Response to Referee #1

We would like to thank the Referee for his/her constructive comments. We have taken into account all the points that were raised and we document the changes below.

Major comment :

Abstract and elsewhere: The authors claim that the Northwest Corner is a source of vorticity through the non-linear terms for driving the subpolar gyre in the North Atlantic. But I do not see how this is possible dynamically. The problem is that information propagates westwards along potential vorticity contours – either lines of latitude, as in the formulation of the barotropic vorticity balance given by equation (1), or along f/H contours as in the formulation used by Wang et al. (2017) (see comment 10. below). In either case, it is not possible for a vorticity forcing applied at the northwest corner to influence the gyre interior. It seems to me, therefore, that it is the eastern boundary regions that are important for driving the gyre and not the Northwest corner. Unfortunately, one cannot appeal to non-linear advection to get around this problem. To be influential, the non-linear term must be important in the eastern part of the gyre or in the gyre interior itself.

We thank the reviewer for this comment. The confusion comes from our definition of the gyre interior with the 3000-m isobath and the -3-Sv barotropic streamfunction contour (along the South Eastern edge). Due to baroclinicity, this region includes the Northwestern Corner (NWC) which can also be viewed as part of the subtropical gyre.
It is true there is no vorticity flux from the NWC to the subpolar gyre interior (excepted maybe by a small eddy component). However the vorticity balance of the region we have defined as subpolar (based on the barotropic streamfunction) is influenced by the NWC.
This is now made clear in (l.387-390) :
« Barotropic vorticity is also provided through a mean-baroclinic signal located in the NWC. Our definition of the subpolar gyre, based on a barotropic streamfunction contour, includes a part of the NWC which is a complex transition region between the subtropical and the subpolar gyre. »

Minor comments :

1. Line 31: Why mention Munk (1950) but not Stommel (1948)? I would refer to both.

A reference to Stommel (1948) was added on l.31

2. Line 35: An important role for the bottom pressure torque is also anticipated in the early, diagnostic model of Greatbatch et al. (1991) – their Figure 6.

A reference to Greatbatch (1991) was added on l.35

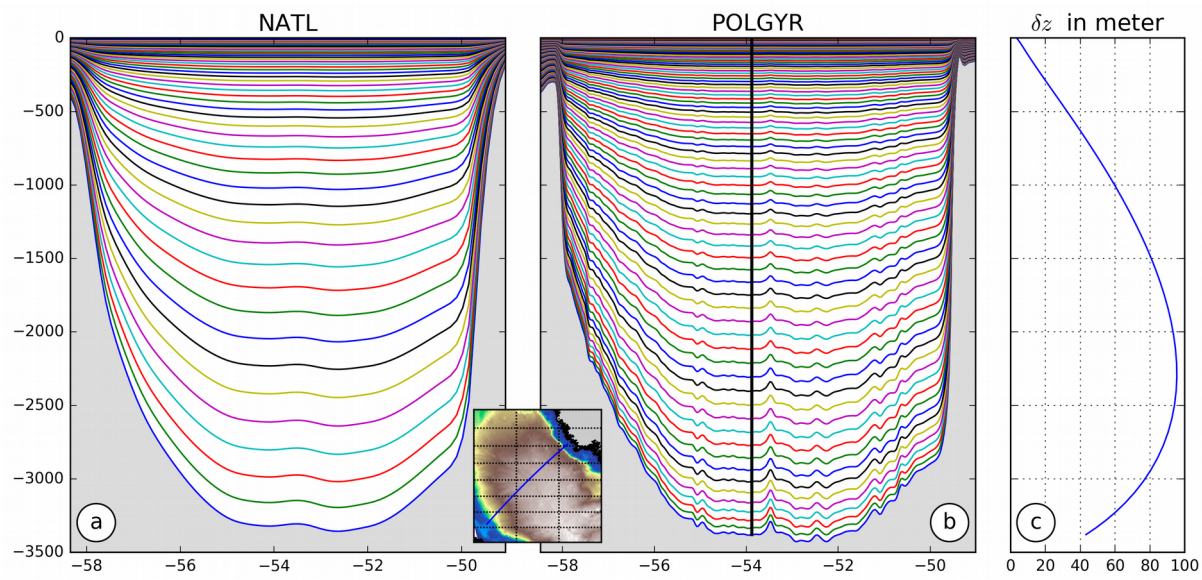
3. Lines 51-52: Wang et al. also showed the importance of the nonlinear terms in the subpolar gyre for driving the so-called Lavender recirculation – see their Figure 2c.

The case of the Lavender recirculation was added to the list of locations where the NL term is important (l.54)

4. Line 80: From Chelton et al., I would say that the radius of deformation for the 1 st baroclinic mode has trouble exceeding 10 kms and certainly does not reach 20 kms – see their Figure 6.

On the Southern edge of the domain the first radius of deformation is close to 20-km. To nuance our words we are now saying « first Rossby deformation radius remains below 10-km over most of the region » (l.80)

5. Line 97: From Figure 2, the vertical grid does not look to be particularly bottom intensified? We replaced the section in the Irminger basin by one in the Labrador Sea where we think it is clearer. We also added the variation of the grid spacing with depth along the vertical black line in (b).



6. Figure 3: It is not easy to see all the details in this figure – although I do not have specific suggestions for improvement.

We tried to improve the figure by changing the colormap to make the arrows more visible (p.6)

7. Line 156-157: As noted above, Wang et al. find an important role for what they call "mean flow advection" for driving the Lavender recirculation along the slope around the Labrador Sea.

Thank you for pointing this oversight. The following was added : « More recently Wang (2017) showed the importance of the mean flow advection in these circulations. ». (l.161)

8. Paragraph beginning on line 165: Could refer to Brandt et al. (2004, JGR).

Thank you for suggesting Brandt et al. (2004), the reference was added. (l.169)

9. Line 175: How is EAPE defined? This should be given somewhere.

The definition of EAPE is now added in the new equation (1) :

$$\text{EAPE} = \frac{-g}{2\rho_0}\langle z'\rho'\rangle$$

Where z' is the vertical isopycnal displacement,ρ' the density anomaly associated with this displacement and $\langle . \rangle$ is the time average.
Also precisions on the EAPE version of Roullet et al. (2014) were added. (l.180-186)

10. Line 186 and equation (1): Should mention that this is the vorticity equation for the vertically integrated flow. There is also an equivalent vorticity equation for the vertically averaged flow.

We are now mentionning the two different versions for the barotropic equations and commenting their differences in the text. (l.195-199)

11. Lines 205-206: Do the acronyms for these different models get defined somewhere?

Acronyms are now defined along with references to previous studies using these models. (l.216-218)

12. Line 218: ...also the subpolar North Atlantic, as noted above (point 3).

Same as in point 3 (l.231-232)

13. Line 221: I would not say the "advection of vorticity" when you are referring to the nonlinear term. It is easy to confuse with the "advection of planetary vorticity".

In order to avoid confusion « advection of vorticity » was changed by « nonlinear term » (l.233)

14. Line 280: Should "over" be replaced by "within"? Actually, the integral of this term should be very close to zero by construction.

« Over » was replaced by « within ». Because of model discretisation the integral is not exactly zero but very close. (l.293)

15. Line 294: My only objection here is that the Csanady paper uses dynamics linearized about a state of rest which means that the NL term plays no role, as could, perhaps, be made clearer. However, the comparison with the arrested topographic wave is certainly illuminating.

A reference to Csanady (1997) about JEBAR effect on the shelf has been added. The NL term is only important along the Greenland shelf and is related to eddy-barotropic component suggesting eddy interaction between the shelf and the open ocean. On the Canadian shelf the NL term is small and is barely contributing to the dynamics, thus the use of linearized dynamics seems valid there. The part with the coordinate changes has been removed for clarity. (l.305-310)

16. Figure 12: The dashed lines show isopycnal surfaces but which density is this? From the labelling, it must be a potential density of some kind. Please make clear.

Indeed, we are talking about potential density referenced at the surface. This precision was added in the caption. (p.19)

<u>Typos and language issues</u> :

Typos and language issues were corrected.