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Ocean Science Discussions

Interactive comment on "Technical Note: Is radiation important for the high amplitude variability of the MOC in the North Atlantic?" *by* D. Nof and L. Yu

Anonymous Referee #1

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Review of Nof and Lu: Technical Note: Is radiation important for the high amplitude variability of the MOC in the North Atlantic?

Principal Criteria;

Scientific Significance: Poor (4)

Scientific Quality: Poor (4)

Presentation Quality: Poor (4)

General Comments

I fail to see the point of this paper. I also fail to follow the logic of the authors arguments. Because the annual mean sensible and latent heat fluxes at high latitudes are different in the Atlantic (which has an Meridional Overturning Circulation, MOC) from those in the Pacific (which does not), the authors state that these fluxes are important in causing the MOC to shut down (or start up). In contrast, because the net radiation is similar in both oceans , they believe that it plays a “negligible role” in variations of the MOC. While I agree that the larger turbulent heat fluxes in the Atlantic are associated with the transport of warmer water to higher latitudes, this does not prove that they are dominant in forcing any variability in the MOC; they might be or they might not be.

The authors state that this assumed dominance is due to the turbulent fluxes being dependent on the air-sea temperature difference which would vary significantly between on and off states of the MOC due to the atmosphere cooling more rapidly than the ocean due to its lower heat capacity. In contrast they state that radiation is dependent on the ocean surface temperature which would vary less. They therefore appear to suggest that radiation need not be considered in modelling changes in the MOC. They neglect the fact that the air can only cool by radiative processes, primarily long wave radiation from land surfaces. However the authors do acknowledge that the alteration of surface albedo by sea ice (and, presumably ice and snow on land) is important to climate change.

Finally the authors conclude that where turbulent fluxes are dominant, (I quote) “a smaller heat flux implies a much warmer atmosphere and a slightly cooler ocean. This implies that a reduced MOC will warm, not cool Europe.” Even if their previous discussion were correct, I completely fail to see the logic in this argument.

Specific comments

(1) Equation (1) is the upward longwave radiation from the ocean, not as stated the net longwave cooling. Since the downward longwave is dependent on the effective radiative temperature of the atmosphere, a cooler atmosphere would increase the longwave

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cooling. Thus the net radiation does depend on the air-sea temperature difference.

(2) Equation (3) is not a suitable way to calculate latent heat flux over the ocean. It relies on the equilibrium Bowen ratio which is a poorly defined concept (e.g. see Raupach, 2001, Quart. J. Roy. Met. Soc. 127 (574) 1149 - 1182). The latent heat flux shown in Figure 1 was not calculated using equation (3).

(3) The authors state that the “air heat capacity is about 1/4 of the water heat capacity”. That is the ratio of the specific heats. For an order of magnitude calculation of the heat capacity ratio lets say: density of water = 1000 x density of air (at least at the surface); depth of water = 3500m over 70% of earth i.e average depth ~ 2500m; height of atmosphere = 10000m to the tropopause, but lets say 5000m to allow for our over estimate of the mean density. Then heat capacity ratio: 4 x 1000 x (2500/5000) = 2000 i.e O(1000). Before the authors say that that only reinforces their point, they should remember the oft quoted estimate that the top 3m of the ocean is equivalent to the heat capacity of the atmosphere. That heat can be released to warm the atmosphere. Cooling the atmosphere over the ocean is not as easy as the authors imply; you have to have the radiative sink provided by land surfaces to produce cold air.

Conclusion

Because of the faulty logic and incorrect statements I can not recommend the paper for publication.

Interactive comment on Ocean Sci. Discuss., 4, 699, 2007.