2nd review of Hohenegger's ICON-Sapphire coupled overview paper

As I said in the previous review, I think this is a well written paper on an important topic. I'm very pleased by how the authors responded to the suggestions of both reviewers, which I think makes the paper even better. I'm particularly happy with the injection of comments on my big-picture questions (e.g. how does/doesn't km-scale resolution help, what can we learn from a 1 yr simulation, etc). I suggest the authors are given one more chance to make minor edits in response to the comments below, with the understanding that their next revision will be immediately accepted.

1. Reviewer 1 L304 comment (L324 in new manuscript): Saying you didn't spin up the land because it was unclear how to do so felt a bit naïve to me, but maybe that just reflects different capabilities at different modeling centers. In E3SM we frequently spin up the land model either by running with the land model interacting with an atmosphere that's continually nudged to reanalysis for the period leading up to our target start date or by running the land model in standalone mode driven directly by atmospheric observations. These approaches are explained in the most recent CAPT overview paper: https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2015MS000490 . For SCREAM, we do land-model standalone runs at the full resolution of the model because they're cheap. With nudging, we use full-resolution for the land model but coarser resolution for the atmosphere. Another approach would be just to interpolate land conditions from coarser resolution coupled simulations (though this can lead to issues with land/sea masks). I mention these approaches not necessarily for you to mention in the paper, but in case it helps you come up with a plan for future runs.

   More importantly, I didn't get the sense from Fig R1 that any soil layers in the tropics are equilibrated (except maybe the deepest layer, but that's the one we least expect to equilibrate fast!). Do you really think your "3 of 5 soil layers in the tropics are spun up" statement is accurate? To be clear, I don't think this detail affects the integrity of the paper in any way and I'm just pointing it out to make the paper as perfect as possible.

   In any case, I do think it would be worth articulating more clearly what "land not spun up" means in this case – it could mean that there's no moisture in the soil at all or that soil moisture is initialized to a single value everywhere, or that a spatially-varying climatology is used.

2. Reviewer 1's comment on Fig 5 (Connection between TOA and large scale circulation): while the atmospheric general circulation responds rapidly to a given forcing change, SST changes slowly and the atmospheric general circulation will evolve in response to those slow changes. In that context, TOA energy imbalance will definitely cause sea level pressure and zonal winds to evolve if you ran your simulation longer. I don't think there's anything to change in your paper – adding the individual observed years in the graphic is nice and you've acknowledged now that the short length of your simulations

precludes definitive analysis of ocean-related variables. I just thought that the response to reviewer 1 wasn't correct.

3. L511 in new draft: it felt odd to me that you say there are 2 parallel bands of precipitation in the W Pacific instead of saying you seem to have a double ITCZ. It reminded me of describing a camel as a horse-like animal with 2 bumps on its back – an accurate description but more verbose than needed and harder for people to connect to. It's fine if you have a reason to avoid calling it a double ITCZ, but if not you might as well use standard terminology.

4. Reviewer 1 comment on L623: is the effect of mesoscale eddies on phytoplankton production in your high-res and coarser rest simulations in the same direction as found in Harrison et al 2018? If so, that's stronger evidence for your finding. If not, it suggests the difference may be due to chance.

5. Reviewer 2 comment 20: I agree that excessive precipitation implies too much radiative cooling *within the atmosphere*, but I disagree that excessive atmospheric radiative cooling implies a TOA radiative imbalance where more radiation is leaving the planet. My reasoning is that the heat capacity of the atmosphere is negligible compared to the surface, so TOA imbalances are more likely to reflect a surface that is too emissive or an atmosphere that reflects (rather than emits) too much energy.