Ocean Sci. Discuss., 8, C391–C394, 2011
www.ocean-sci-discuss.net/8/C391/2011/

Ocean Science Discussions

Interactive comment on "A multi-decadal meridional displacement of the Subpolar Front in the Newfoundland Basin" *by* I. Núñez-Riboni et al.

Anonymous Referee #2

Received and published: 11 July 2011

The main idea of the paper, as I see it, is to compare observations with a model, and offer some explanations to observed variability. This is an interesting study, but I think simple correlations are not sufficient for linking two processes, the chains of causes and consequences are too long and each link is essentially non-linear, so I would suggest a more careful rethinking of the process and mechanisms suggested in the paper. Like the volume of LSW is not an easy measure – LSW is not a body proportionally expanding and shrinking in time, so its volume can't be measured by a position of the SBF, and even more it can be much deeper than a type of processes discussed here.

More specifically, but not in order of appearance in the manuscript:

I heard about "inter-gyre gyre", but don't know of any eddy-like oceanographically

sound or quasi-permanent feature in the Newfoundland Basin other then Mann Eddy, a smaller eddy E-SE of Flemish Cap and the North-west corner or a distinct anticyclonic loop of the North- Atlantic Current. Is IGG the same thing as the Mann Eddy or all three features I mention here combined together? I suggest to authors to relate their characteristic to more established or classical definitions, crediting the real features and not their model imitation.

I don't thing the separation of wind and water-mass production is valid. Wind in fact is a strong player in the latter, because it establishes gyre circulation important for water retention and together with SST-TA determines the heat loss from the sea surface controlling production of LSW and other intermediate water masses.

I disagree that the seasonal processes are small at 500 m. In fact in and near the convection and boundary regions, the winter pulses of water mass renewal create strong signals seen as deep as 500 m (line 14), at least, and 1000 m or even 1500 m in some years. So, any work like that would require a proper examination of seasonal variability if data coverage permits. This brings the next question.

How good is the data coverage for the annual resolution? How strong is a bias of shifting distribution of available observations over the decades? Could any of the observed changes be due to shifts from the IIP data in earlier years to Soviet Section to Argo? Consider that the box used in the analysis is large and any shifts in data coverage between years and decades would play there. Each value should have some indicator of its error due to irregularity and sparseness in sampling. Adding here the question to seasonality (you would definitely see it in Argo if not in other data), I would question the quality of the observed metrics presented in the work – I wouldn't if had this paper for review two decades ago, but we know better by now about the data-related problems I bring up here.

Potential density anomaly of 27.6 is too low for LSW, and it is not as trivial for defining if there is more than one vintage present at the same time. So, the approach to identification of LSW may also be improved.

I wonder what was the effect of salinity drift in the model used in the current study on the ocean stratification, mixing and the important characteristics presented here?

Figure 1. Considering amount of smoothing applied to the data, and that not the actual value, but its normalized form used, I don't think that the agreement is as striking as the authors conclude. One could notice agreement in parts of the simulated and observed records, while other parts don't show any. What the author might show was a simulate vs observed plots showing lines and indicating respective years or decades. Separating the data and model lines by an offset would also show spots of agreement/disagreement a bit better. For example, if you remove the last five years from SPF plot, would you see agreement in the lines? Or take just the second half of salinity data. The arrival of low salinity waters of the 1980s was totally missed by the model.

Page 464, lines 3-4. As I already mentioned the two processes are not independent – you can't say WSC has no affect on atmospheric circulation or other way around and heat flux and therefore production of LSW.

Page 464, lines 10-13. It is very strange to hear that if it is not due to WSC, it mast be a result of hydrographic changes, mainly LSW, and therefore everything at 500 m not explained by WSC is attributed to LSW. I have a different opinion and tend to think that some other no less important processes are left out of picture.

Pages 464-465. The statement between the two pages is very speculative, and I would add very much counterintuitive – there was a record high production of LSW in the late 1980s – through mid-1990, so how can one be sure that WSC played a greater role on the southward displacement in the 1990s? I am not sure if SPF is really affected by LSW – LSW is generally deeper than SPF, but is there was such a connection between the two the whole period of the 1990s would be ideal place to look for one.

Section 5.2. I totally disagree with the logic of measuring LSW volume changes with

SPF position – the processes may be connected to a common large scale pattern, and therefore correlated, but I wouldn't be using one as an indicator of the other – this would mislead the readers. Disagree with the last statement. Fig. 3d is probably similar to NAO (even that might be questioned), but don't see much of LSW.

Section 5.3. Starting statement – I though LSW volume changed not like that (negative before 1976 and positive after 1976), there were strong changes inside decades and between the decades.

Figure 8. I don't think it agrees with observations. First the magnitude of LSW thickness anomaly – from 1966 or 1971 to 1996 it must be more than 100 m of total change in theLabrador Sea and near. Then the strongest convection was observed in the early 1990s leaving a very thick LSW to mid-1990s and first years after. So, why 1996 is so much smaller than 1986 and even 1981 when not much of LSW was formed? 1986 was a record high, but was it really?

Section 5.4. I don't understand why 1991 was strongest in the mid- latitudes. If the strongest convection was observed closer to mid-1990s and it takes time for the signal to arrive it should be there after 1995 or so. So, it is not obvious to me from the present study how all these processes are inter-related, and how do they express themselves in the position of SPF at 500 m.

I think the theme is a very interesting and also challenging area for research, but more effort needs to be put to advance along the lines suggested by the authors. The Newfoundland Basin is a very complex area with 3-dimesional circulation and variability and as a good starting point I might also suggest to examine existing literature on theNewfoundland Basin and the areas around it.

Interactive comment on Ocean Sci. Discuss., 8, 453, 2011.