REVIEW #2 RESPONSE

We thank Reviewer #2 for their constructive review and for raising some interesting points for us to consider. Here we address each of the reviewer's comments in turn; in the ensuing text the *reviewer comments are provided in italics* with author responses provided in blue.

The authors present a detailed description and limited evaluation of GSI9, an upgrade to their sea ice coupling scheme for UKESM. While their previous GC3.1/CMIP6 setup employed CICE in both the ocean model NEMO and the atmosphere & land model UM-JULES, the new GC5 prototype for CMIP7 use replaces CICE on the ocean side with the NEMO native sea ice model SI3. The conductive coupling approach, which places the atmosphere-sea ice coupling interface between the top and second layer of sea ice rather than between the top layer and the atmosphere, remains unchanged.

The technical description of the work is clear and concise, and the idiosyncrasies of the conductive coupling approach are well-documented in the cited literature. However, I would like to see a paragraph on the motivation for using SI3 for NEMO4 rather than continuing to use CICE.

Technically the main focus of the paper is to provide a detailed description of GSI9, an upgrade to the UK sea ice model configuration. This GSI9 configuration, which is part of the GC5 coupled model configuration, will form the physical basis for the UKESM2 and HadGEM3 contributions to CMIP7. We also provide a thorough documentation of the sea ice coupling within GC5. Although the GSI9 configuration has transitioned from using CICE to SI3, which has required adaptation of the latter to work with the coupling, the fundamentals of the conductivity coupling approach is largely unchanged from the previous model versions (except for the addition of penetrating short-wave radiation described in this paper).

Another technical clarification is that, whilst it is true that the previous configuration used CICE on the ocean side of the coupler and the new configuration uses SI3, it is not true that either of the configurations use CICE on the atmosphere side of the coupler. The UM-JULES code used on the atmosphere side of the coupler includes an adapted version of the CCSM3 4-band albedo scheme (as outlined in Collins et al., 2006) that was previously used in CICE. However, we do not use CICE per se.

We shall modify the introductory text to ensure that these points are made more clearly.

Regarding the motivation for using SI3 with NEMO 4 rather than 'continuing to use CICE' CICE we should make the point that this would have meant staying with the CICE5 codebase, which is not being actively developed anymore. Our conductivity coupling changes were not included in the CICE5 code that was used to initiate CICE6 development, and subsequently changed considerably. This means that we would have to have undertaken considerable development to 'move' to CICE6, along the same lines as moving to SI3 (or move away from the implicit coupling formulation that we use in the MetUM).

The choice to use SI3 was more about strategic and technical efficiencies than scientific. The scientific complexity of the SI3-based sea ice model configuration described in this paper (GSI9) is broadly similar than the CICE-based sea ice model configuration (GSI8) used previously. There are some slight differences (e.g. the lateral melting and salinity evolution are more advanced in GSI9, the sea ice strength was more advanced in GSI8), which are described in this paper already.

Meanwhile SI3 is part of the NEMO ocean modelling framework, which is the UK's ocean modelling framework of choice. As NEMO consortium members we therefore physically own SI3. We are also involved already in the management and technical development of the NEMO code and so there are economies of scale there. From a technical point-of-view, the fact that SI3 runs on the same grid as the NEMO ocean component also provides improvements. The efficiency of the ocean-sea ice coupling is greatly increased rather than having to interpolate velocity fields twice on every model time-step – both before, and after, the sea ice model is called. More importantly there are also scientific differences between the NEMO ocean and CICE sea ice grid can cause problems for the advection of sea ice in narrow channels. In the previous model component this disparity would often lead to the creation of ice pillars in areas of tight bathymetry where sea ice was static but the ocean mobile.

We will try to work some of these points into the paper introduction, noting that not all of the above discussion is relevant for a configuration description or suitable for inclusion in a scientific paper.

<u>References</u>:

Collins, W. D., and Coauthors, 2006: The Community Climate System Model Version 3 (CCSM3). J. Climate, 19, 2122–2143, https://doi.org/10.1175/JCLI3761.1.

Additionally, the manuscript would benefit from detailing why the advantages of using SI3 outweigh any potential advantages of maintaining a consistent sea ice physics formulation with CICE on both sides of the coupler.

As stated above, we do not use CICE on the atmosphere side of the coupler. The radiation and surface exchanges are carried out within the JULES model and not within CICE. The CCSM3 radiation scheme used in JULES is based upon that used within CICE, but that is independent of the choice of sea ice model used and no other aspects of the surface exchange are related to CICE. Therefore, there are no "potential advantages of maintaining a consistent sea ice physics formulation with CICE on both sides of the coupler".

In fact, the opposite is true because with SI3 the sea ice is now on the same grid as the ocean, which has removed one of the inconsistencies that we had in the ocean-sea ice coupling. Furthermore, the conductivity coupling itself already removes the largest inconsistency that exists in standard coupling approach by allowing a consistent calculation of surface exchanges and (atmospheric) boundary-layer evolution across the globe. When using the standard coupling approach, the sea ice-atmosphere interface is actually located above the sea ice and bulk formulae are used to calculate the surface exchanges either side of the coupler (often not using the same bulk formulae).

We will tighten up the text in the introduction and the conductivity coupling description in Section 3 to make the above points clearer.

Although the authors state that a detailed analysis of the resulting sea ice climate is not within the scope of this paper, I encourage them to broaden the scope slightly to include a basic characterization of the sensitivity of the old and new sea ice schemes different climate states. One approach could be to run a short 1850 control simulation followed by a 1% CO2 increase per year, allowing for the computation of transient climate response (TCR) and sea ice response. Another option may be to approximate the CMIP6 HighResMIP protocol (as forcing implementation permits) and show the Arctic Amplification Indices and sea ice response. Other approaches can yield similar information. I consider this relevant as UKESM GC3.1 was an outlier with the highest climate sensitivity CMIP6 dataset.

The reviewer is correct to highlight that climate sensitivity is a pertinent issue for the CMIP climate model contributions. Whilst the GC5 configuration will form the physical basis for the UK's CMIP7 contributions, it will not be used 'out of the box'. Development of the UK CMIP7 climate model configurations from GC5 (inc. UKESM2) is an active area of research – both at the Met Office and within the wider UKESM group. There will likely be several papers on this activity produced in the build-up to our releasing CMIP7 simulations, including an overview of the climate model performance. It would therefore be misleading for us to include details of climate sensitivity or transient climate evolution in this paper.

Finally, it would be beneficial to include a discussion, or provide a reference if analyzed elsewhere, on the phase error in the Arctic summer sea ice area minimum, which occurs in September in observations but shifts to August in both UKESM versions., See Figure 2. The improvements in the Southern Hemisphere, on the other hand, are very encouraging, even if much of it may be the result of ocean modelling improvements.

Regarding the August minimum in Arctic sea ice area: it is worth noting that there are considerable uncertainties associated with the satellite observations in the high summer, particularly August, given that the presence of surface melt water (melt-ponds) increases the relative error of passive microwave observations considerably. The passive microwave retrieval algorithms artificially inflate the sea ice concentrations to compensate for the presence of melt ponds, leading to considerably higher uncertainties in the observational products during the summer months. For these reasons many previous studies have used extent instead of area. However, this is not considered good practice for sea ice studies because extent is a nonlinear, grid-dependent metric – see the arguments presented in SIMIP Community (2020) & Notz (2014).

This difference in phase between the satellite observations and model is not a new thing and is not unique to UK model versions. If we used extent instead of area, as reported for previous configuration, then the modelled seasonal minimum would be in September and consistent with satellite observations. It should also be noted that an August areal minimum is certainly not unique to the UK models. Figure 1b of Roach et al. (2020) (replicated as Figure 1 below) shows that around a quarter of the CMIP6 models considered in that study (at least 10 of 40) have lower sea ice area in August then September. Meanwhile Keen et al. (2021) found the same to be true for around half of the subset of models they considered (7 of 15).

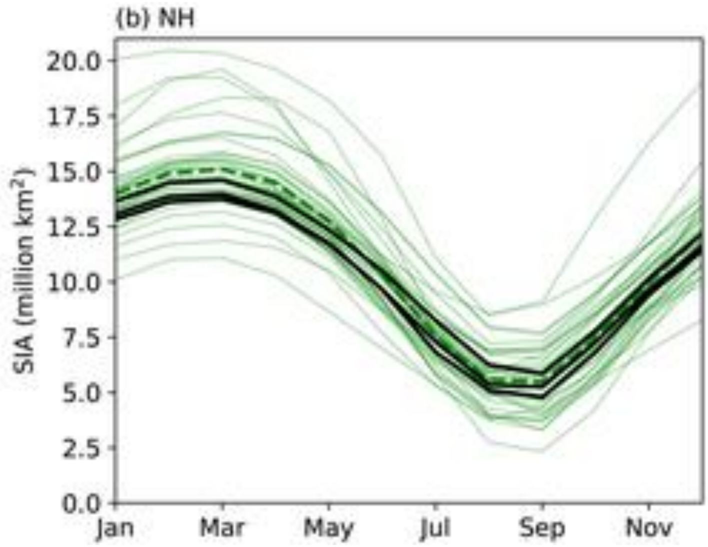

Figure 1: Arctic sea ice area mean seasonal cycles from 40 CMIP6 models (thin green lines) along with multi-model mean (thick green dashed line) and three observational references (thick black lines). Taken from Roach et al. (2020) Figure 1b.

All that said, the reviewer is correct that this is an interesting topic. The fact that the minimum extent occurs in September but minimum area in August suggests a competition between melting of the ice edge at lower latitudes and (re)freezing processes in the ice pack at higher latitudes. After all, changes in area within the central ice pack would not contribute to increased extent if the concentration were already above the 15% extent threshold.

This competition between growth and melt is illustrated in Figure 2 below, which shows the average total sea ice mass flux due to thermodynamic processes for August, September & October. Whilst August is entirely melting (green) and October is dominated by growth (purple), the situation in September is a balance between ice melt at lower latitudes and ice growth at higher latitudes. For the model it seems that the growth processes out-weight the melting to produce a higher area in September than August.

We shall add a paragraph to Section 4 to explain the above.

It would be really interesting to dig into this further and ascertain at what point in September the model changes from net melt to net growth. It might be that the model timing is only out by a few days and that this is biasing the September mean. However, we unfortunately only have monthly output because storing daily fields from these 100-year coupled model runs would be a challenge. This will have to remain something interesting to investigate in future.

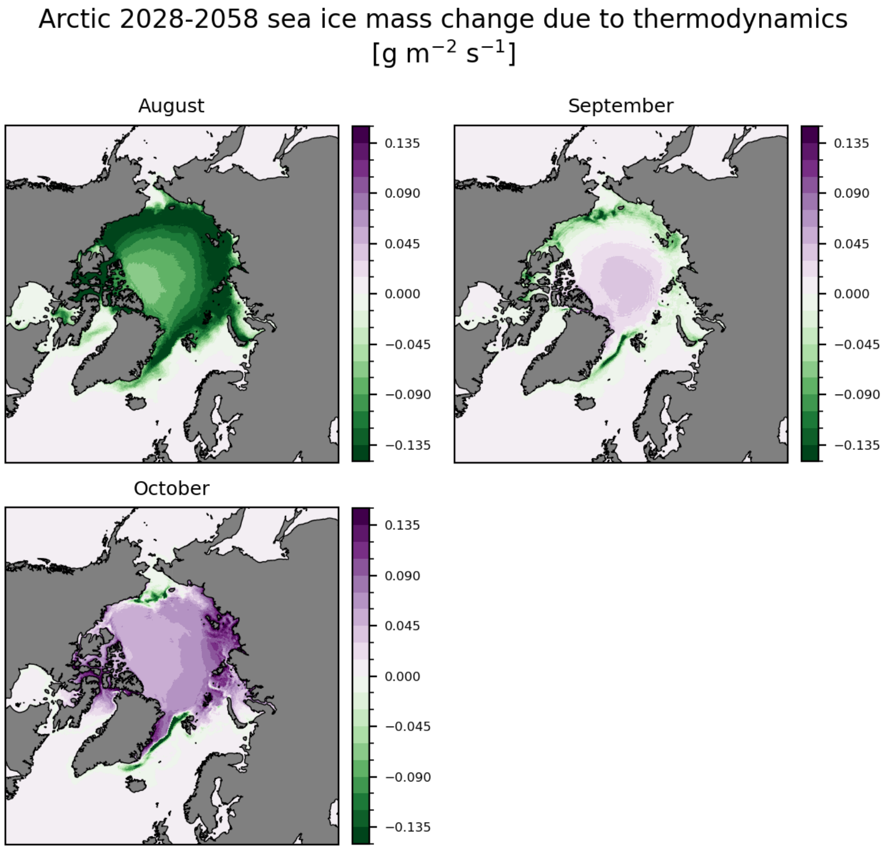

Figure 2: illustration of the competing thermodynamic contributions to Arctic sea ice melt/growth in September. Showing the total sea ice mass change due to thermodynamic processes in Augst, September, and October averaged over the 50-year assessment period used in this paper. Positive fluxes (purple) represent sea ice growth, whilst negative (green) fluxes represent sea ice melt.

References:

Keen, A., Blockley, E., Bailey, D. A., Boldingh Debernard, J., Bushuk, M., Delhaye, S., Docquier, D., Feltham, D., Massonnet, F., O'Farrell, S., Ponsoni, L., Rodriguez, J. M., Schroeder, D., Swart, N., Toyoda, T., Tsujino, H., Vancoppenolle, M., and Wyser, K.: An inter-comparison of the mass budget of the Arctic sea ice in CMIP6 models, The Cryosphere, 15, 951–982, https://doi.org/10.5194/tc-15-951-2021, 2021.

Notz, D.: Sea-ice extent and its trend provide limited metrics of model performance, The Cryosphere, 8, 229–243, https://doi.org/10.5194/tc-8-229-2014, 2014.

Roach, L. A., Dörr, J., Holmes, C. R., Massonnet, F., Blockley, E. W., Notz, D., et al.: Antarctic sea ice area in CMIP6. Geophysical Research Letters, 47, e2019GL086729, https://doi.org/10.1029/2019GL086729, 2020.

SIMIP Community: Arctic sea ice in CMIP6, Geophysical Research Letters, 47, e2019GL086749. https://doi.org/10.1029/2019GL086749, 2020.

Overall, I recommend the paper receive a minor revision before acceptance.

Great! Many thanks for the constructive review and for raising some interesting points.

Minor Comments:

- *L525: Why is second-order accuracy used for only two out of the nine radiative fluxes?*
  We prefer to use second order regridding where possible because it is higher accuracy. However, this is not possible for spatial fields that contain high horizontal gradients or heterogeneity because the second order schemes can cause over-shoots. This could lead to unphysical quantities in the coupling fields such as negative radiation or an ice area fraction outside of the range [0,1].

- *Appendix A, page 24: Snow volume is halved after ridging (as noted in the text), yet the melt pond fraction remains the same (rn_fpndrdg=1). While this may be good for tuning, it seems incorrect from a practical perspective. If the snow falls off, so should the liquid water.*
  This is an interesting point although not one we can address in this paper or as part of this model configuration, which is already frozen. We are currently performing some runs to test the sensitivity of the model configuration to the proportion of melt-ponds retained during ridging to inform the parameter settings used in our next model configuration.

- *Appendix A, page 24: The text mentions increasing the number of layers from 2 to 4, but here it says the layers are reduced from 4 to 2. Please verify this information.*
  On page 24 the namelist table states that we use 4 layers, but that the default for the SI3 model is 2. Therefore, we have increased the number of layers from the default of 2 up to 4, which is consistent with the text, which mentions increasing the number of layers from 2 to 4.