

Dear Dr. Dow and co-authors:

Thank you for your thoughtful responses to the concerns and comments of the reviewers on your manuscript. Reviewer #2 has remaining concerns that must be addressed before the paper can be accepted for publication in WCD (posted to you, I believe). Below I add my concerns which should be addressed, and some suggestions to make text. Line numbers refer to the uploaded pdf named "egosphere-2023-1595-manuscript-version3.pdf"

Regards, David

We thank the Editor for the further feedback and for collating additional comments from the reviewers. Below are, first, the responses to the Editor's comments, and second, the responses to the comments of reviewer 2. The responses below are in blue. We hope you find the revised manuscript improved and ready for publication in WCD.

In his original review, Reviewer 2 asked: "What does it mean to express the anomalies between NUDGED and CONTROL "per standard deviation of the PDO index"? And how does one compare the amplitudes between the two?" Your response (reproduced next in blue) was very helpful and I strongly urge you to include this in the text – and to add similar text to the caption of Figure 1.

**The anomaly between NUDGED and CONTROL is projected onto the first EOF from the control run to generate a pseudo-PC. The anomaly is divided by the pseudo-PC to calculate the anomaly per standard deviation of the PDO index expressed in a similar way to that derived from CONTROL.**

As suggested this text has been included in the manuscript and the caption of Figure 1

The schematic in Fig. 7. is somewhat misleading: The negative SLP anomaly north of the equator is centered near ~30N and not 10N as presently shown. This is important because south of the maximum (near 10-20N) there will be anomalous westerlies – reduced trade winds, and thus reduced evaporation. Also, the red arrows just south of the equator that point northward are inconsistent with the SLP field (they should point southward). Finally, the text in the figure caption should be sharpened to avoid confusion (in particular, subtropics usually refers to ~10-30N, while extratropics includes the midlatitudes and subpolar regions). I suggest replacing the caption with the following (or something like it):

**"Figure 7:** Schematic depicting the mechanisms involved in the tropical SST anomalies manifest as a result of an intensification of the AL. An intensified AL (dashed black line) imposed during boreal winter is associated with westerly anomalies (reduced easterlies; solid red arrows) in the

*subtropics and downward latent heat transfer. The migration of the SST anomalies southward during boreal winter is associated with westerly anomalies in the subtropics (reduced trades). The westerly anomalies act to weaken the background trades (filled red arrow) which reduces latent cooling due to decreased evaporation and hence an increase in subtropical Pacific SSTs. In the season after nudging, the temperature asymmetry about the equator induces an SLP gradient (solid black line, positive SLP; dashed black line, negative SLP) that drives southerly winds across the equator. The Coriolis force acts to turn the southerly winds in the southern hemisphere westward and in the northern hemisphere eastward. When these anomalous winds are imposed on the background easterly trade winds (filled red arrows), the southerlies south of the equator increase the wind speed and therefore evaporative cooling, whilst north of the equator the background trades are weakened, reducing evaporative cooling. The westerly wind anomalies along the equator deepen the thermocline in the eastern tropical Pacific (red dotted line) and reduce upwelling/divergence of cooler waters at the equator.”*

*Thanks for this feedback - the figure and the caption have been updated as suggested*

*The paragraph on lines 73-93 is not relevant and is a distraction for the reader. Please remove it.*

*Done*

*There is some sloppiness in Eqns. 1-6 that need to be fixed. I will attach at the end of this document a page that will help.*

*Thanks for sending this through - the equations have been amended as suggested*

*Line 154, change to read “within the nudging period ( $d = 0$  is 15 Jan)”.*

*Done*

*Line 158-162, this is confusing. How the amplitude of the imposed anomaly compares to the maximum amplitude in ERA5 isn't helpful. What is relevant is how the variability in the CONTROL compares to the variability in ERA5. With this in mind, I suggest you change the text on line 158 to read*

*“... with an NPI anomaly of -10.76 hPa, or -3.02 s, where  $s = 3.53$  hPa is the standard deviation ...”, change the text on line 161-2 to read “... reanalysis data from 1979-2020, a 1 s NPI anomaly is 5.20 hPa.”, and change the text on line 163 to read “... conducted using a comparably sized NPI anomaly in reanalysis data.”*

*This point was also noted by Reviewer 2. The authors agree and have changed the text accordingly.*

*Line 223, "... Pacific Ocean ..."*

*Done*

*Line 275, change to read "... There are positive (downward) ..."*

*Done*

*Line 279-282, change this sentence to read "The pattern of surface latent heat flux anomalies in JJA in the extratropical North Pacific resembles the SST pattern associated with the internal PDO (Fig. S1d) and represents a damping of the SST anomalies; positive flux anomalies extend eastward from the KOE region, which are enveloped by negative anomalies in the northeast Pacific and subtropical North Pacific. The ..."*

*Done*

*Line 299, change to read "...zonal wind anomalies represent a ..."*

*Done*

*Line 331, change to read "...from the surface in the northern subtropics due to reduced..."*

*Done*

*Line 340-3421, the NPO is an intrinsic mode of atmosphere variability, not an intrinsic coupled atmosphere-ocean mode. Change to read "...the North Pacific Oscillation (NPO), but they imposed ..."*

*Done*

*Line 350, change to read "...coincides with an anomalous northward..."*

*Done*

*Line 358, replace "Investigation into" with "The"*

*Done*

*Line 355, change to read "...the warming in the central near-equatorial Pacific..."*

*Done*

*Figure caption 3: change "NUDGED-CONTROL" to read "NUDGED minus CONTROL". Also, add the text "The subtropical North Pacific and Nino3.4 domains are indicated by the boxes in Fig. 1".*

*Done*

*Figure caption 6: change "NUDGED-CONTROL" to read "NUDGED minus CONTROL".*

*Done*

*Figure caption S1: change "Seasonal mean surface" to read "Seasonal mean skin"*

*Done*

*Figure caption S4: What are the box limits? The whisker limits? Presumably 10, 25 75 and 90%, but best to state this explicitly rather than making the reader guess. The text on line 36 in parentheses is confusing. Suggest replacing this text with the sentence: "The maximum and minimum values of Nino3.4 in the HadISST4 and Control run are indicated by an "x" (and then put the x's on the plot.*

*Done*

## Author Response to Reviewer 2 Further Comments

Thanks for engaging constructively with the review process and improving the framing of the manuscript. The title and abstract much more accurately reflect the results of the study and connect it to the state of the literature. While the model used is borderline inadequate for the questions investigated, this is at least caveated, so I don't think this needs to preclude publication. I have some more additional (mostly) minor comments to address before publication.

We thank the reviewer for the further constructive comments on the revised manuscript. We have taken these into account in the resubmitted version. We hope the reviewer finds the manuscript improved and ready for publication in WCD.

Major(ish) comments:

1. Your explanation of the quantification of statistical significance on lines 170-178 is quite sloppy and I think it is done incorrectly. You talk about multiplying by  $\sqrt{2}$  to correct for the difference of 30-year means, but you should also divide by  $\sqrt{2}$  to go from 15-year to 30-year averages. Technically this should also account for the autocorrelation of subsequent 15-year averages, but it would be easier just to use 30-year means of the control run instead. An even bigger omission is that you should also divide by  $\sqrt{50}$  to account for the ensemble average. Slightly concerning that this would make almost everything significant, but 50 members is quite a large ensemble.

Thanks for raising this issue. Given the points raised in the comment, we have revised the approach for estimating statistical significance. Instead of comparing the ensemble mean response to unforced variability derived from CONTROL, we have instead computed the standard error on the ensemble mean using the NUDGED ensemble spread. This enables us to account for different averaging periods used in different parts of the analysis (e.g. Figure 2 shows different averaging periods from Figure 4) and overcomes the issues with estimating low frequency variability from the relatively short CONTROL run. We now estimate the significance of the ensemble mean anomaly by computing the standard error of the mean:

$$SE = \sigma/\sqrt{n}$$

where  $\sigma$  is the inter-ensemble standard deviation of the time averaged quantity of interest (e.g. 30 year mean surface temperature) and  $n$  is the ensemble size, 50. We then identify where the ensemble mean NUDGED minus CONTROL anomaly  $\pm 2 \times SE$  is different from zero. This achieves a similar outcome to the point raised in the comment that the significance of the ensemble mean response is increased. The revised method is described in the Section 2.2.

2. Section 3.3: It is not clear from your figures that any of the wind anomalies are significant other than the ones in DJF and during the ramp up/down in November/March, unless you are only meaning to draw attention to the anomalies outside the nudging region in the subtropics.

Note that “persistent anomalies extending into the spring after nudging ceases (MAM)” is not true unless you remove March from the averaging, since nudging is still active in March. Perhaps you could instead show the anomalies from what would be expected if all you saw was the imposed nudging. When mentioning the upper tropospheric anomalies in Figure S5, it would be worth reminding readers that the nudging is smaller in the upper troposphere.

During the analysis, the results from separate months were analysed (not shown in the manuscript) and the anomalies referred to here do persist into April after nudging has ceased. We have amended the sentence to specify we mean April for clarity. We have also added a sentence referring to the weaker upper tropospheric nudging in the description of Figure S5: *“Recall that the nudging strength in the upper troposphere is several times weaker than at the surface (Fig. S2), so the upper-level circulation anomalies likely represent a response to the lower tropospheric forcing.”*

3. Line 391-396: Nothing you say here is inconsistent with Klavans et al. (submitted, [https://staff.cgd.ucar.edu/cdeser/docs/submitted.klavans.human\\_emissions\\_pdo.apr23.pdf](https://staff.cgd.ucar.edu/cdeser/docs/submitted.klavans.human_emissions_pdo.apr23.pdf)), who also not the small amplitude in models and suggest some reasons for it. Please remove this reference from this final paragraph, because your study does nothing to refute it (Note that I am in no way affiliated with the Klavans study). The references to Klavans et al. elsewhere were okay.

Thanks for highlighting this - reference has been removed from this sentence.

Remaining minor comments:

- Line 73: I had to read this sentence several times to make sense of it, and I think the problem is that “via” should be replaced with “by” - Done
- Section 2.1: It might help to add a sentence of two in this section why this low-resolution model was used, though caveats added later are sufficient.

We have added *“While the model is run at relatively low horizontal and vertical resolution, the model code is sufficiently flexible to apply the nudging method described in Section 2.2 and the model is computationally efficient to run enabling a large ensemble to be produced.”* to the end of Section 2.1.

- Eq. 5: spurious dot after a - removed
- Line 161: Based on the values in hPa, it is clear that the sigma is not the same here as defined on line 158. Please clarify this in the text. It would be cleaner (and more robust statistically) to state what the corresponding ~3 sigma value would be in ERA5, rather than stating the most extreme value.

This section has been corrected by changing the text to:

“Comparing with ERA5 reanalysis data from 1979-2020, a  $1\sigma$  NPI anomaly is 5.20 hPa. The imposed atmospheric forcing is therefore weaker than if an equivalent experiment was conducted using a comparably sized NPI anomaly in reanalysis data.”

- Line 168: Given the use of the long-term climatology of CONTROL here, it would be good to make a quick check to make sure there is not long-term drift in CONTROL, which could affect the results.

Thanks - this was checked at the outset of the study and the first 150 years of the run were discarded for spin-up/drift.

- What does LR mean in several of the figure titles? It is never defined.

Linear Regression - it is now defined in the caption for Figure 1 and S1.

- Figure S1: I think you mean HadSST4, which is used in HadISST1. Something looks very off with the patterns in HadSST4; have you removed the linear trend (if so, Section 2.4 doesn't say so)?

You are correct - these are HadSST4 monthly anomalies (not detrended) for the last 100 years. The 1<sup>st</sup> EOF of SST anomalies in the PDO region is calculated and the SST anomalies are regressed onto the corresponding PC1.

- Just to check, for Figure 1, you are computing the EOF from SSTs and then showing the associated surface air temperature anomalies? If you computed the EOF from surface air temperatures (including land), then this is not okay and needs to be updated.

You are correct - the 1<sup>st</sup> EOF from SSTs inside the region defining the PDO is computed then the surface temperature anomaly derived from regressing surface temperature anomalies onto PC1 is what is shown.

- Figure 2: I strongly caution against discussion SON as an initial response. From comparing Figure 2a and 2e, it is clear that almost the entire signal in (a) is coming from year 2, where the anomaly persists from the previous DJF.

Analysing the first year in isolation showed (not shown in manuscript) that there is a response in SON in year 1 (due to nudging starting in November) – therefore we are comfortable with the term ‘initial response’.

- Line 250-255: You need to state explicitly when you compare with observations that the value of the tropical response in the nudged runs is ~10x weaker than the value in the observed PDO variability.

This has now been stated explicitly.

- Line 268-273: missing reference to Figure 3c,d

Added.

- Figure 3c,d: It is not clear what the shading is showing

The shading was a visual aid and has now been removed to avoid confusion.

- Line 279-280: Figure 4 does not show the surface heat flux anomalies associated with the internal PDO, which is what your text on this line indicates.

To avoid confusion we have removed the clause “resembles the SST pattern associated with the internal PDO” from the sentence.

- Line 285-286: Not clear what is meant by “limited dynamical feedback to the atmosphere”
- Figure 5: Please state what is meant by near-surface. 10 meter? Lowest model level?

Done.

- Figure S5: Which season?

DJF - Added in caption.

- Line 305: it could be helpful to remind readers that this is showing anomalies that are entirely outside of the nudging region

Thanks for the suggestion – we have added this to the text.

- Line 368: “Specifically” is repeated with the last sentence and doesn’t really fit here. Suggest a use of also or furthermore instead, since this is a separate point from the ocean vertical resolution.

Have changed to “Furthermore”