

2) My main concern is that the reported differences between the interactive and fixed SST runs are weak and moderately significant, both at the large scale (in term of p-value in Table 1), and over the maps, in which the areas with insignificant changes are much larger than the ones with a significant change. The main exception is the SSTs themselves, but this is not very informative given that their variability is very different

by construction in the two kinds of experiments (see also my comments a-b below).

Most of the recent papers that deal with tiny changes against the internal variability of the climate system use an ensemble approach to be more convincing from a statistical point of view, and I would like the authors explaining why they did not do the same.

Even if we accept that the comparison of single members for each experiment is justified, information is missing in the paper regarding the experiment design and the subsequent statistical analysis:

a) I understood that the fixed SST simulations were analyzed over 50 years, while the SST forcing is available over 9 years only (1996-2004): how is it done, by cycling the 9-yr forcing over the 50 years? If so, it implies a very different variability to the one of the interactive SST simulations, for which we also need to know if you impose or not an increasing amount of GHG and aerosols, as this may induce a trend in addition to the seasonal to inter-annual variability (I'm not a specialist of slab oceans, so I need that kind of information to make sense of the results). I also wonder why the fixed SST runs do not rely more strongly on the AMIP protocol, which comes with a much longer SST forcing data set, starting in 1979.

b) The above information is important since the significance of the analyzed differences is tested based on Student's t-test, which basically compares the mean difference to the variability (standard deviation) of the two compared samples. Regarding the test, I did not understand what was behind the following mention (p3, L8) "with the degrees of freedom adjusted based on the lag-1 autocorrelation of the time series". Since the significance depends on the degrees of freedom, I recommend clarifying this point in the paper.

c) The paper relies on comparing the effect of irrigation in fixed and interactive SST experiments. These effects are respectively called Δ_A and Δ_O, and calculated as the difference between an irrigated and control experiment, in each of the confirmations. The rationale is that if there is a significant difference between Δ_A and Δ_O, then it means that the interactive SST influences the response of the climate to irrigation. But we may imagine another explanation to a significant difference between Δ_A and Δ_O, because the two control simulations must be different (AMIP and CMIP simulations are different), and may drive the differences between Δ_A and Δ_O. Thus, I think the authors need to compare the differences between the two control experiments and the ones between Δ_A and Δ_O before concluding anything.

3) I also regret that the analysis of the changes and the attempt to give them a physical explanation is rather superficial. The studied changes are induced by enhanced moisture input to the atmosphere over irrigated land, and the atmospheric humidity is not analyzed. The only circulation variable is the 300-mb height, and no mention is made to moisture convergence and convection for the atmospheric compartment, nor to monsoons and surface ocean currents. Yet, if there is an influence of the interactive ocean on the response of the climate to irrigation, it should imply that irrigation changes the ocean's behavior between the two interactive SST runs.

4) An illustrative example of the overstatement and lack of physical insight that can be found throughout the paper is the analysis of the precipitation changes over eastern Africa. We are asked to compare the Δ_A and Δ_O in precipitation in MAM over eastern Africa in Fig. 5, but there is almost nothing! I'm not even sure there would be something discernable with a magnifying glass. This extremely weak change receives the longest explanation of the entire paper, with a 10-line paragraph, but it ends with a rather weak and speculative conclusion (p7, L13-15): "Thus, ocean-atmosphere interactions may importantly affect the magnitude and location of non-local irrigation impacts on climate, such as those potentially implicated in precipitation trends in eastern Africa."

5) Minor comments:
p2, L22: summed should probably be replaced by averaged
p3, L10 and p4, L24: do you analyze the rms or the standard deviation? The latter seems more informative, as it excludes the effect of differing means.
P4, L14: when analyzing large scale means (land vs ocean), it's abusive to write that "interactive SST spreads the cooling": you need maps to draw this conclusion.

Caption of Table 2 should mention that the values correspond to the northern hemisphere only.
Caption of Table 3 should mention that the studied variability (by means of rms) is spatial (and not temporal, which could be worth an analysis too).
P7, 1st L15-16: please clarify "supporting the role of air-sea interaction in driving the divergence in surface air temperature and geopotential height irrigation responses between the fixed-SST and interactive-SST simulations."
P7, L17-18: please clarify what is really meant by "the same stationary wave pattern [. . .] is found [. . .], with shifted phase"
P7, L5 (according to the numbering in the pdf): please clarify "The role of interactive SST in non-local irrigation climate forcing"

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