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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 #2

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1 General comments

The present article addresses a research question that, as of late, is being investigated by an increasing number of studies, i.e. irrigation as a man made land cover change that substantially alters the (global) climate. Many of these studies rely on a range of climate models, and the present study presents a valuable contribution by demonstrating how the setup of these models (treatment of the ocean) can affect the simulated impact of irrigation. As the authors, state there has been a number of studies, both with and without interactive SSTs, but to the best of my knowledge, no study investigates the respective effects in detail. Here, the authors do a very good job presenting their motivation and placing their study within the existing body of literature, giving a concise overview of recent studies.

The authors address the issue of interactive vs. prescribed SSTs by running the GISS model in a setup with a slab-ocean and compare the simulated irrigation induced impacts to simulations in which SSTs were prescribed. This approach is very intuitive and, in my opinion, the authors choose a good approach, by keeping the introduction short, but providing sufficient information about the model and the setup.

The result section of the article is well structured and written using appropriate language. There are only few sentence that required a second reading and these could be fixed with a minimum effort (see point by point). Additionally, the figures and tables are well compiled, making it very easy to follow the authors.

However, I am concerned with one of the conclusions the authors appear to draw from their results, namely, that simulations, with an interactive ocean model are better suited to represent ocean-atmosphere interactions and are therefore superior to those with prescribed SSTs. This conclusion has substantial implications for anyone who will investigate irrigation related impacts using a climate model, as it would basically disqualify studies using prescribed SSTs. I am not convinced that the methods/ results suffice to make such a substantial claim. Nonetheless, despite this disagreement on the conclusion, my opinion is that the authors present a well constructed study that provides instrumental insights into an important topic.

2 Specific comments

With respect to the method section of the article, I thought that it was appropriate in length and style and I generally liked that it was very concise. However, I think it would be beneficial if the authors could provide some form of measure or graph,

demonstrating to the reader that the model is in fact in equilibrium after 10 years of spin-up time. The authors state that, if possible, irrigation requirements are satisfied from the river discharge. This should have a substantial impact on the land-surface hydrology and by that possibly on the ocean as well. This is especially relevant because in the discussion section the authors give an interesting comparison to another GISS-based simulation in which the system was not in equilibrium. Here, they speculate that the differences in the ocean's response to the irrigation forcing could be related to the fact that in the present study an equilibrium response was investigated whereas the other study looked at a transient response (and not to the fact that one study used a slab-ocean and the other a fully dynamic 3-d model).

Furthermore, maybe the authors could give a brief reasoning for why they used the slab-ocean instead of a fully dynamic 3-d model (as I understand, some ocean modellers would claim that the 3-d model is a better representation of reality)?

As stated above, my only real concern is that, from the results, I do not arrive at the same conclusions as the authors (that simulations, with an interactive ocean model do a better job at capturing ocean-atmosphere interactions and are therefore superior to those with prescribed SSTs). An example for this conclusion is the claim at the end of the abstract, i.e. that (simulation-based) attribution studies should include interactive oceans. This I find questionable for the following reasons:

It can be argued that an interactive ocean component introduces additional variability and uncertainty into the model. Thus, by prescribing SSTs (if these can be considered to be reliable) the model is actually constrained to a more realistic representation of present day climate than a model with an interactive ocean.

But more importantly, the authors compare individual simulations, not ensembles, and it is debatable whether a ttest is a sufficient tool to evaluate if differences are related to changes in the model physics. Often even slight changes (not even in the physics), e.g.

in the initial conditions, are sufficient to cause the distributions in two simulations to be statistically significantly different. Taking this into account, the differences between $Delta - A$ and $Delta - O$ on the land surface appear to be quite small, in figures 2,5 and 6. This is also indicated by the mean differences $Delta - Delta$ for most variables not being statistically significant.

Thus, another possible way to interpret the study's results is that, with respect to the land surface, simulations with a model setup with a slab-ocean are quite comparable to those forced with prescribed SSTs. This shows that the irrigation induced impacts are persistent and so strong that they are not concealed by the (ocean) model's internal variability. Additionally it indicates that a model configuration with an interactive (slab-) ocean is a very suitable tool to investigate irrigation related climate impacts in the future when reliable SST-data may not be obtainable.

If the authors maintain the claim that the statistically significant differences in the simulated irrigation impact are due to atmosphere ocean feedbacks (that can only be accounted for with an interactive ocean model), I would urge them to demonstrate this using ensemble simulations. If it is not feasible to conduct ensemble simulations, it would greatly help if the authors could at least perform one additional simulation with an interactive ocean and slightly altered initial conditions. With this simulation they could show that there are no statistically significant differences between two simulations that are based on the same model setup but using slightly different initial conditions. This would give some confidence that the significant differences they show are related to the ocean atmosphere feedbacks, even though I think ensemble simulations would be the preferable approach.

Furthermore, for the claim that simulations (for the present day) using prescribed SSTs may miss important effects, it would be very helpful if they could give some indication for these effects also existing in the real world, e.g via a comparison to observations of

e.g. precipitation, surface temperatures or the wind field.

3 Point by point

- P. 1, l. 3: In the title it is "irrigation's climate impacts"?
- P. 1, l. 3: Maybe better "... contemporary irrigation (the geographic extent and irrigation intensity correspond to those of the year 2000) ..."
- P. 1, l. 9,10: as stated above, from the results presented, I do not arrive at this conclusion, (that attribution studies should include an interactive ocean).
- P. 2, l. 5: Maybe better "... to persist and to be transferred between ...".
- P. 2, l. 5: Maybe better "... the irrigation related climate forcing ...".
- P. 2, l. 22 - 24: Would it be possible to convert these values to km^3/a as this would make it easier to compare them with other studies ?
- P. 3, l. 1 - 2: Would it be possible to give more information on how these 10 years were determined?
- P. 3, l. 3: Maybe better "For the climate variables of interest ...".
- P. 3, l. 5: Maybe better " ... interactive SSTs ...".
- P. 3, l. 7: Maybe better "... using a Student's ...".
- P. 4, l. 15: Maybe better "... that directly quantify the conditions and the moisture status at earth's surface ...".

- P. 4, l. 7: Maybe better " The irrigation-induced ... over irrigated areas, spreads ...".
- P. 4, l. 12: Maybe better ". Irrigation-induced changes in the surface latent and sensible heat fluxes ...".
- P. 4, l. 18: I think the information in the brackets is not required as the terms SST and soil moisture already imply the geographic location.
- P. 4, l. 18: Maybe better ". Over land, the cooling ...".
- P. 5, l. 19: Maybe the sentence could be split up. At the moment it reads as if the mean amount would refer to the cooling.
- P. 4, l. 20: Maybe better ". Over the ocean, the cooling ...".
- P. 4, l. 23: Reading the sentence I was wondering whether I had overlooked the zonal means. As they are not shown maybe its better to just refer to the global mean.
- P. 6, l. 2: I find this difficult to see in the figure. To me it appears that over land areas the patterns of pronounced impacts especially in Southern Asia are actually quite comparable. Maybe an irregular spaced colorbar could be helpful to see differences between 0.4 and 0.8 K.
- P. 6, l. 12: The wave patterns are not exclusiv for the q-flux simulations, but there is also a wave pattern present for fixed SSTs in the Southern Hemisphere in JJA.
- P. 7, l. 15 - 16: Maybe better "... air-sea interactions ... the divergence in the irrigation responses (surface air temperature and geopotential height) between ...".

- P. 7, l. 17 - 18: Maybe better "... with the phases shifted between the interactive SST and fixed SST simulations ...".
- P. 7, l. 6 - 7: Maybe better "... study using a different atmosphere and land surface model and found that ...".
- P. 7, l. 10 - 15 : This is possibly true, but just as likely the differences are not related to the model physics. This is very hard to tell from comparing individual simulations.
- P. 7, l. 27: Maybe better "... patterns are less pronounced ...".
- P. 9, l. 33: Maybe better "... to illuminate the ... an to identify ...".
- P. 9, l. 2: Maybe better "... the irrigation forcing ...".
- P. 9, l. 2 - 4 : Here, it is true that the simulations with slab-ocean are energy conserving and thus more physics-based, but at the same time there is additional uncertainty that could lead to simulations with a slab-ocean to be further from reality than those with fixed SSTs. As in the following the authors discuss how the simulations may compare to the real world I think this could also be mentioned at this point.
- P. 13, last sentence section 4.: Again, this is possibly true, but just as likely the differences found in this study are not related to the model physics. This is very hard to tell from comparing individual simulations.
- P. 13, l. 3 - 4 : Is this the surface air temperature? Does this mean include the ocean?
- With respect to figure 2, I just had slight difficulties to clearly see the differences between 0.4 and 0.8 Kelvin that the authors discuss on page 6 line 30 ff. Maybe a

slight alteration of the colorbar (maybe irregular intervals ?) could make it easier to identify these differences.

- With respect to the tables, would it be possible to also include the value of $Delta - Delta$? Maybe the authors could also give an indication of significance for $Delta - A$ and $Delta - O$? I think this would make it even easier for the reader to get a feeling of the importance of $Delta - Delta$ relative to $Delta - A$ and $Delta - O$.