Dear Dr. Dow:

The comments by the two reviewers are pertinent and I hope you take them all into consideration in revising the manuscript. I have further comments that I would hope you address in revising the manuscript that should help the reader to get a clearer picture of what is going on in the model.

A basic calculation that should be presented in the revised manuscript is a comparison of the amplitude of the 3 sigma Aleutian Low anomaly in the model to the amplitude of an observed 3 sigma anomaly. That is, is the forcing applied to the model of realistic amplitude, or is the simulated variability in the Aleutian Low to weak? If the latter is the case, then that would explain why the observed PDO variability is too weak in the model. But I expect there is another, more likely, explanation for the "apparent" weakness of the PDO variability in the model compared to observations. (In fact, as far as I can tell by the figures in the manuscript, the SST variability driven by the internal intrinsic Aleutian Low variability is quite consistent with that observed.)

In reading the manuscript, it is clear that some of the concerns of the reviewers stems from a misinterpretation of what is the PDO. This paper uses an old definition of the PDO (the first EOF of SST in the N. Pacific; e.g., Mantua et al. 1997) that reflects the leading pattern of interannual to decadal variability in the N. Pacific. As such, it includes interannual SST variability that is driven by ENSO and the interannual and lower frequency SST variability that is driven by the internal variability in the Aleutian Low (as measured by the NPI index) and external forcing (e.g., volcanic eruptions). The SST patterns associated with both driving mechanisms are very similar. Hence, the traditional "PDO" index used in the current manuscript includes SST variability due to ENSO variability (hereafter $PDO_{hf}$) and SST variability due to stochastic forcing by the Aleutian Low that (hereafter $PDO_{lf}$) is intrinsic to the midlatitude atmosphere (i.e., associated with fixed climatological SST); hereafter I use quotes to denote the traditional PDO index, "PDO", because it is a statistical artifact that conflates two different driving processes). When statistical methods are used to remove the ENSO contribution from the "PDO", the extratropically driven contribution ($PDO_{lf}$) is well described as the response of the midlatitude ocean to stochastic forcing by the internal variability associated with the Aleutian Low, with SST anomalies local to the climatological Aleutian Low driven by turbulent heat fluxes, and delayed SST anomalies in the Kuroshio region driven by the ocean gyre adjustment to wind stress curl anomalies associated with a stochastic Aleutian Low (Wills et al. 2019, Newman et al. 2016, Zhao et al. 2021 and references therein). This view that the PDO is an extratropical phenomenon stems from analysis of the observations and analysis of the CMIP5 and CMIP6 climate models (ibid), and is now widely referred to the PDO. Importantly, the Wills et al show the $PDO_{lf}$ has only a very weak footprint in the tropical Pacific. (Zhao et al show secondary feedbacks that couple the tropical Pacific and midlatitude N. Pacific, contributing to the variance along the west coast of the North America).

There are several important implications for the interpretation of the results presented in the authors manuscript. First, ENSO contributes approximately equally to the observed "PDO" amplitude. Hence, the discrepancy in the amplitude of the "PDO" seen in Fig S1 may be due to an ENSO that is too weak in the model that is superimposed on a *realistic* amplitude $PDO_{lf}$ variability. Given that the observed $PDO_{lf}$ has a very weak footprint into the equatorial Pacific, one would have to scale up the model "PDO" to see the weak tropical footprint of the $PDO_{lf}$ to match the PDO pattern in the observation. Indeed, it seems to me that if the amplitude of the midlatitude SST anomalies in Fig S1 in the control were divided by ~3, they would have the nearly the same amplitude of the $PDO_{lf}$ anomalies in the observations shown in Wills et al.[1] Second, if this is the case, then the present study is a consistent with the analysis of observations that show the SST variability driven by anomalies in the Aleutian Low have a very weak footprint in the equatorial Pacific.

Specific comments:
The title: it doesn't really make sense. *Variability* in the Aleutian Low provides the forcing for the SST *variability* in the N. Pacific on decadal and multidecadal time scales that features the PDO pattern of SST anomalies. What this paper has shown is that a *climatological* change (increase) in the amplitude of the Aleutian Low will act change the *climatological* SST with a pattern that is very similar PDV pattern; it cannot change the variance in the PDV, however. A more apt description of the paper conclusions is that a persistent positive Aleutian Low forcing causes a persistent PDO-like pattern of SST change in the N. Pacific.

Lines 57-58, "The prevailing paradigm for the PDO regards the role of the Aleutian Low to be largely driven by tropical processes". This is not true. The prevailing paradigm of the PDO is that is interannual to decadal variability in the North Pacific Ocean that is driven primarily by stochastic variability in the Aleutian Low that is largely unrelated to changes in SST – including changes in tropical Pacific SST; it includes SST anomalies driven by turbulent heat flux anomalies associated with the Aleutian Low anomalies and the delayed response due to the integrated wind stress curl anomalies associated with the Aleutian Low. The leading pattern of SST variability in the North Pacific (the traditional PDO definition) includes both the midlatitude PDO and a contribution due to ENSO variability, communicated to the N. Pacific by atmospheric teleconnections.

Line 81: please add Wills et al 2019 to the list of references, as this brings the list up to date and arguably is the cleanest description of the modern day view of the PDO.

Line 135-136: Nudging is applied to the region of the observed climatological (DJF) Aleutian Low, but does it also align with the model simulated Aleutian Low? [See also comment #2 of reviewer #2.] Is the pattern and amplitude of the leading mode of atmospheric variability in the observations, the Aleutian Low/NPI index, consistent with that simulated by the model? Panels b and f in Fig. S1 suggest the model Aleutian Low/NPI might be centered ~30 degrees west of that observed.

The description of the heat budget analysis is confusing. It is well known that the turbulent fluxes are the leading term in the variance budget for winter averaged SST tendencies and they should explicitly appear in Eqn. 5 and not lumped in with diffusion. See also comments by reviewer 1 on the 30 m depth vs. mixed layer depth.

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[1] To fix ideas, consider the SST anomalies along 40-50N in the central Pacific. A observed 1 sigma ENSO event (a 0.8C Nino3 anomaly) causes a ~0.4C anomaly ($PDO_{hf}$) in SST in this region. From Wills et al, a SST anomaly associated with a 1 sigma $PDO_{lf}$ is ~0.2C. If these were independent processes and shared the same pattern, then the standard deviation of the total variability in the extratropics would be 0.44C =sqrt(0.2^2 +0.4^2)). However, if the model had a very weak ENSO compared to observations (say 1 sigma Nino3 of only 0.37C, 1/3 of that observed) but a realistic $PDO_{lf}$, then the extratropical SST amplitude would be 0.24C (=sqrt((0.4/3)^2 +0.2^2)) and scaling the model result by a factor of 2 or 3 would bring the tropical and extratropical SSTs in line with observations.

The tropical anomalies in the schematic in Figure 7 and the description in the text doesn't make sense to me. Figure 6 shows that in response to the Aleutian Low forcing, warm anomalies and negative SLP anomalies in the northern subtropics ~20N, and cold SST anomalies and positive SLP anomalies in the southern subtropics ~20S; hence, a cross equatorial pressure gradient to the north (there must also be a zonal pressure gradient to accompany the zonal wind anomalies centered on the equator). The schematic shows just the opposite.

I concur with Reviewer #2 that the similarity of the patterns in Figs. 1a and 1b should be quantified by a pattern correlation.

In the captions to figures 2-6, please note the averaging period that is being displayed. Are these figures composites for years 1 to 2, years 3-4, years 5-30?

Figure S1 (bottom panels) show the observed PDO has a similar pattern with the same sign in all seasons. This makes sense because the SST is dominated by low frequency variability. It is difficult to explain how, in the model, SON differs can differ in sign from the other three seasons.

Please show the same field in the top and bottom rows of Fig S1 (presently, it appears that the simulated 2m temperature is shown in the top row, but the observed SST is shown in the bottom row. I suggest showing SST for both.

Figure S3 is confusing. The contours are the same in all four panels, yet the caption states that anomalies are contoured. The amplitude of the surface heat flux anomalies (~ 0.03 $W/m^2$) seems to be two orders of magnitude too small to explain the SST anomalies. Most confusing of all is the pattern of heat flux anomalies that accompany the imposed Aleutian Low anomaly: the pattern should look like that in Fig. 4b and 4f (and in observations) – and yet there are anomalies of opposing signs on along the southern flank of the imposed Aleutian Low.

References


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