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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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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.

The paper aims to classify sea level variability in the northeast Pacific in terms of different characteristic time scales and representative regions, finishing with the production of three indices of variability and a claim that these may be associated with a degree of predictability and with fish catches in the region.

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 overinterpreted and of dubious statistical reliability.

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.

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 pattern for the 3-9 month band is described as remeniscent 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.

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. 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). A few conclusions are drawn based on these areaaveraged 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).

Then, in section 3, we switch back to grid point time series and start looking at wavelet spectra (with and without prewhitening, and with no cone of boundary influence plotted). Given the prefiltering 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.

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

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 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 (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 net result of all of this is that I have lost faith in any of the conclusions, as there appears to be no coherent story being told here. There are hints of interesting long period variability around 47N, and of a relationship between this and fish catch, although I do not know how new this is (the mode has already been identified and related to wind stress curl). The rest of the paper appears to me just too confusing to interpret. I do not see an incremental route to turn this paper into an acceptable publication. What is needed is to identify a particular result or set of results to claim clearly, and to assemble the evidence to support these results. As it stands, the paper appears as a collection of almost unrelated analyses, with no connecting thread.

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