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Discussions

Interactive comment on "Reconciling the north–south density difference scaling for the Meridional Overturning Circulation strength with geostrophy" *by* A. A. Cimatoribus et al.

Anonymous Referee #2

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It has been shown before that the AMOC often correlates to the N-S density difference across the Atlantic. In this paper Climatoribus et al. present a hypothesis for why the N-S density gradient across the Atlantic basin relates to the E-W density gradient which is driving a northward overturning through geostrophy. They thus provide a hypothesis for the validity of an assumption that is often stated without much justification. In that sense the paper is potentially useful. However, the points in the papers are not fully justified and I have some reservations that I would like the authors to address before I can recommend publication. The question of how pressure gradients drive the AMOC is very interesting and I hope the authors can address these concerns and elaborate on some points. Otherwise the paper is well written both structurally and grammatically.

As pointed out, numerous authors have found the maximum Atlantic meridional overturning streamfunction (AMOC, or max psi) to scale with the N-S density/buoyancy difference, b_ns, across the full basin (or the northern hemisphere). Usually it is not so sensitive to where the densities are evaluated and not in this paper either. However, it has been very difficult so far to find the same consistent scaling for the AMOC to the E-W density difference, b_ew. This is probably because the density structures on the eastern and western boundary are much more complex so that the thermal wind relation does not have simple scaling values that describe the motion. However, in this paper a latitude and depth is found where the scaling relationship does seem to hold. This could be interesting, but I have some reservation about whether the correlation between this specific b_ew and the AMOC is really a manifestation of the thermal wind relation rather than just so by experiment design. Specifically, in this model as in studies before, psi $\sim$ b_ns*hˆ2. The b_ew here is designed to be about the same as b_ns so it is not at all surprising that psi $\sim$ b_ew*hˆ2. As far as I can gather, b_e = b_s, and not just b_e $\sim$ b_s. That is, the eastern value is evaluated at the deepest place where b=b_s on the eastern boundary. And b_w is in the middle of the southern flowing WBC that brings water from the region where b_n is evaluated. I cannot gather from the paper whether it is argued that b_ns and b_ew are supposed to be the same or not. Anyway, it seems they are designed to be so. So the question then is whether this scaling of b_ew with psi is due to thermal wind and that is why b_ns scales with psi (in which case, this paper makes an important contribution) or whether b_ns scales with psi for other reasons and because b_ns $\sim$ b_ew it looks as if psi is in fact simply presented by the thermal wind scaling at latitude theta_b. I don't think the former case has been justified adequately yet for a couple of reasons.

The first test of whether psi $\sim$ b_ew*hˆ2 follows from thermal wind is to evaluate the theory. If one would guess an appropriate b_ew on the basis of a scaling analysis, would one really choose b_ew at the bottom of the pycnocline? Most of the upper layer northward flow (in this model at least) is in the upper part of the pycnocline. Moreover, since the geostrophic flow is in fact pressure driven, b_ew in the upper part of the pycnocline would affect the whole layer while b_ew in the lower part would only affect the flow from there and below. So if it was really thermal wind that was causing the correlation, shouldn't the correlation be stronger if b_ew was evaluated at a more shallow depth?

Fig 5a,b is interpreted to indicate a strong correlation between max psi and b_ew and b_ns respectively. However, it is in fact only showing a correlation with b_ew*hˆ2 and b_ns*hˆ2. There is no mention of the variability of h between these runs. It is stated that h correlated well with depth of max psi, but nowhere do we know how these two values are changing in the runs. That means you can only talk about pressure gradients here and not density gradients. To be able to change the conclusions to density gradients, it is important to show that both h and depth of max psi do not change. And even then, we know they are likely to change under some circumstances (take for instance the glacial circulation). It is difficult to imagine they would not change when changing the diffusivity as much as here. How does the correlation look when keeping h constant? And do you also get the correlation with b_ew and b_ns constant but varying h?

Also, it seems to me the correlations in Fig a,b are only true in the low diffusivity cases. Taking those off the plot would lead to no or even a negative correlation (despite it stated in the paper that it also holds for high diffusivity). Why would that be? Any ideas? It may be due to incorrect h being used or perhaps b_ew not really representing thermal wind in the whole upper layer of the AMOC?

Then, since the thermal wind scaling is shown to hold at a specific latitude, theta_b, where b_e and h are evaluated, it is important to define this properly. It seems to be foremost defined to be the latitude where b_s reaches a max depth on the eastern boundary. It is then loosely associated with a few other properties but these connections have not been justified. For example, in caption Fig 2, it is said to be where psi is a max, it is also said to be at the Subtropical and Subpolar gyre boundary, and indirectly this latitude is assumed to be where the pycnocline outcrops in the west (since that is the schematic of Fig 1 which Fig 4 is said to resemble). Which of these are true

in a) theory, b) the control run, c) all runs found here, and d) expected to be true in the real world? In other words, how universally applicable are these assumptions? In this paper it is for instance shown that this latitude is not where psi is a max (and why should it be).

It would be good so see a figure of the upper layer geostrophic flow or the barotropic streamfunction to see the horizontal flow structure. The velocity structure here is rather odd or overly smooth to my knowledge. Usually at these high latitudes the flow is more zonallt variable with a stronger barotropic component. I suspect the simpler structure is because of a flat bottom, smooth wind profile and relatively low resolution. I assume that this latitude is not in Sverdrup balance as since it is quite a bit north of where Sverdrup balance breaks down usually in the ocean? In fact, the windstress curl is about 0 and yet there is substantial northward flow. Usually the flow at these latitudes is then driven by bottom pressure torque on large scales. What is driving/balancing the flow here in a vorticity sense? Is this northward flow actually a northeastward current like the North Atlantic current? It's important to understand why you get this flow structure if one needs this kind of pycnocline and velocity structure at the latitude where the AMOC is evaluated. In short, is this simple velocity and pycnocline structure at this latitude really representative of the real ocean or found in more realistic models? Perhaps all this Sverdrup business is not so relevant to the conclusion of the paper and more for my own interest, but I would nevertheless at least discuss the horizontal flow structure if this latitude theta_b is being associated with the boundary of the gyres.

Specific points:

There is some confusion in the paper about what is psi. It is defined as the max overturning streamfunction in the beginning but later is also referred to as the maximum at 52N which is not the maximum at the basin. Also, psi usually refers to the streamfunction itself. I would suggest keeping psi to mean the streamfunction and give it an appropriate subscripts when referring to its maximum value basin wide or at a latitude.

Please drop the Ne and Se abbreviations and just write out northern and southern ends. The abbreviations are confusing and not necessary. (They are too similar to NE and SE and could also be confused as pointing to a specific place such as the northern end of the ACC).

Page 2464, line12. When saying "both are measured at the latitude of psi" do you mean that is where they are measure here in this paper ? Or do you mean this is the most valid latitude for the scaling. You may mean here the max psi in the basin (as it is defined) or just that the b_ew * hˆ2 should give the max psi at the latitude where they are evaluated? Proabably the later but please make sure this is clear.

Page 2469, line 2-9: All this assumes that the scale depth does not change in these simulations. And if one would like to generalize as is done here, one indirectly makes the assumption that the scale depth does not change here or with other types of perturbations of the AMOC. But is this valid? For instance, what would happen in a model with a realistic AMOC depth or in reality if the AABW volume would increase by a huge amount? The AMOC could be forced to shoal and the change in scale depth might change the AMOC irrespective of density gradients. So you need to be more careful about what conclusions can and cannot be drawn from this analysis about what the correlations say about density specifically.

In Fig 2 it is argued that the latitude of max b_s is also the latitude where the AMOC reaches its maximum. Why should that be? In fact, we know from Fig 4 that this is not the case.

It would be good to get Fig 4 a bit bigger.

Figure 5: This figure or at least the texts in the dots need to be bigger. They are very difficult to read.

Interactive comment on Ocean Sci. Discuss., 10, 2461, 2013.