Responses in blue text

Review #2

The study uses a regional ocean model to investigate how the future ocean conditions in the northwest North Atlantic, like the emergence of warming and acidification, are controlled by climate-induce changes in the local circulation. The study demonstrates that a plausible increase in the slope water contribution to the Scotian Shelf associated with a weakening in the shelf break current, can drive enhance bottom water warming and salinification, and lead to localised regions of reduced/increased acidification (with less acidic regions being co-located with warmer regions).

The study will further our understanding of the response of the shelf seas and coastal regions to climate change (which currently is poorly understood). The use of somewhat "idealised" and targeted simulations (e.g., forcing the regional model projections with the same biogeochemical conditions but different physics-dynamics-circulation conditions), in my opinion, is a strength of the study and enables to inform on control mechanisms. The conclusions are well supported by the analysis and figures, and to the most part the manuscript is well written. However**, the description for the implementation of the downscaling experiment is difficult to follow (at least to me) and I am still unsure if I understood the implementation of the forcing for the projections with the regional model correctly**. Hence, I recommend the following revisions for clarity.

Response: We thank the Reviewer for their helpful comments. Please see our responses to each of their individual comments below.

Specific Comments

1. Description of the downscaling experiments with the regional model: I suggest that section 2.2 is reorganised, restructured and re-written for clarity. I suggest that section 2.2 is separated **into two subsections** that each separately describe the two experiments**: 2.2.1 downscaling using forcing from the GFDL-1%pCO2 increase per year for both physics and biogeochemistry; and 2.2.2 downscaling using forcing from the DFO under RCP 8.5** (to me the DFO model projections and their set-up was somewhat unclear) for the physics but using the GFDL-1%pCO2 increase per year for the biogeochemistry. **Please see specific comments below, but consider re-writing the entire section as to provide a clearer description for the set-up of your experiments.**

Response: Creating two subsections to describe the two experiments is an excellent suggestion, and we will implement this.

1.1 Lines 101-103 and 117-119: To me, it is not clear what "adding the anomaly (or delta) to the 1999 distribution or to the 1999 initial file" means and what this 1999 initial file/distribution corresponds to? Do you mean that the trend from the GFLD projection (essentially the de-seasonalised anomaly at 2065 relatively to 1999) was added to the 1999 conditions from the present day run with the regional model? Or do you mean that this trend was added to the 1999 conditions from the GFDL run itself (such as to keep a constant seasonal cycle?). **Please I suggest that you clarify what this 1999 initial file/distribution corresponds to.**

Response: The de-seasonalized anomaly at 2065 relative to 1999 in GFDL was added to the 1999 conditions from the present-day run. We will update the text accordingly.

*Lines 101-103: "The two regional model simulations were initialized in 2065 by adding deltas **from the larger-scale models** (2065 minus 1999 conditions) to the 1999 **regional model** distributions for temperature (T), salinity (S), horizontal momentum (U, V), sea-surface height (SSH), dissolved inorganic carbon (DIC), nitrate (NO3) and oxygen (O2)."*

*Lines 117-119: "The initial file for the time slice was **created by first calculating the difference between 2065 and 1999 for each of the physical variables** from the de-seasonalized monthly means and temporally stretched gridded data**. This difference was then added to** each of the physical variables **in** the 1999 **regional model** initial file, and the model was run for 16 years starting in 2065."*

1.2 Lines 120-121, surface and lateral boundary conditions: I am confused here. If I understood correctly, for the boundary conditions you do not use the same approach of adding "deltas" as for the initial conditions? If yes why not? Also, the text implies that for the ocean boundary conditions and atmospheric forcing you use directly the de-seasonalised GFDL outputs such that the imposed atmospheric and oceanic forcing for the ACM projections does not include any seasonal cycle? I am not sure that makes sense to me, so probably I have misunderstood of how the atmospheric and oceanic forcing is imposed at the open boundaries in the future time-slices experiments. **Please, I suggest that you clarify/re-write how the atmospheric and oceanic forcing along the open boundaries is estimated and imposed in the regional model future projections. Also, it will be useful to clarify which atmospheric fields are used to force your simulations.**

Response: We agree, this is a bit confusing. We will update the text to explain this better (see updated text below). Atmospheric fields used to force the model are air temperature, air pressure, radiation, humidity, rain and wind. We do calculate the lateral and surface boundary forcing files from a similar "delta" approach, which we have hopefully explained more thoroughly below. The GFDL outputs were de-seasonalized since we reconstruct the future forcing files using the seasonality from the present-day climatology (used in the present-day ACM simulation).

*"From the GFDL warming scenario, monthly output of all physical variables (T, S, U, V, SSH) and atmospheric forcing **(air temperature, air pressure, rain, radiation, wind, humidity)** were interpolated to the regional model grid using objective analysis. After interpolation, the mean annual cycle was calculated over the 80-year simulation at each grid cell for both the oceanic and atmospheric variables and removed, leaving de-seasonalized gridded data. The time dimension of this de-seasonalized data was then stretched so that the doubling trajectory of atmospheric CO2 closely resembles that of the RCP6.0 scenario (following Claret et al., 2018). This results in CM2.6 time being stretched by a factor of 1.903 (trcp6 = 1.903tcm26 + 1947.5) to equal RCP6.0 time.*

*The initial file for the time slice was **created by first calculating the difference between 2065 and 1999 for each of the physical variables** from the de-seasonalized monthly means and temporally stretched gridded data**. This difference was then added to** each of the physical variables in the 1999 **regional model** initial file, and the model was run for 16 years starting in 2065. The time-dependent surface and lateral boundary condition**s** were **also** taken from the de-seasonalized and temporally stretched data from CM2.6. **For this, timeseries of both atmospheric and oceanic variables from CM2.6 were normalized to calendar year 1999 by subtracting the 1999 de-seasonalized annual mean from the entire CM2.6 de-seasonalized timeseries** for RCP6.0 years 2065-2080**. These normalized timeseries were then added to the present-day climatology: for the atmospheric forcing, 3-hourly surface forcing from ECMWF Era-Interim data from 1999-2009 was used as the baseline; for the lateral boundaries, a long-term monthly mean from the Urrego-Blanco and Sheng (2012) regional ocean model was used as the baseline climatology**."*

1.3 Lines 124-125, DFO future projections: I am unsure what you mean by "six IPCC future climate runs", (maybe from 6 CMIP5 Earth system models?). Please, I suggest that you clarify.

Response: We have updated this to read "six **CMIP5 Earth System Model (ESM)** future climate runs".

1.4 Lines 131-132: This text suggests that only the air temperature and precipitation from the DFO RCP 8.5 projection are used as surface forcing for your downscaling experiments? **What about winds, humidity, radiation? How are the other atmospheric fields/forcing imposed in the regional model?**

Response: Only air temperature and precipitation were available from the DFO RCP 8.5 projection, thus winds, humidity, radiation, etc. were all assumed to change negligibly in this scenario. This is of course not necessarily an accurate assumption and we will add text to the methods acknowledging this limitation.

1.5 Lines 133-135: To me it is not clear why and how the conditions/fields along the lateral boundaries were averaged to get the delta added to the 1999 initial field. Are the anomalies/deltas (that are added to the 1999 initial fields) in the interior of your regional model extrapolated from the conditions along the oceanic lateral boundaries? To me that does not make so much sense and it will not lead to appropriate or consistent-to-the-forcing initial conditions for the time-sclices projections. I presume that I just have misunderstood as it is not clear and can you please re-write this part for clarity.

Response: David Brickman at DFO was willing to share boundary averaged deltas with us. More detailed output from his simulation is not available to us.

Lines 133-135: "*The boundary point profiles were averaged to get one profile of differences between 2066-2085 and present-day. This average profile was added to the entire 1999 initial condition to get the future scenario initial file."*

2. Line 163 and Figure 2: **Why were 9 months chosen as the timescale for which to present/discuss the averaged concentration of Labrador Sea dye after dye tracer initialization?** Is this 9-months timescale relevant in terms of the Labrador current velocities and shelf-lengthscale (i.e. travel distance) arguments? If you could please clarify.

Response: 9 months was chosen because we found it best illustrated the differences in the shelf-break current between the simulations. These are only snapshots of Labrador Sea dye since temporal averages of this dye tracer experiment don't necessarily make sense – the dye tracer is only initialized once and eventually leaves the model domain. We do believe, however, that these figures offer a nice qualitative complement to the more quantitative metrics like the dye tracer mass fractions and volume transport, both of which are calculated over the full simulation.

3. Figure 2 and lines 165: In my understanding **Figure 2c shows only the decrease in LS** concentration in the future projections, rather than the change in the future minus the present day (such that regions of increase are not shown). Comparing Figure 2a and Figure 2b it seems that they should be regions of increase in LS concentration, especially in the AMC-DFO. This can be confusing and makes it difficult to judge if the amount of LS dye moving along the shelf break declines for the AMC-DFO. **I suggest to update the figure to show the actual change (increase and decrease) rather than just the decrease.**

Response: We will update the figure to show both increases and decreases in LS dye concentrations.

4. Lines 227-228: I am not sure how accurate is this statement. In my understanding, the two simulations have also very different atmospheric conditions/forcing in the future. **Are the heat, momentum and**

freshwater air-sea fluxes similar in the two ACM-projections? If not, I suggest to clarify that the similarity of the air-sea CO2 flux in the two ACM-projections implies that "the shelf-break current strength is less of a control for the surface carbon budget" (rather that generalise to "water properties").

Response: Agree, this will be changed accordingly.

5. Figure 5 (typo in the caption): I believe you mean "Figure 5: Left panel … ph. Right panel….

Response: We thank the Reviewer for catching this!

6. Table S3 in the supplementary Information: For clarity, I suggest you mention in the caption that positive values indicate flux from the ocean to the atmosphere (i.e. outgassing).

Response: Agree, we will update the caption accordingly.

7. Figure S3 in the supplementary information: In the caption it is mentioned that "Open symbols indicate predicted values and filled symbols indicate actual simulated values". Can you please clarify what you mean by predicted vs simulated values here? Also, to me it seems that only filled symbols are shown in Figure S3. Additionally, I am unsure about the meaning/interpretation of the lines connecting the symbols, and of the arrows with the SLE text in Figure S3a and b. If you could please clarify what these lines and arrows represent/highlight (maybe in the caption) that would be very helpful.

Response: Apologies – the "open symbols" in the figure caption were a remnant from an earlier version of the manuscript that included an additional analysis that we ended up not including in the final version. This text will be removed from the figure caption. We will additionally add in text, as follows, to describe the lines and arrows, as suggested.

Figure S3: (a) T-S and (b) T-DIC diagrams, with different symbols indicating different simulations. ***Dashed lines connect endmembers and indicate the bounds of the mixing polygon. Arrows indicate where the St. Lawrence Estuary (SLE) endmember lies outside of the figure bounds.*** *Panels (c) and (d) indicate changes in temperature, salinity and dissolved inorganic carbon (DIC) between the future and present-day values (future minus present).*