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Climate of the Past Discussions

Interactive comment on "High-latitude obliquity forcing drives the agulhas leakage" *by* T. Caley et al.

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fyi - My expertise as reviewer is in physical oceanography and not paleoceanography, therefore many of my comments relate to issues of hydrography and dynamics and some comments relating to clarity may result from lack of expertise. However, clarity enables communication to a wider audience!

Title should have Agulhas with capital A

Overall

This paper would benefit from a much stronger introduction and thread. The explanations are muddy and studies on different time scales from different parts of the ocean and of different quantities (including models) are compared loosely, leading to confusion. The discussion is like a random set of statements which don't compare like to like

or have a clear thread. I think the main conclusions are:

(1) Agulhas variability linked to high latitude southern hemisphere forcing. (2) AC was stronger when leakage was weaker. (3) Obliquity signal transmitted from SH to NH via Agulhas leakage-induced AMOC changes.

None of these ideas is new and how they hang together and compare with other studies is not well explained in this paper.

I feel like I am missing a beat, and perhaps it is down to ignorance, but the case for isolating obliquity as an important (the most important?) forcing for the Agulhas is not made clearly. The statement that changes in the Agulhas system are strongly related to mid to high-latitude changes is not novel. Is it novel (and useful) to link this with obliquity in isolation? Since the 100 kYr time scales appear to be dominant in the Agulhas system (eg Peeters et al, 2004), are the authors invoking Huybers and Wunsch (2005) conclusion that these time scales can be explained by obliquity forcing alone plus some stochastic variability? In that case, what is to be learned by this for the case of the Agulhas? Better prediction?

There is a lot of discussion around other studies that discuss glacial-interglacial changes on time scales of 100 Kyrs in leakage and winds and STF (eg Peeters et al, 2004), but it is not clear how these should be related to the authors' discussion about obliquity forcing? And to the authors claim that high-latitude obliquity forcing controls leakage (as stated in their title). It seems that in effect, the authors are merely stating that their data supports the theory of Huybers and Wunsch (2005). Control of the Agulhas system by high latitude forcing is not a new concept. Linking the 100 kyr cycle to obliquity control is not a new concept. I suppose merging these two to link the Agulhas to obliquity is novel, but is it useful? This could be repeated for other climate indicators, but to what end?

The authors also state that the Agulhas was stronger when leakage was weaker - but related to what time scale of climate change? Related to obliquity forcing? And glacial-

interglacial changes? Is there global warming or global cooling, changes in ice, or wind during this climate change? Any of these could complicate a simple dependency between Agulhas strength and leakage on different time scales. Given the current dynamical understanding (eg Beal et al., Box, 2011) at least the expected shift in winds should be discussed along with Agulhas and leakage strength. And does it follow that higher SSS and SST means stronger Agulhas? Global climate change is currently increasing SSS and SST in the oceans down to some 800 m depth in the Indian Ocean (Alory et al, 2007), but a hindcast model resolving the Agulhas (Biastoch et al, 2009) shows no change in its strength - although leakage does decrease. In that case I would argue that SSS and SST signals alone do not necessarily correlate to a change in Agulhas strength, but instead could reflect a shift in the gyre (e.g. Alory et al, 2007).

Abstract Line 1: This first sentence is too strong. Evidence that Agulhas effects AMOC is limited to model studies, and therefore not proven. Moreover, decadal variability is only 1-2 Sv, resulting from planetary wave perturbations and arguably not worth basing the papers first sentence around. More apropos is the advective link between Agulhas leakage and AMOC, which has timescales of $\sim$40-100s years.

Line 5: I don't recall seeing a paleo paper that argues for a "decisive role" for Agulhas on AMOC? In fact, the link to AMOC is not well established because the paleo proxies for overturning are open to question.

Line 7: Forcing mechanisms can be (and have been) elucidated through modeling and theoretical work - e.g. de Ruijter et al (1999), Weijer et al (2002), Toggweiler et al. (2006), Sijp & England (2009). Probably the leading idea for a forcing mechanism is wind shift related to glacial-interglacial cycles, combined with thermohaline forcing of Leakage on AMOC. The paleo data presented here do not elucidate the forcing connecting the Agulhas with AMOC, but rather elucidate the forcing mechanism (obliquity) on the STF and Agulhas leakage. This sentence should be rewritten.

Line 14: Agulhas Leakage occurs between about 35 and 43 S (e.g. Beal et al. (2011),

Fig 3), which is not a "low-latitude location".

Line 15: Authors state "Together, this suggests that long-term Agulhas leakage dynamics are associated with a high latitude rather than a tropical climate forcing mechanism, probably by varying the position of the Southern Hemisphere subtropical convergence (STC) and its associated westerlies." This is not a novel suggestion, e.g. Beal et al. (2011), Bard & Rickaby (2009), Biastoch et al (2009), de Ruijter et al (1999). In fact, I have not seen a paper which argues that Leakage dynamics are associated with tropical climate forcing, except for a minor note by Peeters et al (2004) that on some timescales the Indian Ocean monsoon could have an influence. The subtropical and mid-latitude wind field has long-been established as important by de Ruijter (1982).

Line 18: Authors state "We argue that during terminations stronger Agulhas leakage was triggered by increased obliquity exerting a positive feedback on the global climate system through modulating long-term AMOC variations." - In this case, why did increased obliquity when not coincident with terminations, not cause a significant peak in Agulhas Leakage - e.g. Peeters et al. (2004)?

Introduction Line 22: This sentence is misleading. The Agulhas Current is not the single "control" on inter-ocean exchange, unless all other forcings are constant.

Line 27: Weijer et al (2002) consider buoyancy forcing of Agulhas Leakage on AMOC, as authors state, however Biastoch et al (2008) consider planetary wave perturbations instigated by Agulhas Rings and their effect on AMOC. These two forcing mechanisms have very different time scales and magnitudes of effect on AMOC.

Fig 1: These maps should cover a reduced region, or include an inset of the core site, because it is difficult to tell where the paleo data are located in relation to bathymetry, currents, rivers etc. Also, if the authors limit the region of these maps more, the range of temperatures and salinities could be reduced and enable more detail to be seen in the region of interest - for instance the Agulhas Current and its Retroflection would be nicely illustrated by these water properties.

Fig 1: I think reference to "subtropical convergence" is dated - I can only find it in The Oceans (1942), or in reference to "subtropical convergence zones" which are found in the middle of subtropical gyres. This oceanic feature should be referred to as "subtropical front", see e.g. Deacon (1982), Orsi et al. (1995), Beal et al (2011).

Fig 1: MD96-2048, 26âŮę10âĂš482 S, 34âŮę01âĂš148 E, 660 m water depth. Looking at an ocean bathymetric map and Lutjeharms and da Silva (1987) (see fig 1 attached), this core seems to be in a "backwater" region of the Agulhas system, known as the Delagoa Bight. (The continental slope, say 1000 m contour, is not shown but it would be helpful if it was, given its importance in guiding the pathway of the Agulhas Current and Retroflection.) The authors should establish that this is indeed a "precursor" region for the Agulhas Current using hydrographic data and/or a model. The Delagoa Bight is known for the spin-up of cyclonic eddies and waters inshore of 35 E have been seen to be influenced by cooler, upwelled waters flowing northward - see fig below. How could this effect the authors' results? Could the Limpopo river also have an effect? Some discussion is needed. Does this core reflect changes in coastal waters, in the Agulhas, or a shift in the whole gyre (e.g. as Bard and Rickaby proposed)? (Fact that variability doesn't follow precession, as discussed later, is an indicator that core reflects non-local changes?)

Fig 2: Labelling for E and F mixed up.

Discussion p2200, Lines 1-end of paragraph: This paragraph is well written and goes a long way to explaining the authors' thinking as regards the link between obliquity and 100 Kyr cycles. However, this link needs more emphasis and discussion since it is central to the paper. It should also be introduced far earlier in the paper to help the reader. Are the authors essentially saying that their data support the hypothesis of Huybers and Wunsch (2005) and that obliquity alone seems to force a 100 Kyr cycle in the Agulhas system? This message should be earlier and clearer.

p 2201, Line 11: On first reading this sentence seems to suggest that Bard and Rick-

aby (2009) and Biastoch et al (2009) link obliquity to STF and Westerlies position. In fact, Bard and Rickaby (2009) refer to glacial-interglacial changes on $\sim$100Kyr time scale and Biastoch et al. (2009) refer to shift in Westerlies related to contemporary climate change (warming plus ozone effects). Sentence needs rewriting. And again, is it useful/insightful to go down the list and link obliquity with shift in STF, shift in westerlies, heat export, sea ice etc - all climate indicators which have been linked with glacial terminations?

p2201, Line 15: "Poleward shifts of the STC modify recirculation in the Indian subtropical gyre (Bard and Rickaby, 2009), which intensifies heat and salt transfer from the Indian Ocean to the South Atlantic, thus increasing SSTs and SSSs of the AC." This sentence doesn't make sense. It does not follow that from more leakage of warm salty water OUT of the Indian Ocean, the Indian Ocean and Agulhas will be warmer and saltier. However sitting in one spot, as the paleo data are, a southward shift in the IO subtropical gyre and STF (e.g. related to current climate change, Alory et al, 2007) would lead to a local increase in T/S. Furthermore, this sentence contradicts these sentences on p2202, line 22... "Increased glacial SSTs were recorded at site MD96-2048 when the STC reached its northern most position, which may be related to a build-up of heat from the return flow that could not escape to the Atlantic as for MIS 12. Alternatively, lateral fluxes and thus the AC were stronger when Agulhas leakage was weaker

p2202, line 13: Some physical and theoretical citations should be used here. There is a strong dynamical basis for linking the position of the STF with the strength of Agulhas leakage - eg de Ruijter et al (1999), Beal et al (2011). Paleo data cannot tell you about dynamics.

p2202, line 25: Again this discussion would benefit from inclusion of dynamical theory (eg Beal et al, 2011, Box), which would clear up "contradictions" by explaining the TWO main mechanisms controlling leakage. Van Sebille et al (2009) cannot be invoked unless all other forcing in the system are unchanging - ie a weaker Agulhas = stronger

leakage isn't the case when insolation and winds change! In other words, it only takes into account the inertial control on the Agulhas and not the movement of the STF. The strength of the Agulhas is predominantly tied to subtropical wind stress curl - ie related to interplay of trades and westerlies. In turn, leakage is tied to strength of Agulhas (its inertia) AND position of westerlies (Beal et al, 2011).

p2202, line 26: Does it follow that the Agulhas is stronger if SSS and SST are higher? From previous argument I would say maybe not. If leakage is reduced, warm and salty water can build up in Indian Ocean, independent of the strength of the Agulhas. In Biastoch et al's (2009) 1950-2000 hindcast, the Agulhas strength does not change while leakage does.

Fig 4: There are severalcitations of Peeters data without reference to fig 4. In fact, figure 4 is useful and interesting, comparing two other cores with the authors' data, but doesn't seem to be well referenced or explained in the text?

p2203, line 10: : "This suggests that the enhanced leakage of warmer and saltier Indian Ocean waters into the South Atlantic during the terminations allowed for the development of a South-North density gradient in the Atlantic before the global ice volume change, reinforcing the AMOC (Weijer et al., 2002; Biastoch et al., 2008)." This sentence needs rewriting in plainer english. AMOC is related to north-south pressure gradient (not density, see de Boer et al (200?)) but why even introduce this concept here? It leads to confusion.

p2203, line 22: "Our results from the Agulhas system provide an important metric for the AMOC response to orbital-obliquity forcing that contributed to global climate changes as a positive feedback." I feel that there is a really important message here, but I don't understand the sentence.

p2204, line 4: "Our finding of obliquity-driven Agulhas leakages sheds light on a new feedback mechanism for long-term AMOC responses to the inter-ocean heat and salt exchange." What is the feedback mechanism?

p2204, line 15: "To trigger ice age terminations, important feedbacks need to be added to the direct effect of insolation changes on ice sheets. We argue that the important transfer of heat and salt via the AC, which affected the resumption of the AMOC and the initiation of interglacial conditions (Weijer et al., 2002; Knorr and Lohmann, 2003; Biastock et al., 2008), is one of the main feedbacks." This is a great sentence, but this hypothesis should be clearly introduced and discussed long before, so that the reader can be lead through the data and argumentation that leads the authors to this conclusion. Currently there is no thread making the paper confused and unclear.

p2204, line 19: The model of Marsh does not resolve the Agulhas either. This citation is superfluous unless there is something more specific to be said.

p2204, line 23: "Therefore, obliquity-induced variability of the Agulhas leakage merits greater attention in global ocean and climate models used for predicting the future climate scenarios." How does it help to distinguish obliquity forced changes in the Agulhas from other changes? Is it important in terms of predicting changes to the Agulhas in a changing climate? What kind of attention does it merit? Can leakage be resolved in paleoclimate simulations?

Interactive comment on Clim. Past Discuss., 7, 2193, 2011.

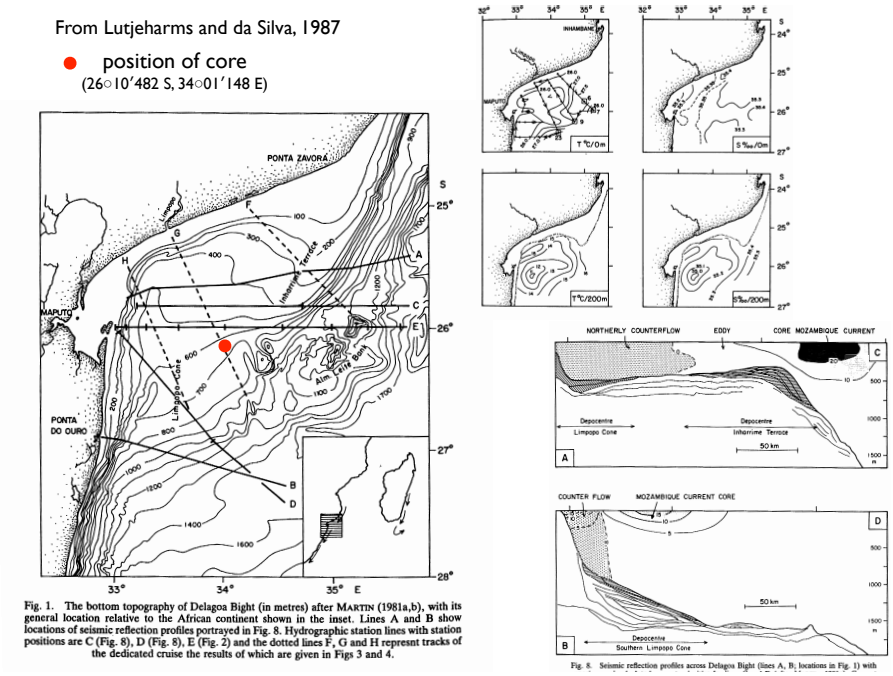

Fig. 1. core position on detailed map