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Discussions

Interactive comment on "Contextualizing time-series data: Quantification of short-term regional variability in the San Pedro Channel using high-resolution in situ glider data" *by* Elizabeth N. Teel et al.

Anonymous Referee #1

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The manuscript by Teel et al. present a dataset of T, S and chlorophyll-a obtained from glider crossings in the San Pedro Channel, in the Southern California Bight. The channel is home to the San Pedro Ocean Time Series (SPOT), a long running oceanographic station that is sampled about once a month. Teel et al. analysis of the glider dataset suggests that SPOT profiles are overall representative of the SPC, with conditions ranging from oligotrophic deep chlorophyll maxima similar to offshore waters (by far the dominant pattern), to post-upwelling surface blooms. Notably, the glider data shows very weak correlations with satellite-based estimates of the surface chlorophyll,

raising doubts about reliability of satellite data in the region. Ocean time series have been fundamental in advancing our understanding of ocean physics and biogeochemistry. However, they are localized in space, and sampled at most at monthly frequencies, raising the question of how representative they may be for broader regions, and how much high-frequency variability they may miss. This is a critical question in a region like the SCB, where complex circulation patterns, including upwelling, mesoscale eddies, island wakes, sustain variability on a range of timescales. Thus the work by Teel et al., is a welcome attempt at characterizing variability at a time series in relation to larger scales. I am of two minds about the paper. The dataset presented is of good quality and potentially useful in elucidating physical and biogeochemical variability in the region. In fact, I encourage the Authors to make the data available for the community. The decomposition of this variability into representative modes is also a useful insight. However, some aspects of the methods, the presentation of the results, and some parts of the discussion are not very clear, and made for a difficult and often opaque read. I encourage the Authors to work on a better synthesis and explanation of their results.

General comments:

Footprint of SPOT data. The paper wants to make a broad claim about the variability at SPOT, but the glider dataset has itself quite a limited footprint, extending for one 28 km between the mainland and Catalina Island. This is scarcely representative of the broader Southern California Bight, and it would have been nicer to have a broader sampling, or comparison with a broader set of observations. This is especially important in light of few of the main aims of the paper, e.g. addressing local vs. regional drivers of variability (abstract, line 19), and determining the spatial domain of a time series (abstract, line 28). I feel that more effort could have been made to discuss how the study resolves these questions, at least for the SPC. After reading the paper, I am not sure I have a clearer idea of the questions.

Actual SPOT data. While the data discusses at depth the representativeness of the

SPOT time series for the context of the SPC, actual profiles from SPOT dataset are not used, but only glider data that pass through the SPOT station. I think there is a missed opportunity to for a reanalysis of the SPOT data in light of the information provided by the new glider dataset. With ∼monthly sampling, nearly 20 years of SPOT data should contain ∼100 profiles (for March-July periods) that could be easily couched within the variability identified by the glider profiles. Do they all fall within the range of variability observed here? Are the frequencies of the different modes observed in the SPOT data in agreement with their frequencies from the glider data analysis? It would be interesting to know if there are outlier in the SPOT data, which may suggest perhaps importance of inter annual variability.

Definition of "end members". The separation of the variability into main modes is a good idea, but I have some criticisms on the way it is conducted and presented. The modes are identified a priori in a somewhat arbitrary way, which is not very well described. I would think an objective (i.e. replicable) approach could have been running a PCA of the entire dataset, then extracting the main axes of variation and use extreme values to define "end-members". Here it seems the Authors qualitatively selected 54 profiles, then identified PCA for them, and projected the entire dataset on the resulting PCA. (In fact, I think the entire methods are not clear, and deserve a dedicated, more detailed section, which could go in the Supplementary Information.) Now, I think expert judgment is often a reasonable approach, but more discussion of the rational between end-members and their translation to the whole dataset should be presented. Also, by looking at Fig. a, the "offshore influence" mode could be considered, rather than an end-member, somewhat a mixture of "early upwelling" and "deep chl max" based on PCA values. Is it really an end-member?

I also have a quibble about the names of the end members: I kept confusing "offshore influence" and "deep chlorophyll maximum". The two names make me both think of offshore oligotrophic subtropical conditions, and it took me a while to realize that "offshore influence" is in fact in between offshore oligotrophic and coastal influences. The

"deep chlorophyll maximum" end-member is in fact more representative of the offshore regions than the "offshore influence". Maybe a better naming strategy can be found.

Comparison between glider and satellite-based estimates of surface chlorophyll-a. This is extremely interesting, and may contain some of the most relevant implications of the study for a broader community. The mismatch between surface glider data and satellite retrieval is glaring (supplementary Fig. S4), and suggest that satellite data should be taken very cautiously in the nearshore SCB, or even completely discarded as a reliable source of information on phytoplankton distribution. The Authors even state in line 278 and in the caption of Fig. S4 that "no correlation was observed between glider and satellite derived integrated chlorophyll": this result seems important enough to require a dedicated figure, at least in the Supplement. At the same time I am not completely convinced of the strength of the Authors' comparison. The Authors do not really get to the bottom of the mismatch, and some of the hypothesis that they put forward don't seem to be able to explain it, especially in light of the systematic variation shown in Fig. S4. I suspect some systematic mismatch in the optical depth over which the glider data should be integrated to provide comparison with the satellite data may be behind the discrepancies. Also, are satellite algorithms really only representative of the first optical depth? Given the exponential nature of light-attenuation in water, perhaps satellite retrievals of ocean color may be representative of a somewhat deeper water column. I think the Authors identify an important issue, but I am not sold it is time to start ignoring satellite retrievals of chlorophyll-a in the region.

The use of the concept of "connectivity" for both horizontal and vertical similarities is somewhat misleading. The fact that inshore and offshore profiles may be similar doesn't necessary imply a direct, material bath connecting the two, as the word connectivity implies, but they may be just responding to remote, synchronous variations that occurs at scales large than few tens of km sampled by the glider. I suggest using a term different than connectivity throughout the text, especially section 3.2 and 3.3. Perhaps "coherence" or "similarity" would be more accurate.

Some of the results could be less vague and speculative, and more quantitative. For example, Line 150 “given sufficient sampling, SPOT data could be representative of the average state of the SPC”: can “sufficient sampling” be actually quantified? Similarly, line 355 in the conclusions, “higher frequency sampling”: can this be quantified based on the new data? Would weekly sampling be needed? or daily? Specific comments:

Line 70, “recurring membership”: please clarify or rephrase the term.

Line 92, “that was perpendicular to the mean flow”: add “approximately”

Line 105, calculation of BVF. This requires a vertical derivative, which dramatically increase the noise in the resulting variable. Was any smoothing applied to the data to reduce the noise?

Line 112, conflation of depth of 12.5C isotherm and nutricline. I wonder if this relationship could be tested with SPOT data for the region. Are nutrients measured at SPOT? How well does the relationship hold there?

Line 126: the “and” after “data” seems out of place.

Line 141-142, “This tilt is consistent with equatorward flow through the channel”. This statement is at odds with a generally poleward flow in the very nearshore band and in the SPC, which tends to bring waters from the southern bight, and is embedded in the Southern California Eddy. The predominantly equatorward California Current is much more offshore.

Line 170, “seasonal traits” clarify o rephrase.

Line 208-209, “we cannot distinguish between local and remote sources”: this is at odds with the previous sentence, and with the general notion that the glider data allows a characterization of the spatial domain of the time series data. Please clarify.

Line 214, “maintained an onshore-offshore gradient on average”: please clarify the sentence.

Line 244, “highly offshore characteristics (Figure 6a)”: this seems to refer to the wrong panel, please double check.âĂÍ Line 249-250: this sentence seems to undermine the idea of connectivity as a material path connecting inshore and offshore.

Line 288, “bloom thickness”: defined how?

Line 321, “decreased sedimentation”: please clarify.

Line 331, “events”: the term doesn’t seem appropriate for the modes considered, especially deep chlorophyll maxima which seem the common “background” state for the SPC. Please clarify.

Figure 1. Some bin numbers could be useful, e.g. 10,20,58, since they are used later.

Figure 2. The mean mixed layer could be shown on this figure too for the 4 end-members.

Figure 3. It would be useful to have a measure of the distance from the Palos Verdes Peninsula together with the bin numbers in the x-axis. Also, it would be useful to add the location of SPOT in the panels. The caption should also specify the months of the observations, besides the years.

Figure 4. Are two panels really necessary? It seems that all information of panel b could be contained in panel a. Also, how were the ellipses defined?

Figure 5 is a bit messy, and could go into the Supplement. It is not very successful in showing a clear seasonal progression or cycle, like the similar figures in Jacox, 2016 that presumably inspired it. To me the message is that the potential seasonal progression of upwelling is swamped by high frequency variability. (Or that perhaps the 2 PCA axes are not well positioned to highlight this progression.)

Figure 7: I wonder if the ratio between surface and total integrated cha may be a better variable to show here.

Supplementary Table S1: many of the thresholds and combinations behind the end-member definition seem somewhat arbitrary, and could be better justified, e.g. with a dedicated Supplement section.

Supplementary Figure S2: what are the vectors on the plot? Please explain in the caption.

Supplementary Figure S3: please correct the units in the caption of panel a, from ug to mg/m3.

Supplementary Figure S4: the legend of the figure states “Satellite:Glider” mach-ups, but the labels and caption state “Glider-Satellite”, please clarify.

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