**Journal Name:** International Journal of Climatology

**Manuscript ID:** JOC-13-0063

**Manuscript Title:** Climate impacts of stochastic perturbations in the atmosphere on the ocean: sea surface temperature, Meridional overturning circulation and the ENSO variability

In the following, the text with italicization indicates the reviewer’s comments, and the normal text is our response.

**Reply to Reviewer 2:**

***Comments to the Author***

*This paper discusses some of the effects of the weather variability on the mean state of the ocean and the atmosphere, based on an Interactive Ensemble (IE) strategy with the CSM model. Reducing the influence of weather noise leads to a surface cooling, which is largest in most polar regions and is attributed to weakened evaporation, enhanced by the positive ice-albedo feedbacks and a weakening of the meridional overturning circulation (MOC). The changes in rainfall are also discussed.*

*Although some of the results are of interest, the analysis is mostly superficial and descriptive. Some of the effects of the weather forcing are obvious, and in most cases little new insight is provided, except in the discussion of the impact of the MOC changes. Hence, more work and major revisions are needed before the paper can be considered for publication in IJC.*

**Response:**Thank you for your insightful comments. In the updated manuscript, we focused on the influence of stochastic noise generated by atmospheric dynamics on the ocean. The mixed layer ocean heat budget, the oceanic meridional overturning circulation (MOC) and the ENSO variability were examined. The possible roles of the stochastic perturbations in the atmosphere were discussed. Here is the item-by-item reply to your comments.

***Major comments***

1. *The style is poor, wordy at time, and the English often approximate. Please try to improve them.*

**Response:** Done as suggested. The text has been extensively reworded

1. *The discussion is often limited to showing figures and describing the differences between standard and IE simulations. It should also give physical insights, and always explain or speculate on the reasons for the differences. For example, no explanation is provided when remarking that* *the spread due to initial conditions occur where the differences are largest. Is it that hard to figure it out?*

**Response:** We have revised the paper to include more discussions of the physical mechanisms. To

The spread of surface temperature of the SC ensemble runs with different initial conditions occurs where the differences between the SC simulation and the IE simulation are largest. This is because region with stronger internal variability is also the place where the atmospheric stochastic noise is more active. We have clarified this in Page 8, Line 5-9.

1. *The main surface stress differences in Fig. 4(6) should be discussed and explained. Why does the stress increase in some regions and decrease in others?*

**Response:** We have revised to point out that the wind stress at the peripheries of the storm tracks, especially on the poleward side, is reduced in the IE . These regions are expected to have the largest atmospheric stochastic variability is large. The sustained significant surface stress weakening may be also partly explained by the positive feedback of surface stress to surface temperature changes. Convective instability over warmer water will lead to a deeper boundary layer and result in intensified surface stress over the warmer water, and vise versa (Samelson et al., 2006). We have clarified this in Page 9 Line 15 to Page 10 Line 2.

1. *Evaporation depends on wind speed, not on wind stress. Again, there is no discussion, just a (repetitive) description of some of the differences. Also, please briefly summarize here the possible links to the NAO and the PNA.*

**Response:** In the revised manuscript, the evaporation, surface wind speed and their relationship with surface latent flux release are examined and discussed (Figure 9 and 10). The upward latent heat flux is linked with evaporation of water at the surface and the subsequent condensation of water vapor releasing energy into the atmosphere. The surface wind speed may serve as a medium and be responsible for the latent heat flux changes in IE. The obstruct effect of the grown sea ice may be responsible for the weak correlation between the evaporation and wind speed in sub-polar regions. We have clarified this in the second paragraph on Page 13.

As shown in Figure S1, there are not significant NAO-like or the PNA-like changes in the IE simulation. But a positive AAO – like changes are evident and suggested to be related to the increased precipitation in eastern China. We have clarified this in Page 8 Line 23 to Page 9 Line 7.

****

**Figure S1** (a) Spatial structure of the sea level pressure anomalies in IE averaged from year 541 to year 570, units: hPa. (b) is the same as (a), but for the geopotential height at 500hPa, units: m. The shading area represents the differences significant at the 5% level using student’s ***t*** test.

1. *Section 4.1: the cooling does not only depend on the surface turbulent fluxes, but also on the radiation fluxes. The latter are mentioned in section 4.2, but shouldn’t they be included when discussing the global balance? What about cloudiness? Any significant changes?*

**Response:** We reorganized the manuscript as suggested. We examined the mixed layer heat flux budget in section 4.1, including the net radiation at sea surface, surface latent and sensible heat fluxes, as well as the solar penetration at the bottom of the mixed layer.

We examined the cloud radiative forcing in Figure 8d. The cloud radiative forcing partly offsets the warming effect of net incoming radiation increase in the Arctic and coastal region along 70°S and tends to amplify the role of net surface radiation at mid and low latitudes. The cloud cover changes in the IE platform are mainly due to the anomalous low-level cloud amount, decreasing/increasing over cold/warm sea surface. We have clarified this in the second paragraph on Page 12.

1. *The discussion of the sea ice differences does not distinguish between causes and effects, and it should not ignore the possible direct effects of the weather forcing.*

**Response:** You are right and thank you for this suggestion. The direct cause of the difference is stochastic forcing. Sea ice further amplifies the cooling through a positive feedback. This is now included in the revised text (Page??, Line??).

1. To better (and more concisely) explain the MOC changes, it would be useful to show the changes in the oceanic mixed layer depth at the end of winter, when deep water is formed. Also, the changes in the wind stress curl should be shown.

**Response:** We examined the changes in the oceanic mixed layer depth at the end of winter in Figure S2. The MLD weakening is strongest during the boreal winter. Although the annual mean results are weaker than that during the wintertime, they can well describe the main MLD changes in both the Northern and Southern Hemispheres. So we only showed the annual mean results in the revised manuscript (Figure 6). We have clarified this on Page 15 Line 8-11.

The changes in the wind stress curl are shown in Figure 11b. The regional ocean current adjustments to surface wind stress curl may be responsible for the changes at regional scale. The changes in the wind stress curl and the associated sea surface temperature anomalies are now discussed on Page 13 Line 22 to Page 14 Line 22.

E:\Cooperations\973\June_ini\eps\revision\mixedlayer.eps

**Figure S2** Mixed-Layer Depth (MLD) differences between IE and SC outputs. Values significant at the 5% level using student’s ***t*** test are shown. Units: m.

***Miscellaneous***

1. *There are a large number of unnecessary statements that distract the reader, such as line 3 to 5 in the Introduction, or most of the upper half of p. 24.*

**Response:** We deleted the unnecessary statements in the Introduction and have tried to polish the entire manuscript.

1. *Reference to Collins and Allen: somewhat outdated. Newer results suggest enhanced predictability.*

**Response:** We have rewritten the Introduction The reference to Collins and Allen was deleted.

1. *p. 3: I thought that the main interest of ensemble simulations is to reduce the signal to noise ratio, not the sampling uncertainty.*

**Response:** The original statement has been deleted.

*4. Fig. 1 does not bring much and could be omitted.*

**Response:** Done as suggested.

1. *p. 6: CSM or CCSM?*

**Response:** The specific name for the standard coupled climate model in the previous manuscript is CSM, which may cause confusion with the other model that exists already, e.g. CCSM. Therefore, the specific name for the standard coupled climate model is revised to SC in the revised manuscript.

*6. p. 6, middle: the spread is small for 50-yr averages.*

**Response:** The traditional initial-condition ensemble and the interactive-ensemble are quite different. Therefore, in the revised manuscript, we focus on the differences between the IE model output and one single SC model output.

*7. Please do not call “drought conditions” a decrease in precipitation, unless precipitation has become very small.*

**Response:** The statement was revised as suggested.