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

**Reply to Reviewer 1:**

***Comments to the Author***

*This manuscript tackles an important problem, namely the climatic importance of stochastic fluxes across the air-sea interface. The key finding, that the fluxes are crucial for faithfully reproducing the mean climate state, is certainly worthy of publication in IJOC.*

*However, I have some major and minor comments that will need to be addressed before publication.*

**Response:** Thank you for your encouragement and insightful comments, which helped us to make a significant improvement to the original paper. Here is the item-by-item reply to your comments.

***MAJOR COMMENTS***

*1) Page 5 states "the purpose of this study is to estimate the influence of stochastic noise... at the air-sea interface". In fact this has been done before, using a complementary approach, by explicitly adding stochastic noise to simulations (Williams 2012, doi:10.1029/2012GL051813). The differences between the two approaches should be discussed. For example, the* *approach of Williams (2012) requires only a single model integration, not seven, so it is computationally much cheaper. Also, in the first sentence of page 9, it should be mentioned that this expectation is consistent with Williams (2012), whose stochastic noise increased the oceanic mixing and reduced the sea-surface temperature.*

**Response:** The works in Williams (2012) do help us to better understand the climate impact of stochastic noise at the air-sea interface and is an important complementary approach to our study. We cited his work in the Introduction (Page 3, Line 7-9): There are noise-induced the mean climate drift and noise enhanced variability when explicitly adding stochastic noise to simulations (William, 2012).

It should be noticed that the changes in our IE platform are suggested mainly being wind-driven, whereas William emphasized the effects of the surface buoyancy fluxes. We discussed the differences between the two approaches in the Conclusion (Page 19 Line 25 to Page 20, Line 10): It should be noticed that the changes in our IE platform are mainly wind-driven. The dampened stochastic perturbations decreases the oceanic mixing, increases the solar penetration at the bottom of the mixed layer and leads to sea surface cooling, especially at middle and high latitudes. On the other hand, in Williams (2012), the water and heat components were perturbed separately in two experimental runs, emphasizing the effects of the surface buoyancy fluxes. The stochastic modifications in Williams (2012) are more pronounce in the tropics, increasing the oceanic mixing and reducing the sea surface temperature. The approach of Williams (2012) requires only a single model integration, so it is cheaper in terms of computational costs and is an important complementary approach to our study.

*2) Pages 12-14 study the* *Atlantic meridional overturning circulation. I have some concerns about these results.* *First, the integration length is only 150 years, which is not long enough for the deep ocean to come into equilibrium with the new IE forcing. Second, the manuscript analyzes only the final 50 years, so the changes identified* *could be simply multi-decadal variability rather than a changed mean. The consequences of this limitation will need to be discussed in the manuscript.*

**Response:** We examined the Atlantic meridional overturning circulation (AMOC) in the 850-year pre-industrial equilibrium run with the SC model (Figure 1c) and the 128-year simulation with the IE platform (Figure 12a). After the spin-up period, the AMOC index reached a quasi-equilibrium state in the SC model (Page 5, Line 17-22). The IE platform was initialized from the SC equilibrium run every 5 years apart from year 420 to 450. As shown in Figure 12a, the Atlantic MOC strength is about 1.2 Sv weaker than SC in the first 40 years, drops almost linearly in the following 20 years by about 0.8 Sv, and then reaches a quasi-equilibrium state. The limitation of the relatively short integration length was now discussed as suggested. Details can be found in the first paragraph on Page 16.

*3) The statistical significance of the changes is never assessed. I suggest applying standard significance tests (e.g. t-tests) to all the results, and re-drawing the figures to indicate the significant regions (e.g. using stippling).*

**Response:** Done as suggested. We assessed the statistical significance of the changes and redrawed figs. 4-6, 8-12.

*4) The use of English is poor. I recommend that the manuscript be* *proof-read thoroughly by a native English speaker.*

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

***MINOR COMMENTS***

*1) Page 5, line 42: How were the seven sets of initial conditions selected?*

**Response:** Seven sets of initial conditions selected from the SC equilibrium run every 5 years apart from year 420 to 450 are used to initialize atmospheric components of the IE platform. The initial states for the single ocean model and the sea ice model are from the same initial set. We have clarified this on Page 7, Line 9-11.

*2) Page 5, line 55: How long is the coupling step?*

**Response:** The single ocean model and the sea ice model are coupled with the ensemble mean flux of the multiple atmospheric members at every coupling step, one day for the ocean model and 60 minutes for the sea ice model. We have clarified this on Page 7, Line 12-15.

*3) Figures 1-3 were too small for me to be able to see clearly. But from what I could see, the color bar on Figure 2(b) will need to be improved. Using a single color for most of the globe (dark green, from -1 to 0) is a poor choice. I would like to see contours at -0.25, -0.5, and -0.75.*

**Response:** Done as suggested. We have redrawn Figure 2b (Figure 4b in the revised manuscript). Although the changes in the tropics are small, the cooling is significant in the eastern tropical Pacific, tropical Atlantic, and some spots in the North Indian Ocean.