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Interactive comment on "Interannual correlations between sea surface temperature and concentration of chlorophyll pigment off Punta Eugenia, Baja California during different remote forcing conditions" *by* H. Herrera-Cervantes et al.

Anonymous Referee #1

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Overview:

This paper presents an analysis of the non-seasonal variability of satellite-measured SST and chlorophyll in the vicinity of Punta Eugenia, quantifying in space and time the correlations between them over a 10 year period, and over separate sub-periods characterized by different forcing processes.

Overall, I found the paper and data interesting and relevant, and on a region that has seen relatively little research, but is an important coastal shelf region, especially locally, but also in the context of the overall California Current region. With the extensive

published analysis of biological and physical variability from higher latitudes in the California Current, I found this to be a welcome compliment. In addition, the study area represents one of the largest shallow shelf regions in the California Current region and so is in many ways unique.

However, I have a large number of concerns in both data preparation / analysis and in the presentation. I then found a large number of smaller details that I feel should be addressed, including a better job of relating the results to other satellite work, especially that off Southern California, was warranted. Finally, I urge the authors to get the assistance of a native English editor to help with the final text/grammar, as there are many confusing sentences that hinder their message, which I do not comment on further here.

Broader Questions / Concerns:

The authors take 9km resolution ocean color satellite data and subsample it to the 4km resolution of their SST data. This is exactly opposite to what I would recommend for any research using multi-resolution data sets. Why didn't they use 4km color data? Or scale the SST data to the color data? At the very least, they should present their sub-setting approach and its ramifications for space patterns and coastal resolution in their results. They then use a different, 18 km resolution, color data product for one of their figures. Why not use the 9 km data and be consistent? And then, the non-seasonal portion of this new signal was calculated in a different way than the 9km data anomalies, introducing unknown, and not discussed, differences into the 2 series of anomalies.

The authors should discuss their approach to calculating statistical significance in their correlations; specifically how many degrees of freedom they have, given very obvious dynamic autocorrelation in the time series (Figure 4), and their own imposed 3 month smoothing of the signals. This is of special concern in the subsets they correlate; e.g. the El Nino period Sept 1997 – Dec 1998 is 16 months, with 3 month smoothing provides, at most, n=5 independent data points, and that's without considering underlying dynamic autocorrelation. How many effective degrees of freedom are in these correlations?

No discussion is made of the extent to which the standard NASA ocean color band-ratio algorithms for chlorophyll are valid in this region, especially on the shallow continental shelf area of Bahia Sebastian Vizcaino.

Some Details: An important (and unique!) aspect to the study area is the large shallow shelf region, and many readers, myself included, would like to compare the satellite patterns to features in the shelf bathymetry… yet the color scale in Figure 1 shows only the extremely deep areas and provides absolutely no detail on the shelf at all. In fact the only bathymetric information of use is the location of the shelf break. I'm also curious about the ∼1000m deep canyon that appears to intrude within a few km of the coast in the very far southeast corner of the Figure at 113W. I suggest rescaling the bathymetry information to show details on the shelf and checking this canyon.

The above aspect is important, as the authors state (Abstract, line 28) that wind stress explains the large 2002-03 chlorophyll anomalies, but an interesting aspect that is not discussed is the extent to which the wide shallow shelf plays a role.

The authors have a section labeled Results and a separate section labeled Discussion, yet many items and ideas that are clearly "discussion" are presented in the Results section. [e.g. pg 859: Lines 22-24 "….California Undercurrent, etc" , pg 861: lines 5-10 "cold and fresh intrusion… etc…", lines 18-21 "coastal trapped waves… etc", pg 863: lines 8-11 "conditions could be associated with…..", lines 25-28 "….subarctic water masses", pg 864: lines 3-6 "Biological Action Centers…"]. These should be moved to Discussion. None of these items can be seen in their results, they are discussion points in comparison to other published papers…. This is especially obvious on page 859, where they state that the observed variability is due to interaction between water masses. This is clearly not something that can be seen in their data. I also think this

is likely incorrect, as I think I am looking at a standard deviation calculation done on seasonal data (this is not made clear), so this is just showing the strength of seasonal cycles??? Another possibility is to combine their results and discussion into a single section called "Results and Discussion" and rewrite some of the presentation. This might be beneficial; as I found some of the existing discussion simply restated what had already been presented in the results.

I don't think Figure 3 is necessary. Simply state in the text the % variance explained by Modes 1-3 (only mode 1 is discussed and presented in the paper) and say that modes 2 and 3 were not statistically separable.

I am confused about the author's description of the "joint SST_CHL EOF". In the Methods this is described as "forcing them to have the same temporal variability", which is what I expect (a space pattern for each, and a single time series). Yet in Table 1 there are separate correlations for both SST and CHL resulting from the joint EOF. But then we are told (pg 861, line 13) that the time series for the joint EOF is identical to the individual EOFs, so they are not shown. If a different space pattern emerges (Fig 5 a,b from Fig 4), how can the time series be identical? There is clearly something that I do not understand that needs to be more fully explained/clarified.

It is not explained where the data plotted in Figure 7 were subsampled from in comparison to the study area (Fig 1). More importantly, there is so much latitudinal coherence in the data shown that I question the need for even presenting the data as a Hovmoller plot. One, maybe two, simple line graphs as time series would make the point more effectively. In fact, none of the smaller features that do show up in the Hovmoller plot are discussed. I would also suggest labeling the parts of this plot a, b, c.

The results presented for Figure 8 are not clear (pg 863, lines 15-21). Specifically, "although the study area was affected by oceanographic conditions of subtropical origin…." How do we see this in Figure 8? "….could be associated with remote forcing of northern origin…" How do we see this in Figure 8? Also, what do the westerly (onshore) anomalies in wind imply for upper ocean dynamics and chlorophyll in this region?

pg 865 Line 2: "suggests that CHL, unlike SST, is more influenced by events of northern origin. . ...", but Figure 7 clearly shows very large CHL anomalies during the ENSO cycle of 1997-1999. . .. Larger than those in the 2002 period.

Discussion: Although the authors discuss their results in comparison to other work, there were previous systematic analyses of satellite data time series for this region they do not compare their results to, which I found surprising. Specifically: many papers by Kahru et al. (e.g. 2009, 2012) investigate trends in primary production and chlorophyll (can the trends shown here be compared?), and their 2012 paper shows frontal activity in this region, the work of Thomas et al. 2012, relates SST and CHL anomaly patterns includes views of this region, and Espinosa-Carreon et al 2012 investigate the role of mesoscale variability in controlling CHL patterns in the deeper regions where the authors show strong weights in their EOFs.

Smaller more specific details:

Pg 855: line 7 ". . ..interact at a global scale". . .. This is not a global feature, incorrect terminology. Line 27 "milliondollarsyr-1" this can be written much more concisely

pg 856: line 17. I do not think that the entire 1997-2007 period can be considered an El Nino period

pg 858 line 26 and episodically after: I recommend not inventing a new acronym (HGs). . ... just write out the word. I further note that this acronym is changed later into the manuscript.

Pg 859, the methods present the approach to calculating non-seasonal data, yet this first figure (Figure 2) presents seasonal data (I think). I do think it is interesting and informative, but it needs a better introduction, caption and description as we are never told this, and it is not what a reader expects after reading the Intro and Methods. Then

I note it is never mentioned again, not in Discussion, Conclusions and not in Abstract.

Pg 860 line 4: What is meant by "significant overlap" between these climatological seasonal cycles?

Line 11-18: It was disconcerting to present correlations of the EOF principal components and various forcing metrics BEFORE being shown what the EOFs looked like. I suggest presenting the EOFs (Figure 4) and then presenting their correlation to other items.

Line 21: says the CHL EOF has its sign reversed, yet the space pattern is mostly positive, and the time series negative during the El Nino. . .. suggesting negative anomalies during El Nino which is what I would expect. It is not clear what was reversed? Are anomalies in this area actually positive in the EOF during El Nino?

pg 861 Line 29 "implying increases in sea level" (this is discussion, see above comment), but also might imply increasing stratification. . . in fact, in these data the two cannot be separated.

Figures 4 and 6: none of the contours are labeled. The reader cannot interpret them properly. A description in the caption is not sufficient.

Figure 4. It is not clear how the colored shaded regions are defined. Are they necessary?

Figure 4: It is not clear what the purpose of the trend lines are. And what does a trend in the MEI mean over this relatively short time period, especially when the time series starts with one of the largest El Ninos on record? This also raises the larger question (see degrees of freedom, above) about the extent to which correlations are due to similarity of overall linear trend.

Figure 5c: the sign of the correlation is not given on the color bar and the color scale of the correlation does not compliment (illustrate) the variability in the data well.

Figure 8: The latitude tick marks are evident on 8a, but do not line up with ticks in 8b, and then the map projection of 8c is very strange such that the Southern California Bight appears to be at 35N.

Figure 8: What are the brown shaded regions in 8a? Also, the relationship between the wind anomalies and the CHL is not obvious to me. CHL anomalies in the study area appear to start earlier and last longer than wind anomalies. Wind anomalies early in the year in 2002 are too small to see at this scale.

Figure 8 caption: chl units should be should be mg m-3. Spelling: "Flight".

Pg 862 line 16. "all in the deep zone". But Figure 6b clearly shows very strong signals throughout the shallow area of Bahia Sebastian Vizcaino as well.

Line 18: "mainly in the chl mode", but it seems as though it is strong in the SST as well.

Line 29: 'but negatively correlated with the MEI", …. not in the last 18 months.

Table 1: is a triangular symmetric correlation matrix. I think it would help readability to only present half of the table. Also, … check entry for MEI-MEI correlation.

There are many small items presented many times, creating unnecessary repetition. (e.g. we are told 3 times that modes 1 of the SST and CHL EOF explain 78 and 45% of the variance).

Summary: Despite many difficulties, as outlined in the overview, the paper has the potential to be an important contribution and I hope the authors will address the above concerns.

Interactive comment on Ocean Sci. Discuss., 10, 853, 2013.