**Review of the Manuscript egusphere-2024-2491**

***"Earth's future and its variability simulated at 9 km global resolution"***

The authors provide a very comprehensive revision of the manuscript and managed to address nearly all my earlier concerns. There is just one major point left, which is still related to the spin-up of the HR model system.

**Major comment:**

The authors state in the conclusions that *'although the same ocean resolution is used, the climate background state is still affected by the atmospheric resolution'.* This is an important point and should be mentioned earlier. To better understand the impact of the atmospheric background climate on the ocean – and thereby the coupled system - it would be good to see time series of the ocean at different model depths. This is important to get a better understanding of how long it takes the coupled model system to arrive at a new equilibrium independent of the atmospheric background state. Here, it would be sufficient to mainly focus on the 1950CTL simulations with MR and HR. How long is the ocean drifting? Do we reach an equilibrium within the 10 years? Unlike the changes OLR changes, I expect this drift to be much longer than just 2 years. This is also important to interpret the warming experiments with a transient SSP forcing, given that the ocean may still respond to the initial shock due to the atmospheric conditions.

**Minor comments:**

Page 3, line 79: 'climate models that run … require'

Page 4, line 92: replace OpenIFS/FESOM2 with AWI-CM3, as it was introduced earlier

Page 4, line 102: is the ocean in equilibrium in the two CTL simulations? See major comment.

Page 5, line 114: 'change in THE future' – in general there are many occasions where articles are missing, this should be checked throughout!

Page 5, line 128: 'via the OASIS3-MCT coupler (Ocean…) and the

Page 5, line 131: 'and up to about 38 km'

Page 5, line 140: this is not only an issue of CMIP models but all low-resolution climate models. Hence, I would remove 'participating in CMIP'.

Page 6, line 146: not a fan of including the acronyms in the figure caption. Why not adding parenthesis saying '(see Section XY for details on the individual model components)', as all acronyms are explained in this section.

Page 6, line 158: why can the stratospheric QBO not considered to be sufficiently well resolved? Please add a small explanation.

Page 7, line 167: MR exhibits stable 14C mean surface temperatures. What about HR? Is this run stable too (see earlier comment on ocean time series, maybe also adding the mean surface temperature to it).

Page 10, line 250: I would not consider more than 1C a ‘slightly’ different global mean temperature, given that a temperature increase of 1C in the future is major concern (in Fig. 4).

Page 24, line 489-492: How different are the precipitation values in the 1950CTL simulations of HR and MR? Are the differences similar to the historical period?

Page 37, line 763: ‘neither related to the resolution of the atmosphere, nor the ocean’ – are there other explanations?

Supplement:

- Again, please check the articles throughout the document.
- Fig. S5, S6, I would reverse the stippling/dashing. To grasp finer details of the significant patterns, I would suggest to stipple insignificant changes (hence, reverse the stippling).