## Author response to Reviewer #1 comments:

Reviewer comments are in black, and our responses are in red.

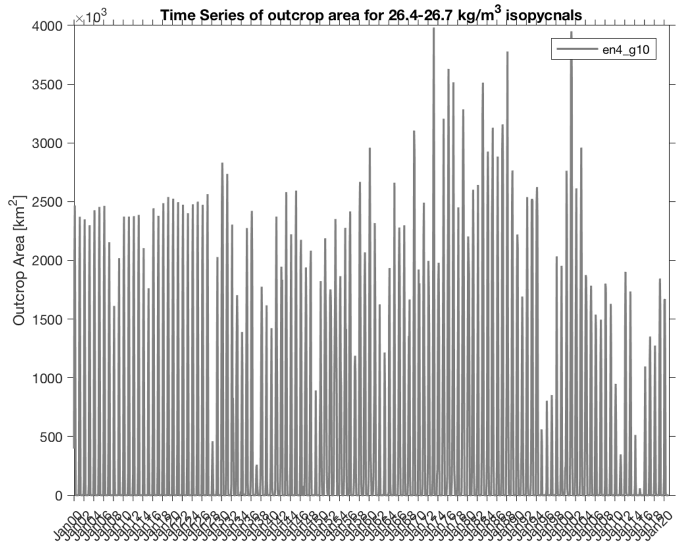
This study attempts to understand the causes of decadal variability of dissolved oxygen in the Gulf of Alaska, observed at the Ocean Station P (OSP) at 145°W, 50°N. The oxygen timeseries at the OSP is the longest record of dissolved oxygen. The decadal variability of this data has been studied intensely over the last 20+ years and there are many hypotheses proposed to explain this variability. This study put forward the idea that the subduction of thermocline waters generates oxygen variability in the western Pacific, which would then propagate eastward following the circulation pathway of the North Pacific Current. Approximately 10 years later, the signal reaches the OSP and is observed there.

This hypothesis itself is not new, but what is new in this study is that a significant correlation was found between dissolved oxygen at OSP and the isopycnal outcrop area in the western Pacific which was reconstructed from the historical observations (EN4). Furthermore, the outcrop area is controlled by the density of the sea water at the surface, which further depends on the variability of sea surface salinity, rather than temperature. However, this variability does not exhibit significant correlation with the dominant mode of climate variability in this region. The maximum lag-correlation values are on the order of 0.5 to 0.6, which would explain about 25-36% of the total variance. This seems a significant contribution to the total oxygen variability, while it also leaves significant room for other mechanisms as well. My overall impression is that the manuscript contains significant progress on this problem and merits publication. Having said this, I would like to raise two points that the authors can consider for a revision.

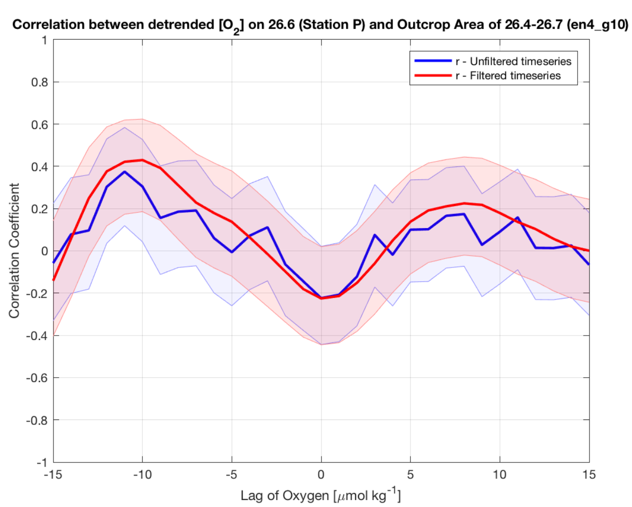
Thank you for the comments. We appreciate the careful analysis of our manuscript. Below we provide responses to the two points raised and how we would address them when revising the manuscript (if invited to do so).

At L75, the authors choose to analyze data after 1982 only. I ask the authors to reconsider this choice because dissolved oxygen data from OSP exists from 1956. Since the focus of this study is the decadal variability with approximately 20-year timescale, the statistical significance of this analysis is critically limited by the effective sample size. The additional 26 years of data can capture additional full cycle of the signal potentially. Figure 2 indeed supports that the EN4-derived outcrop areas show the same pattern of maxima and minima as EN4-OISST. Then, it is possible to include the additional, extended timeseries before 1982.

Thanks for the comment. We did look at the full record of $\sigma_\theta$ = 26.4–26.7 kg m$^{-3}$ outcrop area calculated from the EN4 data since 1900 (in addition to the record since 1982 used in Figure 2) as shown here:




But we did find the relatively constant outcrop area in the early part of the record (1900-1925) questionable. While uncertainties are provided with each data point provided in EN4 due to gridding errors, uncertainties due to measurements error/bias do not appear to be fully accounted for (since uncertainties reported with the EN4 dataset do not become larger going back in time to 1900, as one would expect for earlier data, resulting in small uncertainties in outcrop area for the whole record). As a result, we concluded that the combination of OISST with EN4 SSS provided the most accurate and consistent dataset (at ¼ degree resolution) for us to use from 1982 (the start of OISST) onward. However, it is possible to also use more of the EN4 record as you point out (maybe starting 1925). We have recalculated the lagged correlations between outcrop area and $O_2$ at OSP as in Figure 5 in the manuscript, but using the EN4 record since 1941 for outcrop area (since the $O_2$ record starts in 1956 this is the first data year used when calculating correlations with a maximum lag of 15 years) instead of EN4-OISST. The result is shown here:




The patterns are the same as we had shown for the EN-OISST data in the manuscript (Figure 5) with maximum correlations at close to +/– 10 year lags, thus not altering our conclusions.

However, the magnitude of the maximum correlations is smaller than in Figure 5. For completeness, we suggest to add the two figures shown above (that use the EN4 data from before 1982) to the appendix of a revised version of the manuscript with elaboration and comparison to Figures 2 and 5 as they are now (using the EN4-OISST dataset since 1982) in the text.

At L135, it does make qualitative sense that a larger outcrop area indicates more ventilation, and a smaller outcrop area indicates less ventilation. However, not enough reasons were provided to justify why the spatial extent of the surface outcrop area is used as the only proxy for the amount of ventilation. There are atmospheric reanalysis data products available for estimating buoyancy and wind stress forcing. The latter can provide an estimate of Ekman flow. Mixed layer depths and geostrophic circulation can also be estimated from EN4 products. It makes me wonder if there were any reason that direct subduction estimates weren't used in this study. It is possible that these calculations were already performed by the authors or prior studies by others. If this is the case, it would be important to include this information.

We agree that calculation of subduction rates would provide a more complete metric of ventilation, i.e. of how much water is moved from the mixed layer into the thermocline within density classes. However, we find that using outcrop area as a proxy for ventilation is a simple first step since it only requires temperature and salinity data at the surface (compared to many different data products as you point to properly calculate subduction rates) and is thus easier to monitor, and since it is clear that there is no subduction if a density class stops outcropping. We will highlight these points more in the manuscript. We do point out at a couple of places in the manuscript (see lines 138–140, and lines 388–390 at the end) that proper calculation of subduction rates is a next step (which we have not done yet), but that it is beyond the scope of this paper. While vertical Ekman pumping and lateral induction estimates are needed to fully quantify the amount of water and $O_2$ that is transported across the base of the mixed layer (as carried out in a modeling study by Kwon et al. (2016) which suggested that outcrop area is the primary cause of interannual variability in subduction in the northwestern North Pacific), the spatial extent of the surface outcrop area can be used as an indicator for the amount of ventilation taking place. For clarity, we will make sure that a revised manuscript will emphasize earlier on that outcrop area is used as a simple proxy for ventilation (citing Kwon et al, 2016) and that more complex subduction rate calculations would be required to accurately describe ventilation.

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