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Ocean Science
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5, S94–S100, 2008

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Comment

Interactive comment on "Agulhas ring injection into the South Atlantic during glacials and interglacials" *by* V. Zharkov and D. Nof

V. Zharkov and D. Nof

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Response to first reviewer,

Dear reviewer,

Thanks for your thoughtful comments and for taking so much time to review the document. Please see our response embedded in your review below (in italics).

Review of "Agulhas ring injection into the South Atlantic during glacials and interglacials"

by Zharkov and Nof

In this paper a simple theory is developed to explain reduced Agulhas Ring shedding and reduced inflow of Agulhas leakage into the Atlantic during the glacials and re-

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sumption afterwards. The results (scaling relations) from the semi-analytical model are corroborated by a few experiments with an idealized numerical model. The main result is the (nonlinear) relation between Ring shedding from a retroflecting current and the coastline angle where the WBC retroflects, which also explains the different behavior between the East Australian Current and the Agulhas Current. The authors argue that the retroflection latitude differed between glacials and present day conditions.

The paper is not everywhere well written, there are several spelling errors and grammar errors. Also, the amount of detail in section 4 and especially sec. 3 somewhat hinders the reader to follow the argument. These sections could be written in a more concise manner.

You are right. In the revised version, we attempted to improve the English and eliminate some details is sections 3 and 4.

The main results of the paper (sections 3 and 4), however, are interesting and convincing. I have one major point regarding the motivation of the semi-analytical model. In the introduction arguments are used for the model that are quite unconvincing if not to say purely nonsense. This part should be rewritten. This part, however, is not essential to the paper.

We are not sure that we understand what you mean here. If this concerns your notes about subsections 1.2 and 1.3 then, please, see below.

1.2 Salt balance I suggest to delete this whole subsection.

We are not in agreement here. We view the salt balance issue as critical to the paper so we clarified it and left it as part of the document. Since a reviewer does not have to agree with the authors in order to recommend a paper for publication, we can, hopefully, just agree to disagree on this aspect. See below, please.

The argumentation/motivation for the analytical model is quite unconvincing.

1. The authors say that the volume flux associated with an Agulhas Ring is between

OSD

5, S94–S100, 2008

0.5 and 1.5 Sv. They also say that 4-5 rings a year are shed from the Agulhas Current.

Then they estimate the total flux associated with rings to be 10 Sv = 4.5 times 1??????????

The confusion is partly due to our misprint. We changed the range limits of 0.5 and 1.5 to ***0.4*** *and* ***3.0*** *Sv, respectively, referring to de Ruijter et al. (1999). More importantly, however, the ten Sverdrup flux estimate includes intrusions and filaments associated with the rings but usually not counted as an integral part of the rings themselves.*

2. They estimate the salt contribution to the South Atlantic by rings as 10 SvPSU, taken to be the 10 Sv from above times 1 PSU, being the difference between an Agulhas Ring and AAIW. The argument is that without Agulhas Rings the surface salinity of the South Atlantic would equal that of AAIW. This can't be true. The evaporation - precipitation field in the subtropical Atlantic is quite different from that of the subpolar Southern Ocean.

This calculation is correct as the evaporation minus precipitation fields do not enter the calculation directly. We realize that this aspect is a bit peculiar so one of us (Nof) had an extensive email exchange with Arnold Gordon who concurred with the calculation. This is mentioned in the acknowledgement.

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3. The authors argue that removal of the Agulhas influx would lower the MOC-salinity by 0.7 PSU, being the aforementioned 10SvPSU divided by 15 Sv (total MOC). Apart from the flawed calculation above, *We have cleared that point arguing that the calculation is not flawed.* this argument neglects mixing and surface forcing by E-P.

Again, we are talking about the incoming salinity, which is not affected by the E minus P fields down stream.

Then the authors argue that a freshening of 1.4 PSU of the North Atlantic waters would lead to a collapse of the MOC, because with that salinity cooling to the freezing point

OSD

5, S94–S100, 2008

would no longer produce water that is as dense as the NADW. Then, they use a linear scaling relation to claim that freshening by 0.7 PSU would reduce the MOCby 50%. I think this argument is completely invalid. If this box-model-reasoning would be correct, there wouldn't be any MOC reduction due to freshening until the critical boundary was crossed where surface waters could no longer become as dense as the deep water, and a freshening of 0.7 PSU wouldn't have any impact at all on the MOC.

Probably in reality things are more complicated, but there is no argument whatsoever to assume a linear scaling between MOC-strength and surface salinity in the North Atlantic.

Ok, so you do not agree with box models and linearizations. While we respect your position, box models and linearizations are common ways of estimating things. Furthermore, in this particular case, the calculation is based on Sandal and Nof nonlinear model (2008, JPO, 451-466, see their Fig. 7). We think that, again, we just need to agree to disagree.

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4. In my opinion the collapse of the MOC after cessation of Agulhas leakage has nothing to do with the salt balance in the North Atlantic. The reason is, that Agulhas leakage is probably the only (major) pathway for upwelled NADW to return to the Atlantic. When this pathway is blocked the MOC has to collapse because NADW export can no longer be supplied by a return flow. Ten years ago it was believed that for the return flow there was a competition between Agulhas leakage and a "cold water path" of direct flow from Drake Passage into the South Atlantic. Such a pathway was also present in coarse resolution ocean models. In higher resolution models, however, the ACC speeds are much larger (as is the case for the real ocean) and the ratio of the northward Ekman flow to the eastward advection by the ACC decreases. As a result, NADW that upwells in the Southern ocean is advected further eastward before it reaches the latitude of 35S, which can only occur when this water has entered the Indian and Pacific Ocean.

For this water to return to the Atlantic it has to go via Agulhas leakage. So, if Agulhas leakage ceases to exist there is no longer a pathway for upwelled NADW to re-enter the Atlantic.

We are not in agreement here either. You seem to take the high-resolution models results as if they were absolute truth corresponding to observational data. They are not. These models have their own problems and need to be regarded merely as "other" models results, not data.

. . . .

1.3 The Glacial-interglacial hypothesis.

The authors argue that the retroflection latitude is the latitude where the windstress curl, WSC vanishes, which is true when linear theory applies. They also notice that in some cases the continent terminates equatorward of the latitude where the WSC vanishes. In that case the retroflection latitude is more tied to the latitude where the continent terminates. For instance the WSC vanishes at 45S in the southwest IndianOcean, while the Agulhas Current retroflects at 38S. For this reason, the assumption that the retroflection latitude equally shifts with the zero WSC line is unlikely. One could also argue that the retroflection latitude does not shift at all, as long as the zero WSC line is still poleward of the retroflection latitude.

Ok, but the point is that, during glaciation, the WSC vanished north of the continental termination. Also, consider the EAC circulation [Fig. 1 of Bostock et al. (2006)]. The zero WSC is nearly at the latitude of the westerlies maximal intensity, which is about 47^{0}S. During glacials, the zero WSC was north of 34^{0}S, which is the latitude of the modern Tasman Front.

The authors also cite a possible WSC shift of 25 degrees (sic), supported by a proxy analysis in the Pacific that suggests a shift from 33S to 25S there, during the last glacial. I believe they mix up numbers here.

OSD

5, S94–S100, 2008

Interactive Comment

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Interactive Discussion

Discussion Paper

We might have mixed up some words but certainly not numbers. We corrected this by substituting the erroneously written "as much as 25^0" by "up to at least 25^0S", as written by Esper et al. (2004).

Instead I suggest that the authors refrain from quantitative estimates of the WSC and retroflection latitude shifts between glacials and present day conditions. I think it is sufficient to remark that there is evidence for a significant equatorward shift of the WSC during glacials and that this shift was likely associated with an equatorward shift of the retroflection. How much that shift exactly was is uncertain, and therefore it makes sense to perform a sensitivity study to the latitude of retroflection, i.p. the coastline slant associated with that shift, which is the relevant parameter here.

We think that we need to present some quantitative numbers so that the reader gets a sense of what we are speaking about. Of course, one can never be sure about how relevant the results of any model (analytical or high resolution numerical model) are. We tried to make that clear in the revised version.

Section 3,4 These sections could be rewritten in a more concise way.

As mentioned above, you are right.

3.2 Couldn't the authors check the mass flux that results from their analysis by substituting present day numbers for alpha, beta, gamma etc in Eq, (1) and compare the mass flux to present day estimates?

This is a good idea. We did that on page 14.

Section 5. The authors relate reduced Agulhas leakage to reduced Ring shedding when the Agulhas retroflection moves northward and the coastline slant increases. This is a nice result, but equally important is probably the following effect: Poleward of the zero WSC line wind driven flow is eastward. In the present situation Agulhas Rings are shed equatorward from the zero WSC line, so they can easily drift westward into the Atlantic basin. When the retroflection latitude moves equatorward the coastline

OSD

5, S94–S100, 2008

Interactive Comment

slant forces westward propagating rings also to move poleward and they may cross the zero WSC line. The authors completely neglect advection by a background flow, but I surmise that eastward advection by wind driven currents will be as effective in hindering the propagation of Agulhas rings into the Atlantic, as the effect of rings hindering each other and the increased chance of rings being captured by other rings or meanders because their generation period increases with increasing coastline slant.

In our opinion, this is not quite correct. Your argument is "...When the retroflection latitude moves equatorward the coastline slant forces westward propagating rings also to move poleward and they may cross the zero WSC line".

*This can be correct only for rings that have already been **shed**. However, for the slant in question most detached rings are almost immediately re-captured by the retroflected current. In that sense, only very small part of eddies can be advected by a background flow near the line of zero WSC.*

Spelling errors: Wejer iso Weijer; Burne iso Byrne, etc.

You are right! We corrected these errors.

The authors refer to the thesis of van Veldhoven (2005) which is "grey" literature. The main part of this thesis has been published as van Aken et al. (2003) DSR 50,II, 167-195. The authors should refer to this paper instead of van Veldhoven (2005).

We corrected that.

I am happy to accept the paper when the authors elaborate on the points raised above.

Hopefully, the above is satisfactory to you.

Interactive comment on Ocean Sci. Discuss., 5, 39, 2008.