Responses to Reviews of manuscript "A near-global eddy-resolving OGCM for climate studies" (gmd-2016-17) by X. Zhang, P. R. Oke, M. Feng, M. A. Chamberlain, J. A. Church, D. Monselesan, C. Sun, R. J. Matear, A. Schiller and R. Fiedler.
(The reviewer's comments are in back and our responses are in blue)

Short comment #1 by Y. Yu

The manuscript by X. Zhang et al. presented very comprehensive diagnosis from a high resolution OGCM forced by JRA55 reanalysis, which provided very helpful information for many researchers on modeling, climate change etc. Therefore, it is suitable for GMD journal, and I would like to recommend it to publish.

Thank you for your nice comment above, and detailed comments below. We are going to address your comments one by one below.

Here are some detailed comments as follows.

1. The mechanism of 1998-2004 Hiatus remains unclear, the numerical experiments in this manuscript provide an important opportunity to understand the Hiatus. For example, how does the model reproduce the basic characteristics of Hiatus such as temperature anomalies in the surface, subsurface and deep ocean, and is there any relationship between hiatus and AMOC?

It is a very good suggestion. In fact, we are planning a separate scientific paper describing the heat uptake and redistribution, and underlying physical processes based on this historical modelling experiment, in particular related to heat hiatus period.

2. As you mentioned in page 4, bulk formula as suggested by Large and Yeager (2004) are applied to calculate the turbulent heat flux and moment flux at the sea surface? However, I am very curious that you have to apply for a large heat correction more 16W/(m*m). Is it resulted from overestimated downward radiation flux in JRA55?

We chose to modify downward longwave radiation for the ease of implementation. Since bulk formula is used to calculate heat flux, and some air-sea feedback mechanisms can lead to compensation among different components. In fact, in our model experiment, reduced downward long wave radiation is mainly compensated by less heat loss through latent heat flux (see Fig. C below).

3. Because of large heat flux correction, I suggested that the authors had better show comparison of zonal mean shortwave and longwave radiation from JRA-55, OAFLUX or other available observation.

Upon your suggestion, we compared the zonal mean heat flux component from our model experiment with ECMWF Interim Reanalysis and OAFlux, and found the comparison is quite good (See Fig. C below). Nonetheless, the comparison is better with Interim Reanalysis, since OAFlux has a (unrealistically) large positive mean global net flux around 30 W/m^2. The upper panel of Fig. C is now included in the manuscript.

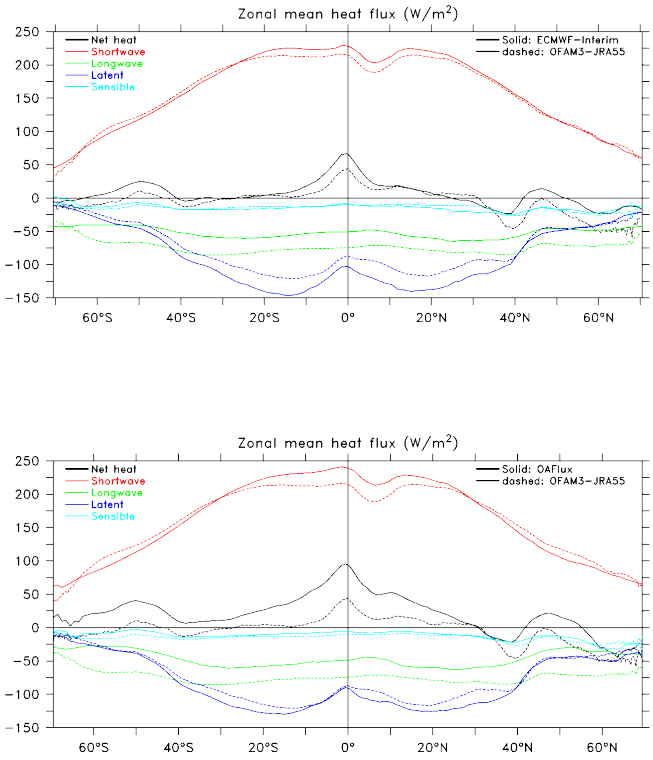

Figure C. Zonal mean heat flux (W/m^2) from our OFAM3-JRA55 model run and ECMWF Interim Reanalysis (upper panel) and OAFlux (lower panel). All flux components and net heat flux are defined positive when the ocean gains heat.

4. How is the temperature change defined in the Figure 4? Is it defined as difference in temperature between the last day and the first day for a given model year?

It's defined as the difference of temperature in the first day between two adjacent years. For example, change over Year 1 is defined as value on the first day of Year 2 minus that on the first day of Year 1, and so on. Information is added.

5. Figure 7, there are significant warm bias at high latitudes in the North Atlantic. Is it due to surface boundary condition, lateral restoring boundary condition or something else?

This warm bias needs further investigation, since it may be caused by several factors. The warm bias off US & Canada east coast may be associated with more poleward location of the Gulf Stream. The warm bias in the Labrador Sea isn't necessarily associated with the northern boundary in our model, since Marzocchi et al. (2015) found similar warm bias in this region in a 1/12^o global OGCM.

6. The caption in Figure 9 is "Mean Eddy Kinetic Energy ...", is this correct?

It's a typo, which should be "Mean Kinetic Energy". Corrected.

7. Figure 10, I suggested that "mean stream function" in the caption should be replaced with "mean barotropical stream function".

We prefer not to modify, since this figure also contains results from the Sverdrup Balance & Island rule, which, strictly speaking, are not barotropical solutions.

8. The authors calculated simulated MKE and EKE using surface currents, but observed MKE and EKE using sea surface height. If both simulated and observed EKE and MKE are estimated from sea surface height, the comparison may be more reasonable.

Observed MKE is based on surface currents from drifter buoys (Lumpkin and Johnson 2013). Only EKE is derived indirectly from altimetry sea level data. We repeated model EKE calculation using sea level rather than surface velocity, and got very similar results in most areas, except some regions (like western boundary currents) where ocean EKE can't be fully derived from sea level.