Responses to Reviewer 2

Dear Reviewer:

Thank you very much for your review and comments. Based on your comments, we will modify the paper accordingly, and here is a summary of the modifications and responses.

(1) FESOM-ECHAM6 is a comprehensive GCM, but its treatment of sea ice processes appears to be fairly rudimentary compared to the current state of the art (e.g., CICE 5), including the representation of the evolution of the ice thickness distribution (following Hibler rather than a newer method such as linear remapping) and ice thermodynamic processes (using a simple zero-layer model rather than a model with multiple thermodynamic layers and treatment of changes in the brine pocket volume). It would be helpful for the paper to include a discussion of these potential deficiencies in the ice component of the GCM and whether or how they are expected to influence the results.

Based on the reviewer's comment, we now add the following content in the discussion part:

FESOM is a high-resolution ice-ocean model which has the advantage of providing a regional focus in an otherwise global setup (Sidorenko et al., 2011). In our work, FESOM applies even higher resolution in the high latitudes than the tropical and sub-tropical region. It is therefore suitable for investigating polar properties, particularly sea ice characteristics, and the related processes. However, a disadvantage of this comprehensive climate model lies on its relatively simple treatment to sea ice processes, which might have the potential to influence the simulation results. In detail:

1) Partitioning of lateral versus vertical growth of sea ice is most realistically simulated in models that explicitly include a sub-scale distribution of ice thickness (Hibler, 1979; Bitz et al., 2001). In such models, lateral melting can roughly be represented by the disappearance of the thinnest sea-ice classes and the accompanying expansion of open water. Bitz et al. (2001) also found that the simulated ice properties are sensitive to number of ice categories. In detail, a one-ice-category model tends to simulate larger ice concentration and smaller ice thickness compared to multi-category sea ice models.

2) A more recent method introduced by Lipscomb (2001)--- linear remapping of ice thickness distribution --- appears to be more accurate and less diffusive than the fixed thickness scheme (Hibler, 1980). Such method is applied in the current state-of-the-art model CICE5 (Hunke, 2010). In CICE5, all the new ice formed on open water is added to the thinnest ice category, without changing the ice concentration. If the new ice thickness exceeds the maximum threshold of the thinnest category, then the model

distributes excess ice volume over all categories, leaving ice area unchanged. Therefore, there is a big discrepancy between the representation of lateral ice evolution in FESOM-ECHAM6 and CICE5, the former is changing with open water ice growth, while the latter is influenced by other elements such as lateral heat flux (Maykut and Perovich,1987; Steele, 1992).

3) FESOM-ECHAM6 applies the simple zero-layer approach (Hilber, 1979) with linear temperature gradients from the sea ice top to bottom. More recently, a multilayer sigma-coordinate thermodynamic method is presented by Huwald et al. (2005). Furthermore, a brine pocket parameterization in introduced by Bitz and Lipscomb (1999); Huwald et al. (2005), which account for the temperature and salinity dependence of the sea ice properties. Compared to FESOM-ECHAM6, the multilayer thermodynamic method and brine pocket parameterization will lead to different vertical sea ice temperature profiles and sea surface salinity values over sea-ice regions, which results in a discrepancy in the conductive heat flux within sea ice and the freezing point of sea water.

Given above, compared to FESOM-ECHAM6, there are some relatively more comprehensive parameterizations describing sea ice processes. However, the lead closing parameter h0 in the advanced representations of sea ice evolution still needs to be determined empirically. A logical next step would be to investigate the sensitivity of the simulated climate to the lead closing parameter in climate models with the above mentioned new approaches.

(2) It was unclear to me whether the SIM includes an albedo feedback. In eq. 7, it appears that the albedo value of multiyear ice is used even when the ice concentration reaches zero. Note that parts of the year with zero ice concentration occur in all the runs that were examined in this study (Fig. 15). If the SIM does assign a different albedo to open water, then this should be clearly specified. If the SIM does not, then this should be justified, especially in light of the point that the SIM is used for comparison with the GCM which of course does have a surface albedo that depends on the presence of sea ice.

We assign a different surface albedo being 0.1 for open water. The ice albedo equation is only used with the existence of sea ice. We will clarify this point in the revised paper.

(3) The main features of Figs. 5-6 appear to match standard expectations. Since the growth rate of ice during wintertime is faster if the ice is thinner and fastest in locations of open water, increasing h0 causes there to be more open water and hence thicker ice. This standard expectation does not appear to be discussed directly in the paper, although the discussion on lines 1-5 of p. 2150 seems related to this. It would be helpful to add a discussion of this point, for example based on Bitz and Roe (J. Climate, 17, 3623-3632) or references therein. One purpose of

such a discussion would be to help distinguish what is novel in these simulation results from what matches standard expectations within the field.

According to the reviewer's comment, we modify the paper as following:

1) In Section 4.1 b which is related to Fig. 5-6, we add a new paragraph:
The anomalies of sea ice thickness and concentration mentioned above are in good agreement with former studies (e.g., Bitz and Roe, 2004; Fichefet and Maqueda,1997; Wang et al., 2010) which revealed that the heat loss in open water during wintertime significantly enhances sea ice formation, therefore increasing the lead closing parameter h0 causes there to be more open water and hence thicker ice.

2) In the discussion part, we add:
The sea ice change in FESOM-ECHAM6 induced by our modification to the open water ice growth representation coincides with previous studies. For example, through sensitivity experiments with the LIM2 sea-ice model, Fichefet and Maqueda (1997) indicated that when the model was run without leads, the air-sea heat fluxes were substantially modified, resulting in drastically reduced sea-ice thickness and total ice volume. Wang et al. (2010) revealed that the increase in h0 leads to decreased ice concentration during ice growth, and the annual mean sea ice volume, thickness and extent all increase with h0. In addition, Bitz and Roe (2004) found a growth–thickness mechanism, which describes a negative feedback between the ice thickness and the adjustment of ice growth rate to external perturbations.

(4) I found Fig. 9 to be less readily interpretable and the discussion of it on lines 6-14 of p. 2149 to be somewhat unconvincing. Is the plotted quantity (i.e., the "annual mean thermodynamic growth rate") the average value of $\dot{h}_{ice}$, or is it the average only considering times when $\dot{h}_{ice}$ > 0? If the former, in steady-state this field must be balanced by the annual-mean horizontal ice advection, and hence it is closely related to the ice motion field in addition to thermodynamic processes (and specifically to the equilibrium that forms between these two).

Thank you for the correction. In Fig. 9 of the original paper, the plotted quantity is the annual average value of $\dot{h}_{ice}$. Combining Fig. 9 and Fig. 5-6, we would like to indicate that the increased sea ice thickness and concentration over the GIN and North Atlantic region can only be affected by Fram Strait sea ice transport.

As the reviewer indicates, in steady-state this field ($\dot{h}_{ice}$) is closely linked to the annual-mean horizontal ice advection in addition to ice thermodynamic processes. Therefore, it is not sufficient for us to come to such a conclusion from Fig. 9 and Fig.

5-6.

To fix this, in the revised paper, we directly plot the time series of Fram Strait sea ice mass transport (Fig. R1 in this letter) instead of Fig. 9 in the original paper, which clearly shows a reduction in of sea ice import to lower latitudes from the Fram Strait in FE80 compared to FE-CTR. Furthermore, we do modifications to the paper as described as following:

1) We show the time series of Fram Strait sea ice mass transport together with the spatial anomalies of ice mass transport (Fig. 10 in the original paper) in Section 4.1 c, before the composite analysis between the sea ice import and sea surface salinity anomalies.

2) In Section 4.1 d, we write: <u>The increased sea ice in the Arctic strongly reinforces sea ice export into the subpolar regions through Fram Strait. Such process is clearly seen in the plot of ice mass transport as illustrated in Fig. 8. Therefore, the increased sea ice in GIN Sea, Labrador Sea and the Atlantic subpolar gyre is partly owing to the stronger Arctic sea ice transport.</u>

(5) How do the relevant differences between the two simulations compare with the level of internal variability in the model? Specifically, it would be useful to know how the results in Fig. 7 and 11 compare with the amplitude of variability from one 50-year period to the next in a control simulation (or in the spinup simulation, if it is sufficiently spin up for an extended period such that this can be examined).

Most of the differences (except surface air temperature and sea level pressure anomalies) shown in this paper are beyond the internal variability of the model. We calculate the standard deviations of sea ice thickness, sea ice concentration, thermodynamic sea ice growth, ocean temperature, ocean salinity, AMOC streamfunction, surface air temperature (SAT), and sea level pressure, based on the last 150 model years of the control simulation (last 100 years spinup run plus 50 years control run). Then we compare the anomalies of those variables mentioned above to their respective standard deviations. In terms of the anomaly figures in the paper, we marked the areas of significant changes with black dots.

Fig. R2 and R3 show the anomalies of sea ice thickness and concentration respectively, area marked with black dots represent that the difference over that region is significant. As can be seen, sea ice changes in the central Arctic, Fram Strait, North Atlantic subpolar region are significant. Furthermore, even though the Arctic SST has only a slight decrease, such little cooling is significant (Fig. R4a), and the pronounced cooling and freshening over North Atlantic are both beyond the standard deviations (Fig. 4a,b). In terms of the zonal ocean profile (Fig. R5a,b), the change of temperature and salinity are significant over the Arctic surface and subsurface, and over region of 0-100 m, 45-65^{o}N. The AMOC streamfunction is significantly weakened (Fig. R5c). Unfortunately, it appears that the warming over Europe and North America, and the sea level pressure (SLP) anomaly over the Northern Hemisphere, do not exceed their respective standard deviation (Fig. R6).

We will discuss about this in the discussion, saying that the parts of the SAT and SLP changes are not robust.

A striking feature of Fig. 7a is the large signal in the tropical Pacific. If this is truly a response to the difference in h0, then it is noteworthy (and relevant to recent discussions of tropics-Arctic teleconnections). If this is more likely related to internal variability in the model, then it should be identified as such.

In terms of the increasing SST over the tropical Pacific, Fig. R3a shows that such positive anomaly is significant, especially over 0-30^{o}S, 60-180^{o}W. Furthermore, a significant cooling is found over the northwestern Pacific Ocean. Such feature is very similar to the leading EOF teleconnection pattern of Pacific SST as described in Weare et al. (1976); Hsiung and Newell (1983); Deser and Blackmon (1995). Further study is needed to determine the nature of this association.

We will furthermore include a discussion of this in the revised paper.

(6) I found the mechanism proposed in Fig. 13 to be plausible but unconvincing. Is this to be taken as a speculative mechanism inspired by the model results or as a mechanism which has been robustly found to be occurring in the model? If the former, it should be specified as such. If the latter, further analysis would be useful. One key point here would be to show that the AMOC weakening really is substantially larger than the GCM's internal variability, such that the AMOC weakening can be expected to confidently be a response to the change in h0. If within the scope of the paper, showing that this positive feedback occurs during the internal variability of the GCM (which seems like it should be expected to happen if the mechanism is accurate) would be a helpful addition to convince the reader that the proposed mechanism really is occurring in the model.

The positive feedback is a robust result in FESOM-ECHAM6. As shown in Figs. R2,R3,R4,R5 the anomalies of ice properties, ocean surface properties, and AMOC is beyond their respective internal variability over regions of interest. The AMOC weakening (Fig. R5c), is significant especially over its maximum region (i.e., 1000-2000m, 30-60^{o}N), and over an area of 1500-2000m, 5^{o}S-5^{o}N.

(7) It would be useful if more details were given regarding the GCM simulations. It says there is a 300-year model spinup before the start of FE-CTR and FE80. Is this spinup using identical parameters as in FE-CTR? 300 years seems rather short for a GCM spinup. What were the initial conditions for the 300-year run?

Is the extent to which the model has equilibrated during these 300 years discussed elsewhere? If so, this should be cited. If not, it would be nice to include discussion of this here.

Yes, FE-CTR is identical to the 300 years spinup, so it is actually a 350 years run.

The control experiments FE-CTR is a 350 years simulation under pre-industrial boundary conditions. The control experiment is initialized by the mean climatology from an Atmospheric Model Intercomparison Project (AMIP) and the data from the World Ocean Atlas (WOA). It is integrated for 350 years in T63 resolution (about 1.9 x 1.9 degree) for the atmosphere component, and varying resolution as shown in Fig. 1 (of the paper) for the ice-ocean component. The sensitivity experiment FE80 is initialized from the 300th year of the control simulation, and integrated for 50 years. The average of the last 20 model years is analyzed.

300 years seems rather short for a GCM spinup. However, the model has run into quasi-equilibrium. Fig. R7 shows the temperature sequences for the last 150 model years of the control run. The trends in global mean ocean temperature of the last 50 model years (yellow region) is less than 0.005 ^{o}C/decade (0.005 ^{o}C/decade, 0.002 ^{o}C/decade and -0.001 ^{o}C/decade for sea surface, 1000 m depth and 3000 m depth, respectively), which meets the quasi-equilibrium criteria described in Braconnot et al. (2007).

Fig. R8 shows the time series of AMOC in both experiments. It is seen that the AMOC in control simulation reach quasi-equilibrium for the last 50 model years, with a trend of less than 0.05 Sv/decade, and a standard deviation of 0.59 Sv. Therefore, the anomaly of AMOC (approximately 1 Sv) is significant and robust.

We provide more details of the control simulation in the revised version.

Minor typo: On line 3 of p. 2139, was it meant to say “insulating” rather than “isolating”?

Thank you. We will correct this in the revised paper.

Figure Captions

Figure R1. Monthly sequences of Fram Strait sea ice mass transport. Units are Sv.

Figure R2. Sea ice thickness for FE-CTR, units are m. Area marked with black dots represent that the difference over that region is significant.

Figure R3. As in Fig. R2, but for sea ice concentration, units are $\%$. Regions with significant differences are marked with black dots.

Figure R4. Difference of annual mean (a) the sea surface temperature and (b) the sea surface salinity between experiments FE80 and FE-CTR (FE80 minus FE-CTR). Area marked with black dots represent that the difference over that region is significant.

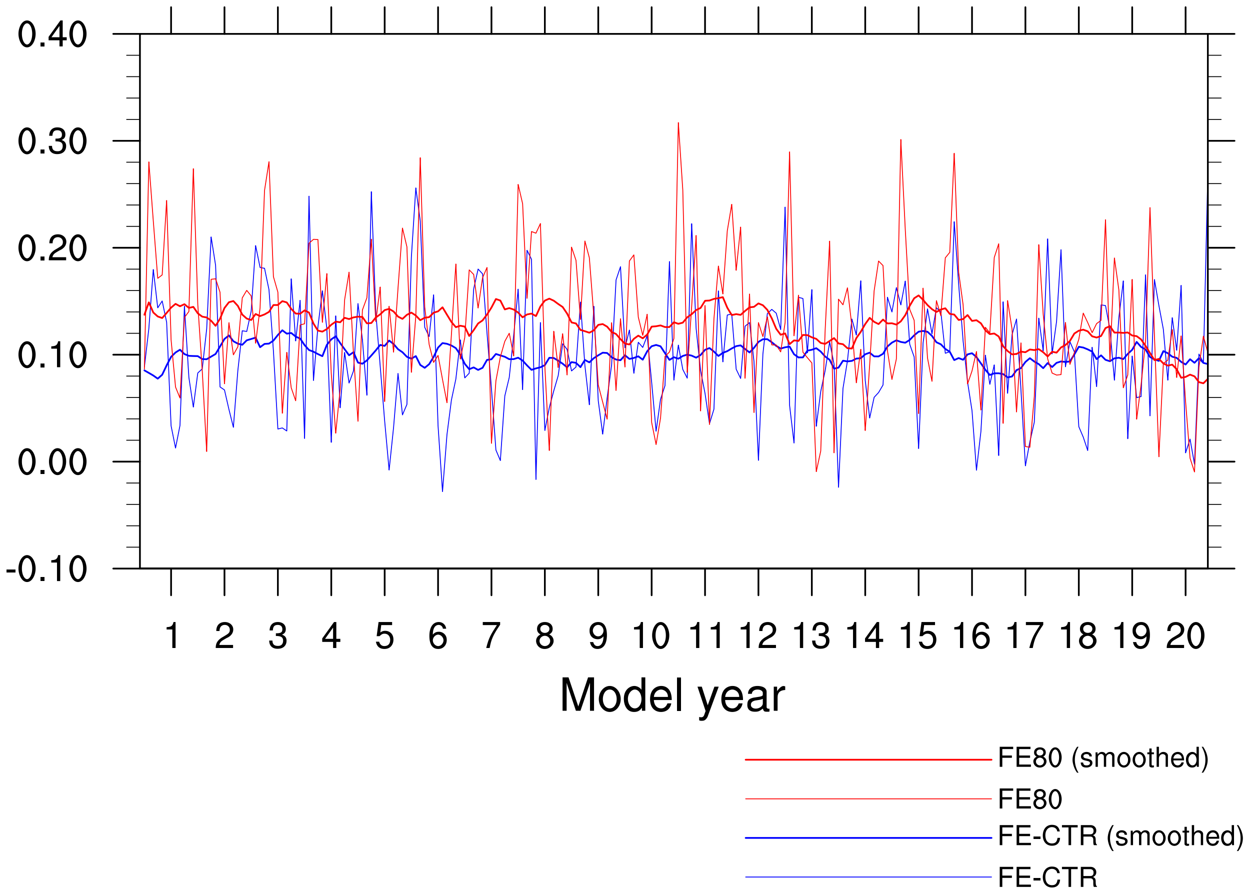
Figure R5. Difference of (a) zonal mean ocean temperature, (b) zonal mean ocean salinity and (c) AMOC between experiments FE80 and FE-CTR (FE80 minus FE-CTR) for the Atlantic region. Area marked with black dots represent that the difference over that region is significant.

Figure R6. Difference of (a) surface air temperature and (b) sea level pressure in boreal winter between experiments FE80 and FE-CTR (FE80 minus FE-CTR). Area marked with black dots represent that the difference over that region is significant. Units are $^{\circ}C$ and hPa.

Figure R7. Time series of global mean ocean temperature for (top) surface, (middle) 1000 m depth and (bottom) 3000 m depth. Units are °C.

Figure R8. Time series of AMOC index in both simulations. Units are Sv.

Figures



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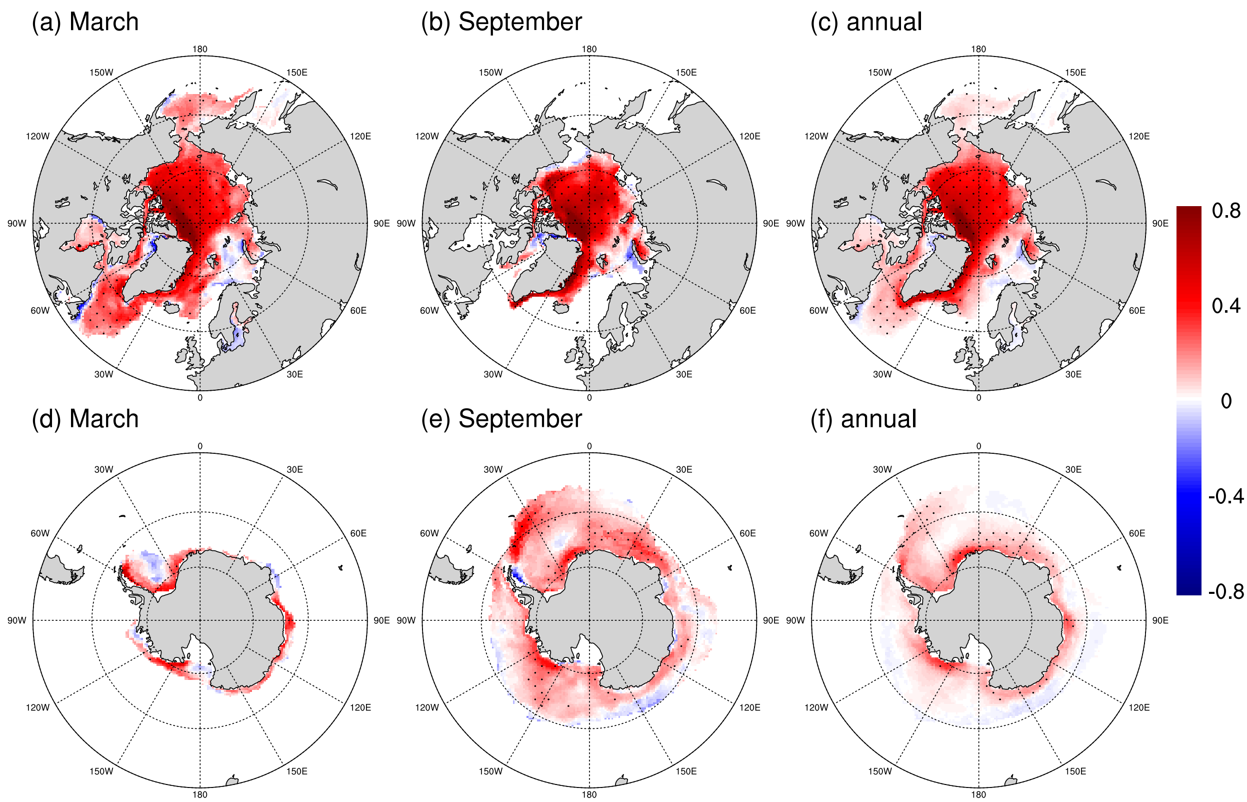


Figure R2. Sea ice thickness for FE-CTR, units are m. Area marked with black dots represent that the difference over that region is significant.

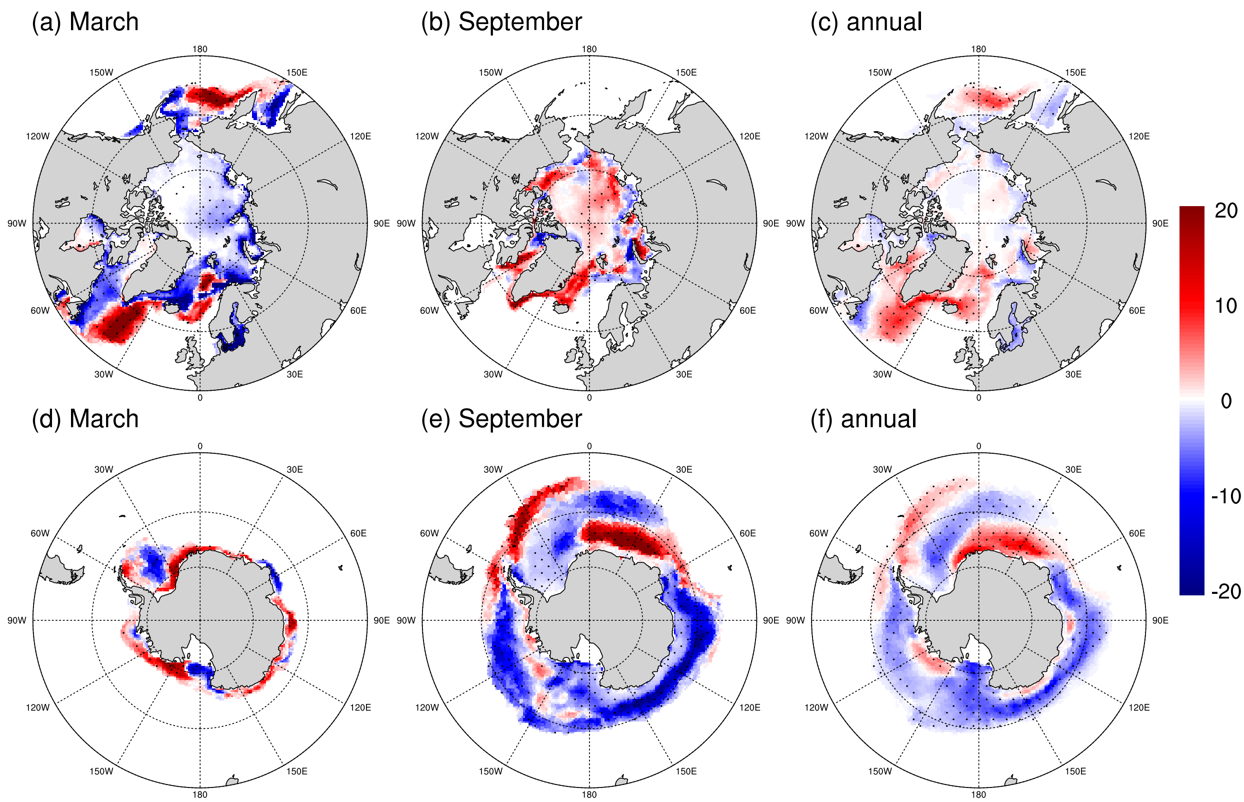
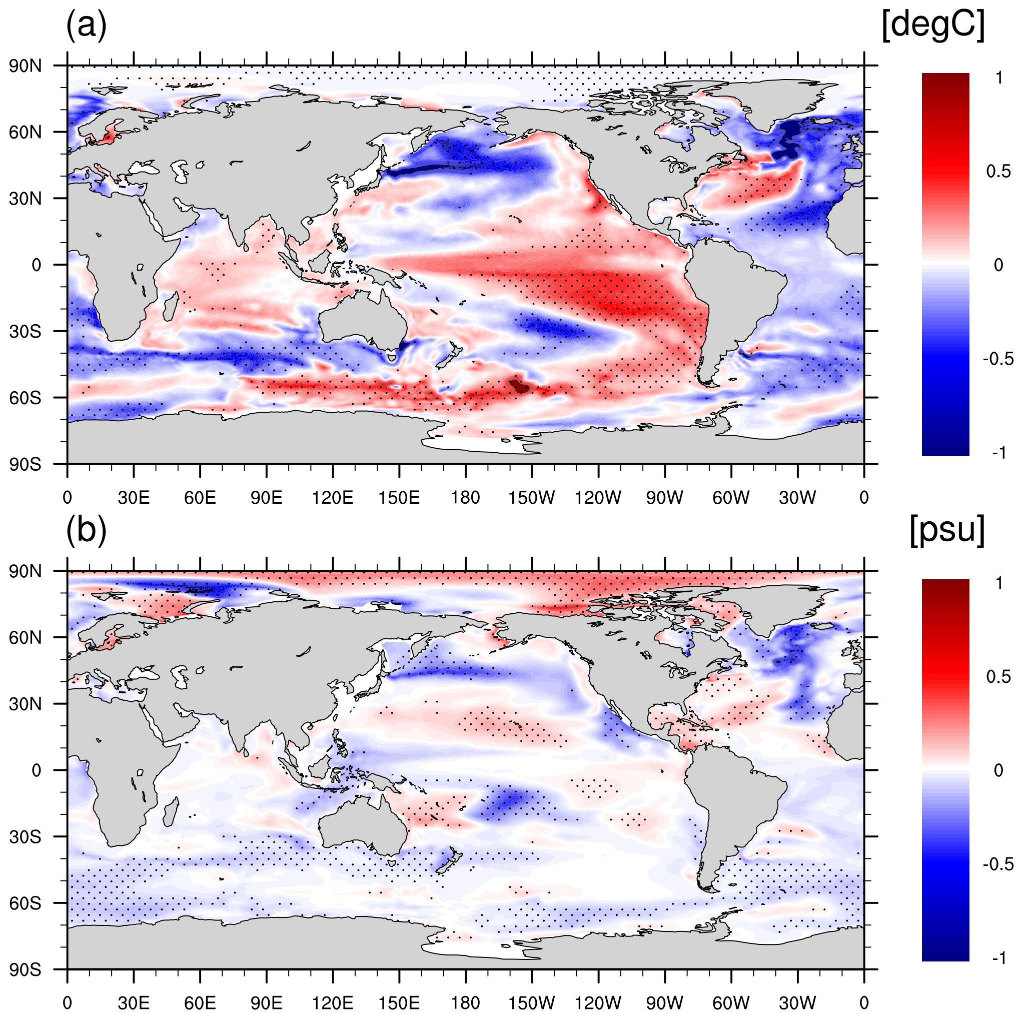


Figure R3. As in Fig. R2, but for sea ice concentration, units are %. Regions with significant differences are marked with black dots.



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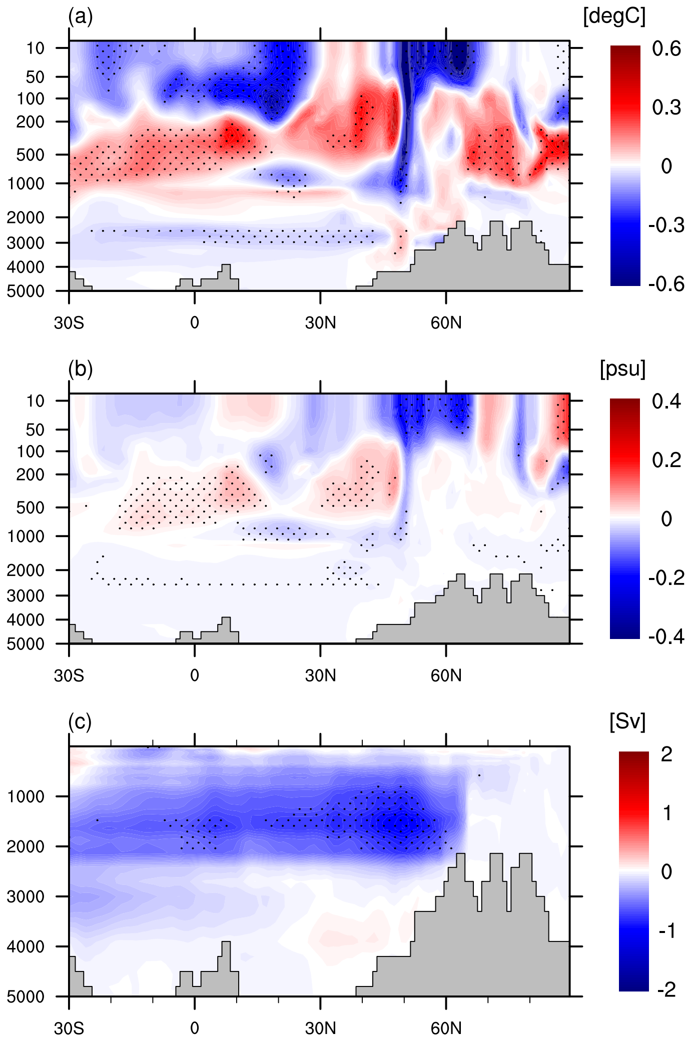

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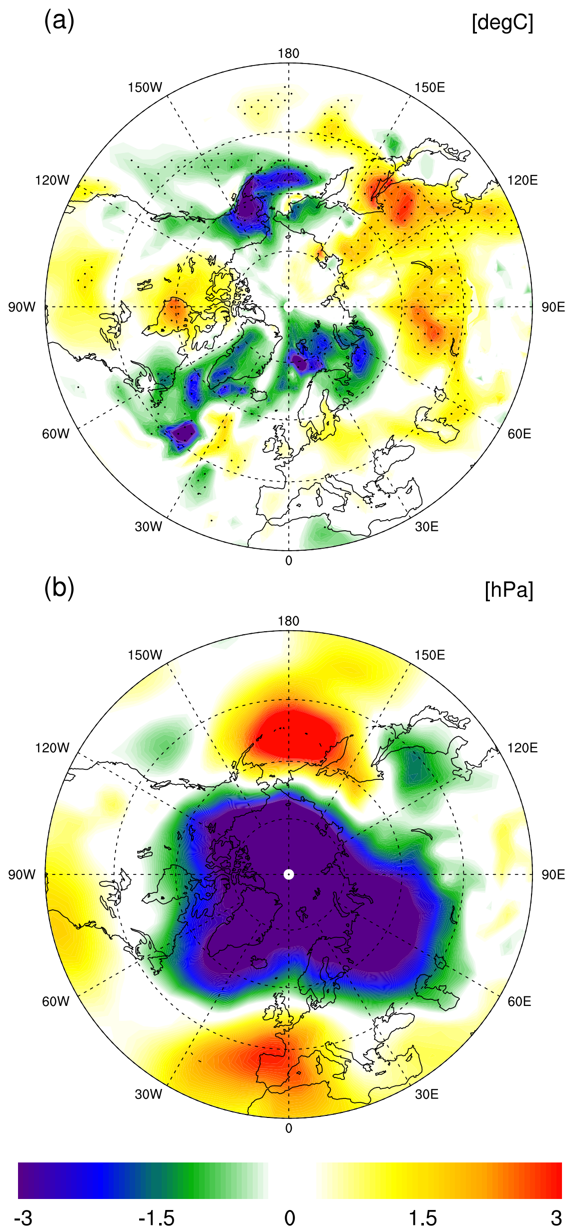

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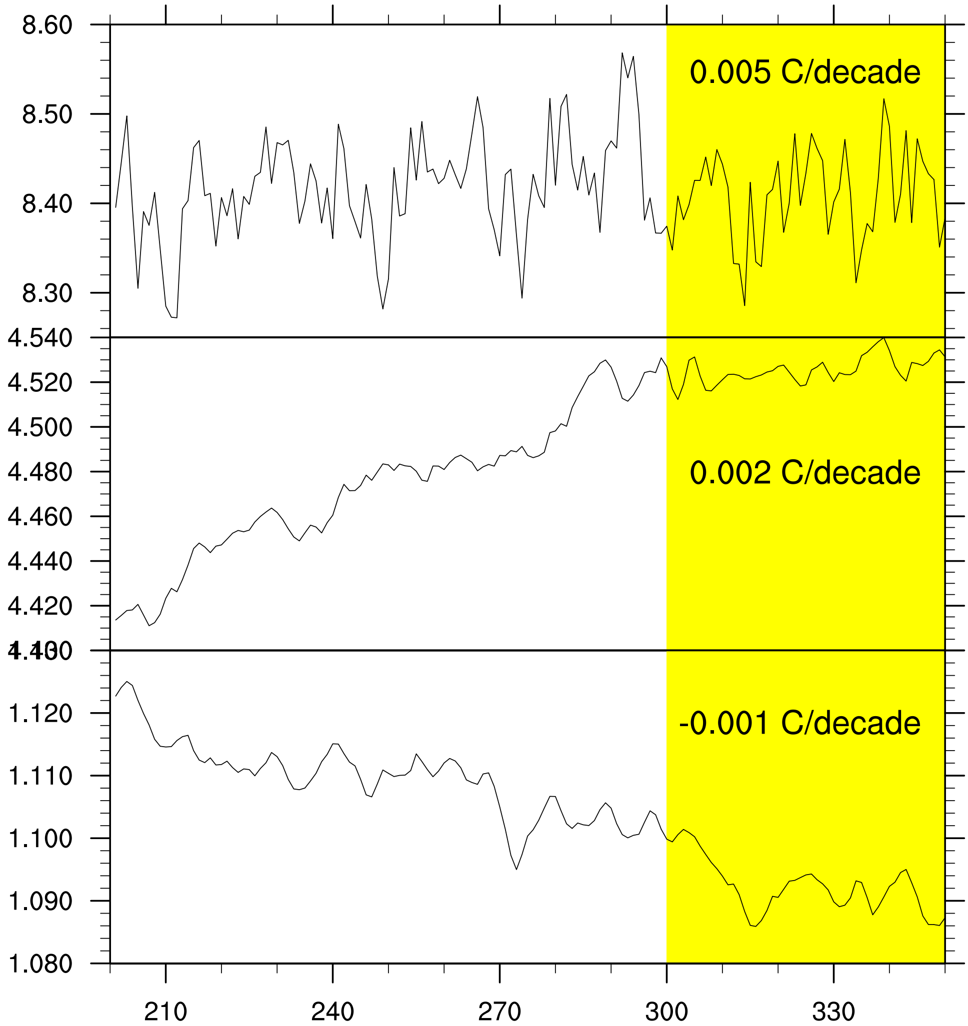

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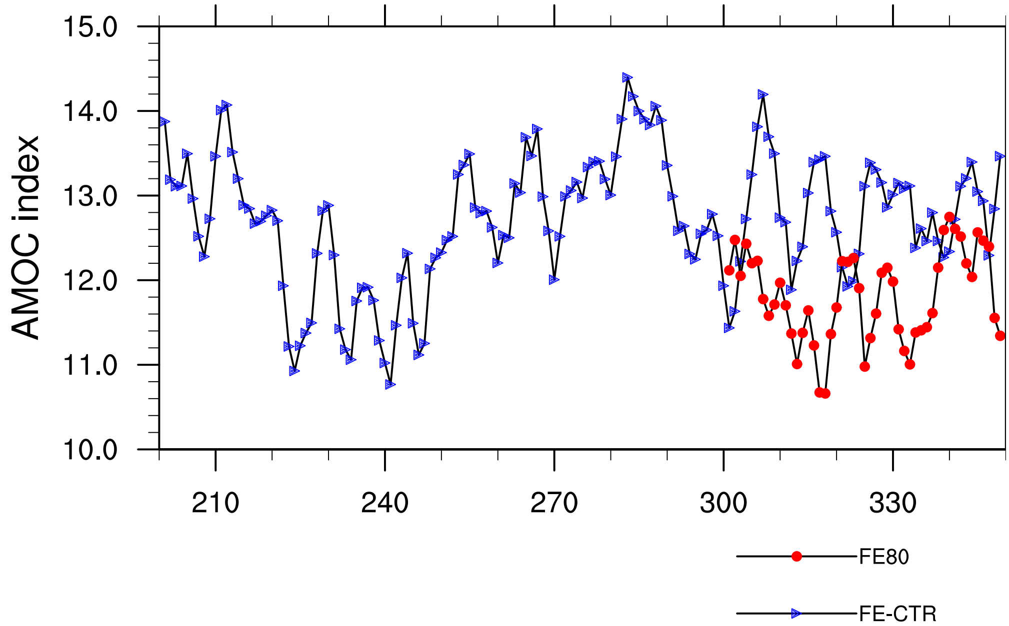

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