Ocean Sci. Discuss., 4, S267–S270, 2007
www.ocean-sci-discuss.net/4/S267/2007/

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

D. Nof and L. Yu

Received and published: 21 September 2007

Response to the 1st reviewer comments

Doron Nof and Lisan Yu - 9/21/2007

For clarity and simplicity, our response in embedded into the review.

I fail to see the point of this paper. I also fail to follow the logic of the authors arguments.

Although this might be our fault (of not clearly explaining the issues at hand), we feel the same way as the reviewer does–we fail to see her/his point and fail to follow his/her logic.

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 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.

We disagree. The fact that the radiation maps for the two oceans are the same strongly suggests that the Atlantic radiation map would have been the same (as it is today) even if the MOC had not been operational. This means that the radiation is unimportant to turning the MOC on and off. Surely, this does not imply that radiation is not important to the overall global heat balance, which it clearly is. Rather, it merely indicates that the radiation does not ***directly*** *affect the MOC. Of course, this is not a “proof“ and this is why this technical note was submitted to a discussion section of a publication rather than to a conventional section of a journal. Also, this is why the title of the note includes a question rather than a statement.*

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.

This statement is taken directly from Sandal and Nof (2007), which provided the foundation for the technical note in question and is referenced as such in the document. We are not sure whether the reviewer has not read it (it was submitted together with the note and is available on line as publication 100 at www.doronnof.net) or decided to ignore it for other reasons.

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

There is no total agreement on the issue of whether radiation is independent of the atmospheric temperature or not. For global climate modelers it is given by equation (3.10) in Rosati and Miyakoda (1988, JPO 1601-1626), which indeed involves the air temperature. However, for the production of climatological maps (more closely related to our interest here), it is given by equation (2) in Bignami et al. (1995, JGR, 2501-2514), which does ***not*** *involve the air temperature.*

(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).

Even though equation (3) is clearly not suitable for climate prediction models, it is perfectly suitable for ***conceptual*** *arguments of the kind made here. Hartmann (1994) presents it in such a conceptual way and it was used extensively in the same sense in Sandal and Nof (2007).*

The latent heat flux shown in Figure 1 was not calculated using equation (3).

True, but what difference does that make?

(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.

We are surprised that the reviewer has made this point. Both C_{pa} *and* C_{pw} *are correctly defined in the text to be the* ***specific*** *heat capacities and the statement of the 1/4 ratio is made in relationship to their ratio. In the other two instances within the text that the 1/4 ratio* *is mentioned, we speak about air/water heat capacity ratio, not atmosphere and ocean heat capacity ratio. While we should have added the word "specific" to be accurate, since we speak about air and water, not atmosphere and ocean, it should have been clear to the reader that we do not speak about the total atmosphere/ocean heat capacity ratio. We were, of course, very well aware of the fact that the ratio of the atmospheric/oceanic heat capacity is $\sim$ O (1/1000), not 1/4.*

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.

So, is the reviewer saying that the MOC does not release heat to the atmosphere or that, when it is completely off, the atmosphere does not cool?

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.

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

OSD

4, S267–S270, 2007

Interactive Comment