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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

D. Nof and L. Yu

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Response to the second reviewer (embedded in text)

General comments

In their technical note "Is radiation important for the high amplitude variability of the MOC in the North Atlantic?" the authors answer this question with "No". Their argument is based on the difference between spatial patterns of radiative fluxes and sensible/latent heat fluxes, in particular on their differences between the Atlantic and the Pacific. I found the logic of the authors' arguments to be flawed many a time and can therefore not recommend the paper for publication in its current form.

Specific comments

1) The authors claim that it is "inevitable" to conclude that radiation is unimportant to the MOC if one accepts that its collapse is responsible for Heinrich events and that present estimates of radiation terms are correct. I do not understand how they come to this conclusion and what the question whether or not an MOC-collapse causes Heinrich events has to do with the impact of radiation on the MOC.

Please note that we speak here about the MOC high amplitude variability, not absolute values (which indeed depend on the global radiation budget). The only high amplitude variability that we know of is that associated with Heinrich events and this is why we deal with it. Of course, Heinrich events do not control the radiation field but one can learn about radiation by examining their behavior.

In writing the technical note, it was assumed that the reader is familiar with the Sandal and Nof (2007) article (pub # 100 at `www.doronnof.net`), which is an involved study that need not be repeated here. No parts of that article were repeated in detail here because it was assumed that editors would (correctly) object to any duplication.

2) The authors claim that the importance of radiation on MOC variability can be estimated simply by comparing average radiation fluxes with average sensible+latent heat fluxes. The argument is apparently that radiation can't be responsible for driving the MOC since net radiation is about the same in the Atlantic and the Pacific, with only the former having a significant MOC. Hence, the MOC must be driven by latent+sensible heat fluxes. However, I fail to see the logic of this argument. Why should, for example, a certain change in radiation pattern not lead to changes in the MOC? And why is it surprising that the sensible heat fluxes in the Atlantic differ from those in the Pacific, given that the SST in the former is at the same latitude usually higher than that in the latter? The question as to what is cause and what is effect in the system remains unanswered in the paper.

As in our response to the first reviewer, the fact that the radiation maps for the

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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 as well as the MOC. Rather, it merely indicates that the radiation does not ***directly*** *affect the MOC* ***variability****. 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.*

3) The authors claim that radiation is independent of air temperature. This is not correct.

Our answer here is the same that we gave the first reviewer. 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.*

4) I fail to understand how the authors come to the conclusion that "a reduced MOC will warm Europe". Given that this is in contrast to everything we know about the impact of the North-Atlantic current on Europe's climate, I'd be very interested in seeing a more conclusive argument of this claim than that given in the paper.

This is discussed in Sandal and Nof (2007).

Given that it is impossible to follow much of the paper's logic, I cannot recommend it for publication in its current form.

If, however, the authors would manage to present a logically more stringent argument

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of their claims I'd be interested to see them, given that much of the paper's conclusions are quite revolutionary.

We are glad that you think so – hopefully, the above response is placing us closer to that goal.

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