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Ocean Science Discussions

Interactive comment on "On the multiple time scales of variability in the Northeast Pacific Ocean" *by* R. Tokmakian

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A kind thank you to the reviewers for taking time to comment on the paper. My responses to their concerns are given below.

1. I do not think this paper is acceptable for publication in Ocean Science, in this or any moderately revised form. The arguments and analyses are too incoherent, and the conclusions too vague. 2. The paper is very confusing. There are constant shifts in the regions discussed, swaps between area averages and local (1 degree) sea level variability, and a wide array of techniques applied, with the results often over-interpreted and of dubious statistical reliability.

These two comments are difficult to respond to because they are so general in nature.

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My response to 2, is that the statistical techniques used are consistent with each other. Others may choose to organize the discussion in a different manner. The manner in which I chose seemed to allow for an appropriate discussion of the complexity of the variability.

3. The author begins by looking at the effect of the 1997/98 El Nino on estimates of variability, but concentrates on a barely-visible westward extension of the variability in the middle of the basin, mentioning the much more obvious difference in the south east corner of the region on secondarily, and completely missing out the clear, coherent increase in variability all along the continental shelf/slope region north of 30N.

Other sections of the paper discuss the coastal signal El Nino response. This particular comment is made with respect to the strong signal of 97/98 and its relative to other areas strong response in the southeast section of the region. The area north of 30 is relatively not as strong a response (5cm^2 vs 10+cm^2).

4. She goes on to identify regions of "significant" variability in three spectral bands. However, the meaning of "significant" is not clear - it appears to mean that the variability is higher than would be expected from an assumed background spectrum shape, but the shape assumed is different for each band (with no reason for the choice given), resulting in the possibility that all three bands (which together account for almost all of the spectrum, since periods near to annual and shorter than 3 months have been filtered out first) can be "significant" at some points, such as on the southern boundary, near 240E. It is not clear what this measure of significance means.

The meaning of significance is clearly defined in the final paragraph of section 2. We define a signal as significant if it passes a significant test relative to an AR(1) process. Several additional sentences have been added to clarify the use of the word within this manuscript.

5. The pattern for the 3-9 month band is described as reminiscent of the PDO, but this is a very superficial resemblance; the variance is high, very roughly, where SST is high

during positive PDO, and low where SST is low, but there is no resemblance between the variance and the expected variance of SST due to the PDO, which would be related to the square of the SST anomaly.

The phrasing of the specific sentence was to try to describe in words the spatial pattern of the signal in this band and because the PDO pattern is familiar to many potential readers, this reference was made. There was no attempt being made to say that the variability was the same as the PDO. The sentence has been reworded to try to clarify this. "The pattern for the 3-9 month band is reminiscent of the Pacific Decadal Oscillation (PDO, Mantua et al., 1997) and the PNA spatial patterns within the central gyre. However, the temporal variability of this pattern does not reflect the PDO. In this example, the central gyre region has insignificant energy at periods less than a year in contrast to the area along the edges and in the southwestern section of the domain."

6. A set of areas was introduced in Fig 1, as the basis for the analysis to follow, but these are then ignored in favour of a new set of regions (described in the text but not illustrated) in which the signal is investigated further.

The reviewer is mistaken. Figure 1 does describe the areas of variability being examined and the text describes the areas and why they are defined. The areas are identified so as to make the analysis easier to follow. The areas are not exact, but general descriptions. The description of the map has been reworded to make this clearer (section 2.1, para. 2).

7. However, the signal which is investigated is not the one mapped in figures 2 and 3, but area averages of sea level over the newly-defined regions. Perhaps the variance in figures 2 and 3 does represent coherent sea level anomalies over some of these regions, but over others it is almost certainly dominated by eddy variability which mostly averages out when a regional average is taken. For example, the high variability in the far north of the region is probably dominated by eddies/meanders in the Alaskan Stream (e.g. Okkonen, GRL,1992; Crawford et al., GRL, 2000).

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It is not clear why the reviewer believes that figures 2 and 3 are not the signal being investigated in the paper. Figure 2 is being used to describe, generally, the overall variance of the SSH signal that is decomposed in the later text. Figure 3 is used to show, in a general way, spatially, where each band has significant energy. The wavelet machinery has been used to create Figure 3, but a similar plot can be constructed using FFT spectral methods. Figures 2 and/or 3 are not attempting to describe, necessarily, coherent spatial structure within a band, rather, they describe, spatially, the areas where there is a significant level of energy for that band. I agree that the high variability in the area of the coast can be eddies and meanders, however, any eddy variability that averages to zero over the area is not of concern in this paper. In this paper we are interested in the large scale variability rather than the small scale eddy variability.

8. A few conclusions are drawn based on these area-averaged time series, but nothing very solid (is a lag of +2 years any better than -1 year in fig 4b? Is the correlation really meaningful with so few degrees of freedom? It would be more convincing if the 2-5 year band was treated as a whole).

Figure 3 shows that there are distinct areas of variability for the 2-3 band and the 4-5 band. While it is true that the two bands could be combined, part of the work is to describe how these two bands are different. As in all time-series analysis, there is always overlap. The correlations are meaningful, if only to give the reader of how the general banded variability is related in one area to another area. This section, 3.1 also shows how difficult it is to conclusively characterize the SSH variability in a region.

9. Then, in section 3, we switch back to grid point time series and start looking at wavelet spectra (with and without pre-whitening, and with no cone of boundary influence plotted). Given the pre-filtering that has taken place, this really tells us nothing that we could not have learned, and seen more clearly, from plots of the time series filtered at 3-9 months and 2-5 years, preferably with the corresponding atmospheric indices treated in the same way. We basically learn that ENSO has an influence on interannual sea level variability in some regions. Not a surprise.

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The plots in figure 3 also show quite a bit more than can understood from overall averages. They show the interannual differences in the bands, of which not all are connected to the ENSO signal. Section 3 is the descriptive section of the paper and is not trying to make any concrete quantitative conclusions. The "cone of boundary influence" is not on the plots because the processing used a method of extending the time series with the use of cyclic boundaries and the "edge" effect of the boundaries is very reduced (see Torrance and Campo, for further information). A comment (end of last paragraph, section 2.2) has been added to the methodology section to address the boundary influence.

10. Figures 11 and 12 introduce yet another geographical split, plotting variance in certain bands as a function of longitude and time, at selected latitudes. Some interesting signals emerge, particularly at 47N, but the interpretation is again not convincing. Is the propagation seen at 4-6 year periods really significant?

As pointed out above the significance is relative to an AR(1) process, so the wavelet decomposition is explaining significantly more information (at the 95% level) than an AR(1). Only the significant signal is shown.

What does it mean in terms of actual sea level time series?

not sure how to interpret the question? The signal analyzed is from a sea level time series after the removal of tides.

The eastward propagation in the plot is at a speed of about 25 degrees of longitude per 8 years (about 0.75 cm/s). It is suggested that this is due to advection, but in order to add support to this suggestion the author turns in figure 13 to the propagation of velocity anomalies along this latitude. It is not clear how that supports the advection idea, but these velocity anomalies appear to propagate at a much faster speed of about 10 degrees per 4 months, or about 7.5 cm/s... another unrelated observation (incidentally, it is impossible to tell which direction this propagation is, as the description of lags applied is ambiguous throughout the paper).

The text is clear as to the direction of propagation, given figures 11 and 12. The paragraphs of section 4.1 have been rewritten as to clarify why the velocities are being examined: "To examine how advection might be contributing to changes seen in the spectral decomposition, geostrophic velocity anomalies were calculated from the SSH fields. Using the same latitudes as above, 47° N and 37° N, several time series were extracted from the fields . The annual cycle and higher frequencies were removed from the fields before calculating the velocities. Figure 13a) shows the zonal velocity anomalies at 47N for 205°e E and for 215° E and also the meridional velocities at 47° N, 235° E, each offset between 4 and 6 months. The plot shows the dominant characteristic of the time series as the "event" centered at 2002. The early part of the record, between 1993 and about 2001, shows little variance in the velocities at all locations, however, the general shape of the low frequency after 2001 is similar in all the series. Figure 13a cannot be compared directly to Figure 12c, as the former contains multiple frequencies over a very broad range of frequencies and the later only shows the very low frequency band. Such changes are the result of advection within the ocean and changes also in the large scale atmospheric conditions. This relational information can be used to inform future changes downstream which may not be obvious. For example: at 37° N in Figure 13b), the two curves are lagged by about 21 months, with the western series located at 225° E and the eastern, shifted by plus 21 months, located at 235° E. While the two series at this location appear to represent a similar signal, defined by either an easterly flow (225° E) or southerly flow (235° E) peaking in 1997 and both decaying in time at the very low frequency, there are distinct differences that complicate the use of the series as shown in the wavelet decomposition and add uncertainties to any estimate of using the more western location for predictions at the eastern location."

11. Furthermore, the value of this propagation as a predictor of sea level signals is highly dubious, with quality factors of 0.68, 0.87 and 0.9 where 1 means effectively of no value whatsoever. Given that the time series have been chosen for their apparent predictability, and that even more selection has been applied by choosing only

the portion of the time series deemed to have a clear signal, these values are hardly convincing.

I am not sure what is intended by this comment. I was very careful not to oversell the predictive skill. The paper states "only the first upstream/downstream pair (47 N, 215 E and 47 N, 235 E) gives a strong indication that the upstream condition is influencing the downstream condition. The other two comparisons indicate that the error in using the upstream condition to predict the downstream condition is less certain, but still the error is less than the variability in the observed record at the downstream location." The whole idea of this section is to explain that significance doesn't necessarily imply predictability.

12. Then, in section 4, we come to the definition of indices. These are based on yet more particular positions and regions (again not illustrated). The index supposedly representative of area 4 is described as "centered at 37N, 225E", which is not even in area 4 but on the open ocean side of that area, and is subsequently described as a coastal area (which, again, area 4 is not).

The text contained an error, the location should be 30N, 235E and with the sentence rewritten as: "The three indices are specifically defined as covering 1) 45° N, 210-230° E, 2) a coastal area near 52° N (area 2) and 3) an area nearer the coast, in the California Current region (area 4). How one should define indices is not clear. Broad area indices and those defined by large-scale changes and patterns are appealing, but are based on empirical decomposition of a signal. Defining indices as is done here by first using the spectral decomposition to understand how a signal varies in space and then defining the index based on a representative location removes some of the empirical nature inherent in some of the broad, large scale indices.

13. The index associated with region 6 is now a zonal average over a particular part of 45 N (not 47 N as used before). What the value of these indices is, is unclear. Most promising is the region 6 index, which appears to bear some relation to fisheries

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(although the description of this aspect is confused - is the correlation with salmon catch direct or negative, and the use of "inverse" to mean negative is confusing).

The reason for the larger area index for region six is because of the large area nature of the signal in this area. The word "inverse" has been changed to negative. The text should state that index 2 is at 47°N, not 45. The figure is correct. The figure locations and the text now are consistent.

See the supplement attached for the revised manuscript.

Please also note the Supplement to this comment.

Interactive comment on Ocean Sci. Discuss., 6, 389, 2009.

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