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Ocean Science Discussions
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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.

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Responses to main assessment from the Anonymous referee # 1 Broader Questions / Concerns:

1. The authors take 9km resolution ocean color satellite data and subsample it to the 4kmresolution 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 nonseasonal 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.

Attended: We decide to standardize to 4km both variables (SST and Chl-a) to have a better resolution near to the coast. In the case of the sub-setting approach and the ramifications for space patterns and coastal resolution since 22°N to 45°N, we needed a better time resolution in Chl-a to cover a relatively short period of time (2002-2003). We decide to use 8-day weekly average joint with its weekly climatology to calculate the 8-day anomalies of the SeaWiFS Chlorophyll concentration and re-binned into 18x18km, this we seemed adequate to cover an extensive line coast (∼23 degrees of latitude). For the wind, we maintained the original temporal resolution (months) and the original 0.25°x0.25° spatial resolution was re-binned into 18x18 km to have the same spatial resolution of the Chl a. For a better understanding, we changed the final text in the subtitle 2 Data and Methods

2. 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?

Attended: We calculate the effective degrees of freedom joint the statistical significance. For a better understanding, we modified the fifth paragraph in the subtitle 2. Data and Methods and Figure caption 3 and 4 .

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3. 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 Vizcaíno

Attended. We modified the first paragraph in the subtitle 2. Data and Methods

4. 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.

Attended: We detected a mistake and rescaling the bathymetry information (see new Figure 1). We modified the paragraph in the subtitles:. Introduction and Results.

5. 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

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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.

Attended: Results and Discussion were re-write in response your observations

6. 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.

Attended: We agree with the reviewer’s suggestion. Figure 3 was removed from the document.

7. 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

Attended: We re-write the paragraph in subtitle 2. Data and methods and we re-write the Table 1

8. It is not explained where the data plotted in Figure 6 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 HovmÓğller plot. One, maybe two, simple line graphs as time series would make

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the point more effectively. In fact, none of the smaller features that do show up in the HovmÓğller plot are discussed. I would also suggest labeling the parts of this plot a, b, c.

Attended: We appreciate the reviewer′s suggestion but the subject to build Hovmöller diagrams based on time series of coastal SST and Chl-a anomalies data was to observe if the different ENSO signals showed changes along the coast of the study area and how both variables represents the subartic water intrusion within the ∼30 km closest to the coast. We labeled the panels with a, b, c.

9. 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?

Attended: We re-write the paragraph about Figure 8:

10. 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.

Attended: We re-write the first paragraph of section 4. Discussions:

11. 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

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of mesoscale variability in controlling CHL patterns in the deeper regions where the authors show strong weights in their EOFs.

Attended: We include references suggested by the referee.

12. 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.

Attended all the observation. We re-write the paragraph of Section 1. Introduction, 2. Data and Methods and the Figure caption #2:

13. 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.

Attended

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

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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.

Attended: We modify Figures and Figure captions

14. 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.

Attended: We modify Figures and Figure captions

15. 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.

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Attended all reviewer's suggestions:

16. 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).

Attended all reviewer's suggestions:

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/10/C362/2013/osd-10-C362-2013-supplement.pdf

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

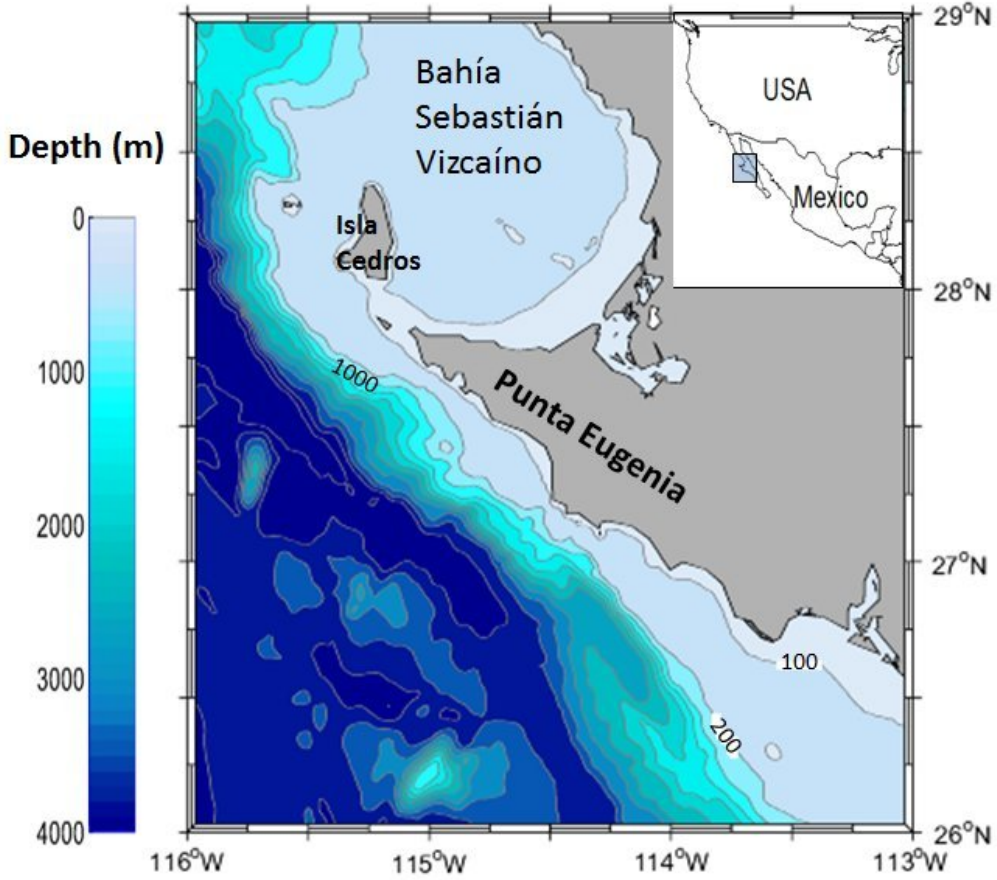

Fig. 1. Figure 1. Location and bathymetry characteristics of the study area.

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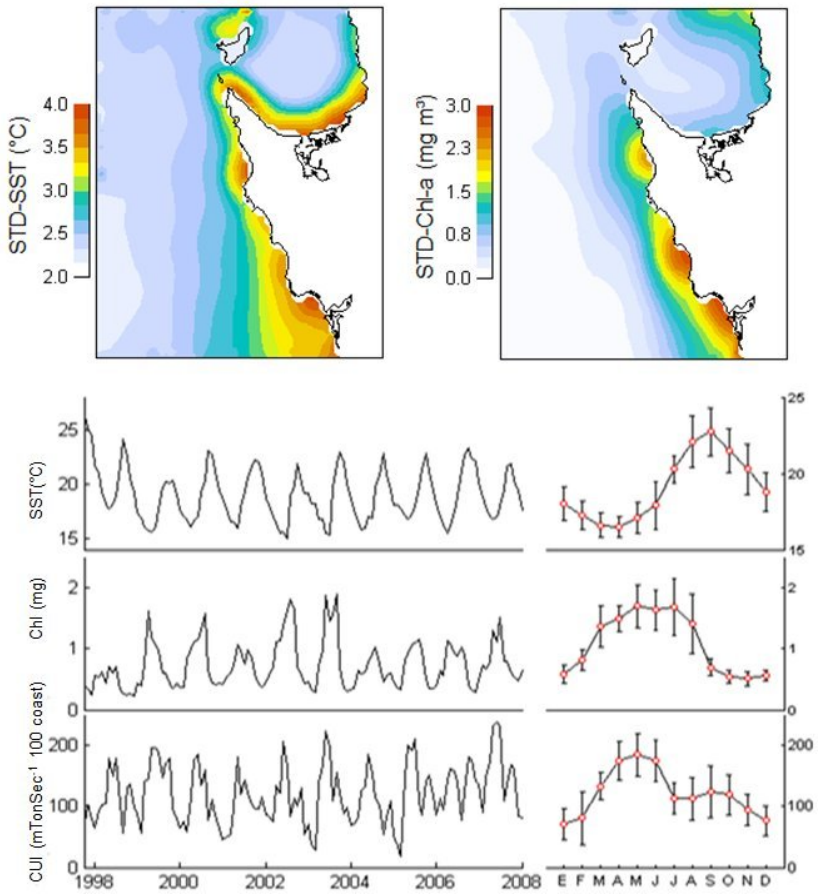

Fig. 2. Figure 2. Surface plots of standard deviation variability (time averaged) and monthly time series (spatial averaged) of the satellite-derived SST and Chlorophyll a compared with the monthly CUI time s

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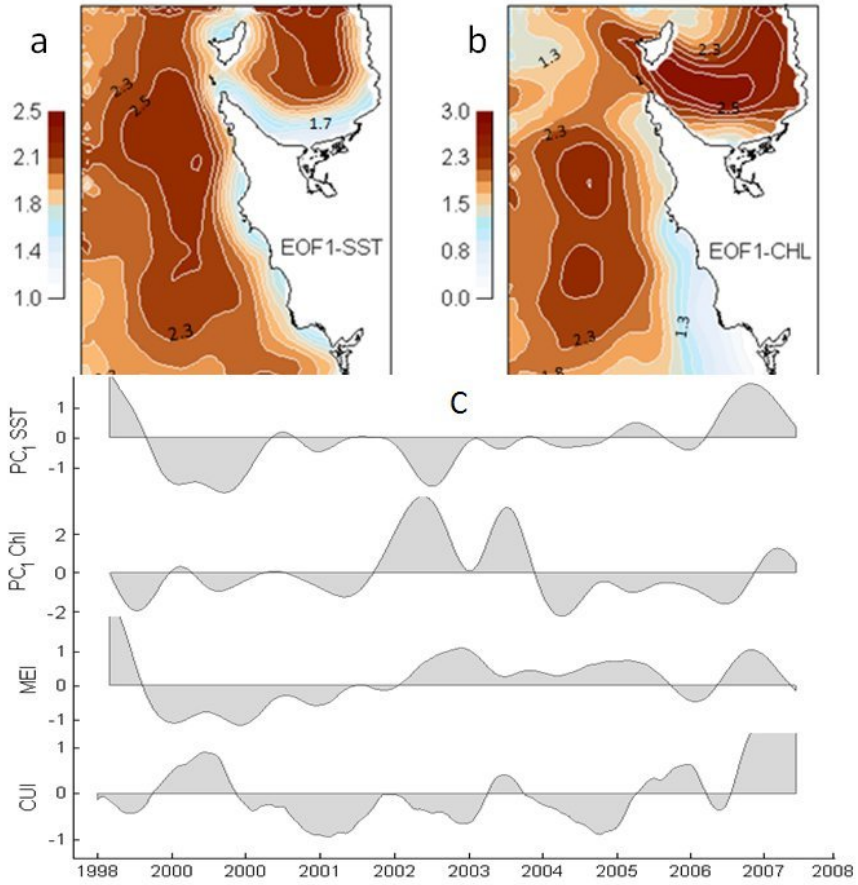

Fig. 3. Figure 3. Spatial patterns for mode 1 of the individual EOF analyses. (a) SST, (b) Chlorophyll a. This mode accounts for 78% and 45% of the total variance for SST and Chlorophyll a (with the sign rev

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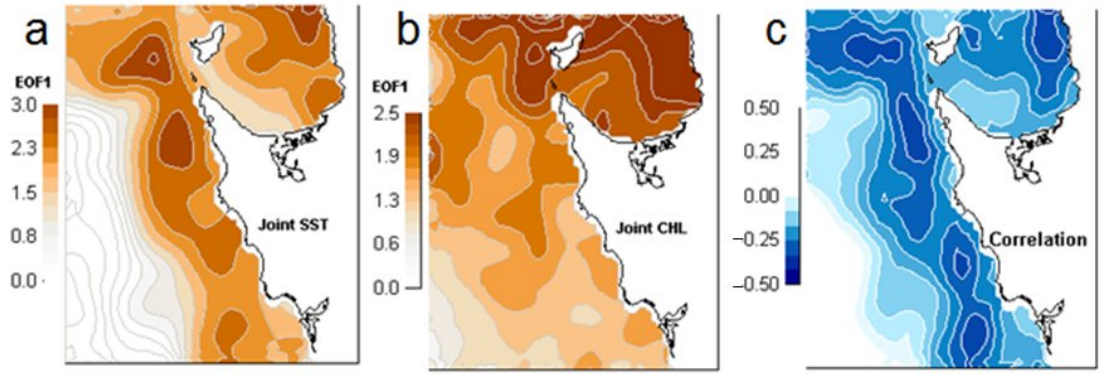

Fig. 4. Figure 4. Spatial patterns for mode 1 of the joint EOF analyses of (a) SST, (b) Chlorophyll a (accounting for 80% of the total variance) and (c) correlation map between monthly anomalies of SST and Ch

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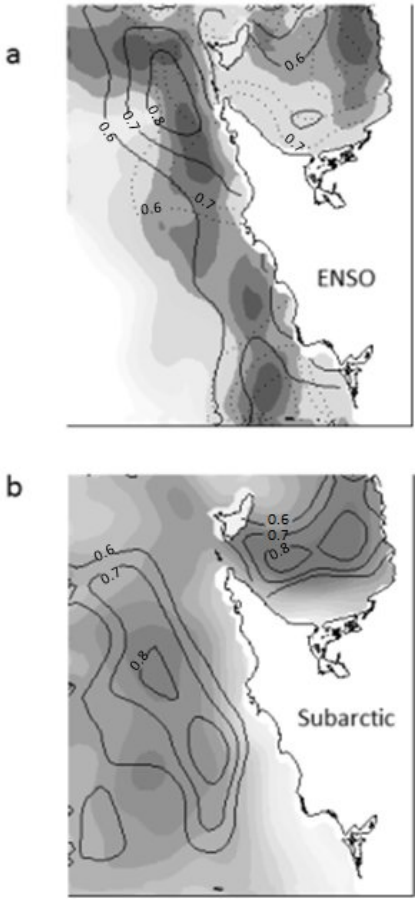

Fig. 5. Figure 5. Contours of mean homogeneous correlation overlaid on (a) the correlation map for the El Niño and La Niña period (solid and dotted contours) and (b) for mode 1 Chlorophyll a for the intrusion

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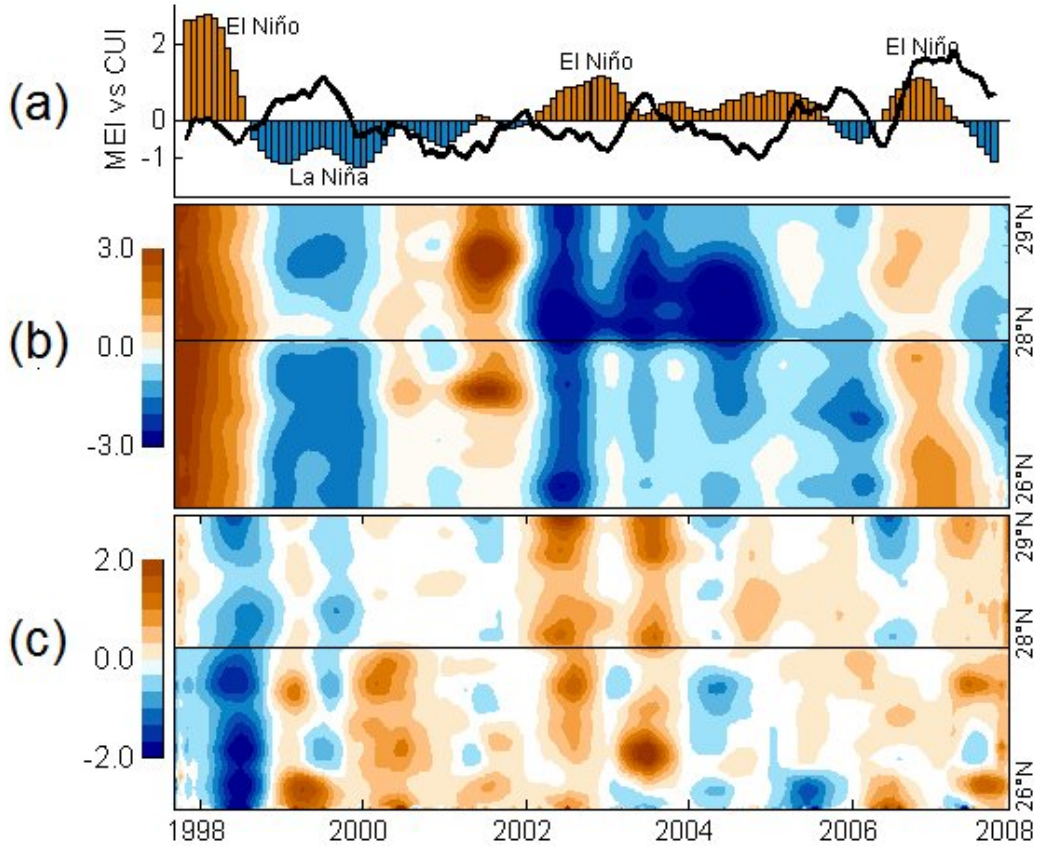

Fig. 6. Figure 6. Temporal evolution of (a) the MEI and monthly CUI anomalies. El Niño (La Niña) episodes are indicated by orange (blue) bars respectively as reported by the Climate Prediction Center of the N

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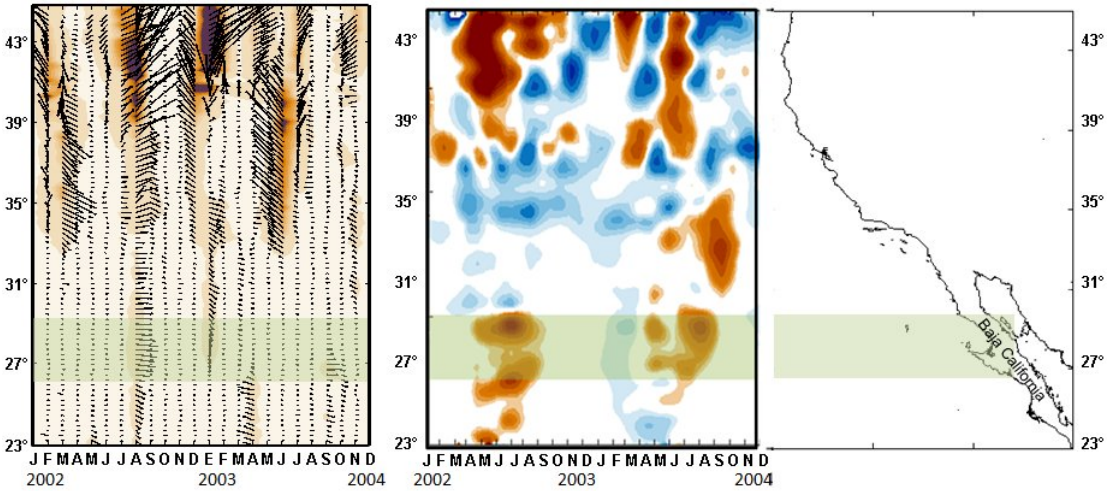

Fig. 7. Figure 7. HovmÓğller diagrams of interannual anomalies of (a) monthly wind stress (magnitude and direction; N m-2 x 10-2) and (b) weekly Chlorophyll a (mg m -3) from January 2002 to December 2003 alon

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