Overall author response: We thank the reviewers and associate editor for careful consideration of our manuscript. We address all reviewer comments below. For ease, we have copied the reviewer comments below. **Original reviewer comments are in Arial font** and *author responses are in Times New Roman italicized.*

Interactive comment on "A year in the life of a central California kelp forest: physical and biological insights into biogeochemical variability" by David A. Koweek et al.
Anonymous Referee #1
Received and published: 4 October 2016
General Comments:
This study presents 1+ year time-series data of weekly samples of carbonate chemistry across a small spatial scale of a kelp forest covering two summer seasons. The data include surface and bottom samples in exposed and protected sites and from inside to outside the kelp forest. The data are of extremely high quality. The paper is well written, articulate, and has logical organization with nice transitions. While carbonate chemistry time series papers are increasing in number, this paper contributes novel and valuable data on small scale spatial variability (depth and spatial). In support of publishing this paper, I consider my comments as minor revisions which would improve the scientific quality from 'good' to 'excellent'.

Thank you for your thoughtful comments and careful review of this paper. We address your specific comments below.

Specific Comments:
I have three specific comments, two with regard to the spatial variability. First, bottom water sampled by site is confounded by depth, which is not explicitly addressed. The spatial variation of bottom water could just be an artifact of the stratification of the water column within which the kelp forest sits (deeper waters have more DIC, so therefore bottom waters of deeper sites will have lower DIC values than bottom waters of shallower sites). The potential depth dependency of the observed dynamics (and conclusions) should be addressed and contextualized with the aims of the study (and the sampling design of surface and bottom waters, which was not explained). The data are valuable in terms of understanding the variation of what, for instance, a benthic kelp forest inhabitant might experience, but then that perspective should be included (Introduction and Discussion).
We agree that the bottom water sample variability is confounded by depth variability. We will make this point more explicit in a revised manuscript. However, we still see robust differences between bottom water samples at similar depths, which hint at other localized processes controlling biogeochemical variability, something we already discuss in the manuscript.

Second, the most valuable portions of this study are the depth gradient (well developed and presented) and the spatial gradient of the time-series (from inside to outside a kelp forest, exposed to protected). The presentation of the latter (Section 3.5) is extremely short and the figures comprise mostly of statistical numbers and not meaningful observations. The authors do themselves a disservice by not highlighting this aspect of the study more in depth. Figures 9 and 10 do not contribute anything that could not be shown in a table (Fig. 9b, 8, and S8 display duplicate data in every plot). Fig. 9a could be interesting if shown as a line graph (bar graph is too cluttering) but I don't think it's necessary in the first place. Instead, I was expecting a figure showing the gradient in carbonate chemistry from inside to outside the kelp forest at the two contrasting sites (protected, exposed). How does these gradient change by season? It looks like the largest spatial differences occur between the exposed vs. protected site and not within the inside vs. outside (I suspect that differences between inside and outside kelp forest will only be apparent with higher frequency sampling). The statistics show this, but the figure space would be better used by using the real data (e.g. select parts of the time-series, moving averages, etc.).
We will re-organize Figure 9 to move the current Fig. 9B to Fig. 9A. This will help highlight the clustering of the Pro Inn and Pro Mid sites relative to the other four sites. We will move the current Fig. 9A to 9B and modify the figure to show the histogram of aragonite saturation state observations for: Pro Inn + Pro Mid and the other four sites (represented as two

clusters). This will help better illustrate the spatial differences between Pro Inn and Pro Mid relative to the four deeper sites. We will augment the text to describe this clustering in more detail. We will also provide additional text to better clarify the unique and complementary contributions of the new Fig. 9 and the current Fig. 10 (likely will remain unchanged). Specifically, Fig. 9 shows differences in average conditions, whereas Fig. 10 shows differences in co-variation between sites.

Lastly, the Discussion is largely devoted to the value of time-series, this could probably be condensed. As an edition, the results should be discussed in terms in the context of other studies of kelp forest or coastal variability in general (some were mentioned in the Introduction). Do these data fall within the range of biochemical observations made previously in other kelp forests?
We will look for ways to condense the Discussion and add a short paragraph comparing our dataset to the limited available data sets.

Other and Technical Comments:

Shorten the LPJPSMR acronym
We will remove this acronym

2.1 L23: of kelp of the kelp
We will correct this error

2.4 L6: provide reason or reference for phosphate assumption
We made phosphate measurements from February-August 2014. We did not discuss these in the manuscript, but they are included in the data set provided in the supplementary material. We will indicate that our phosphate concentrations used for carbonate system calculations come from our own measured, but not presented, data and that the reader can refer to the supplementary material to see the data.

2.4 L8: pHT is defined but not used in subsequent reporting of pH values in the Results.
We will remove the single reference to pH_T *as the Methods section already indicates that all pH data is on the total scale at in-situ temperature.*

2.4 L11: double))
The double)) was used because the reference was cited within a parenthetical statement. We will use brackets for the reference inside of the parenthetical statement.

For all time series figures: simplify x-axis date labels. Adding 01 as the day is not necessary and adds clutter. I recommend to simply label months as 1-12.
We will simplify the x-axis labels on the time series plots to only show the month

3.2 L23: "causing water column temperature differences of up to 4_C" add across what range of depths
We will include the depth of the Protected Offshore site in this sentence

3.3 L 16-17: "Surface DIC concentrations were generally much more spatially homogeneous than bottom water DIC concentrations." Could just be function of depth.
We agree that much (but not all) of the bottom DIC differences can be attributed to depth. We still think that much of the surface homogeneity is due to the strong biological influence in the surface waters, as we discuss in the Discussion section.

3.3 L25: has should be had
We will correct this error

3.3 L27: regarding pCO2 undersaturation, add "with respect to the atmosphere" if that is what you are measuring saturation against.

We will add this clause

Table 2: Since bottom depth differs across sites (7.5-16 m) and there are obvious depth effects, add the depth in m in () following the listed value in Table 2.
Depths for all study sites are already listed in Table 1

3.4 L21: "drove large variations in the ability to buffer against ongoing ocean acidification". Ocean acidification is not detectable across such a short time-series. Reword to simply say, "drove large variations in the Revelle Factor".
We will replace the sentence with the reviewer's suggested sentence

3.5: Why was aragonite used here (and not TA or DIC)?
We chose to use the aragonite saturation state because we thought this carbonate system parameter would be more familiar and more easily comprehendible to a broad suite of scientists working on ocean acidification and global change. We note that section 3.4 establishes that DIC variability is the dominant driver of observed variability in aragonite saturation state.

4.1 L28: Regarding this paragraph, it would be nice if you could find a reference showing the seasonality of phytoplankton blooms of this site. I imagine it is offset from the kelp forest growing season.
This would be a nice addition, but we do not have the data. As we identify in the Discussion section, this is a hypothesis for future work and points to the need to have an offshore control site against which to compare the nearshore biogeochemical variability.

4.2 L22 - State the actual findings. The largest source of variation seems to be the protected vs. unprotected sites, which is actually a function of the oceanographic features, not a function of the biology of the kelp forest. The biological control is in this study is the depth gradient (where primary production takes up DIC at the surface).
We hypothesize that oceanographic processes control the bottom waters, but that biological processes control the surface waters (at least during periods of high primary production). We discuss this hypothesis, along with the supporting observations, in Section 4.1. The paragraph that you reference serves as a starting point for discussing the implications of the work. Further discussion of the mechanisms behind the observed variability would be redundant from the previous section.

Pg. 11 L9: inconsistent use of OA vs. ocean acidification. I recommend to not use the acronym at all.
We will remove all uses of 'OA' and replace with 'ocean acidification'

Pg 11, L10: other studies have shown this previously also: pH sensor-based studies in coastal environments but also cruise data for offshore regions. Cite references in support of this conclusion.
We respectfully disagree with the comment to include offshore references. This study is explicitly about a nearshore ecosystem. We already openly acknowledge the limitations of not having offshore data in our study. Discussing other's offshore observations of primary production is outside the scope of this manuscript.

Pg. 11 L13: The ocean is not acidic, acidifying and acidic are different.
We will replace with 'acidifying'.

Pg. 11 L16: Same as previous comment. It makes more sense to use 'low pH' instead of 'high acidity'
We will replace 'high acidity' with 'low pH'

Interactive comment on "A year in the life of a central California kelp forest: physical and biological insights into biogeochemical variability" by David A. Koweek et al.
Anonymous Referee #2
Received and published: 10 October 2016
This manuscript nicely describes spatial and temporal variation in carbon system variables in a kelp forest in Central California. The data presented are the first to report high frequency measurements of carbon system variables made at small spatial scales across an entire annual cycle within a kelp forest. The data reveal substantial depth-dependent, spatial, and seasonal differences. The authors suggest mechanisms that could be responsible for creating the observed variation. Sampling design, sample collection, and analytical methods all appear appropriate to the research questions posed. The organization of the paper is logical, the writing is clear, and the graphics are appropriate and informative. Below I offer specific comments intended to strengthen the manuscript.

Thank you for your thorough and considerate review of this paper. We address your specific comments below.

Page 2, line 8: more clearly stated as "Calcification and dissolution of kelp-associated organisms, especially shelled invertebrates, can modify water chemistry. . ."

Our current sentence "Calcification and dissolution modify the water chemistry through the uptake and release of carbonate and bicarbonate ions, which modify the total alkalinity and dissolved inorganic carbon" provides a more complete mechanistic description of the water chemistry changes due to calcification and dissolution than does the suggested sentence. We prefer to keep the main components of our sentence, but will look into whether the sentence can be re-worded to be more concise.

Page 2, line 16: better stated as " Despite the recognized importance of the kelp forest. . ."

We will use this wording in the revised manuscript

Page 2, line 16: I searched but did not find reference to kelp as an "ecosystem architect". The earlier use of the term "foundation species" is more consistent with the ecological literature.

We will replace "ecosystem architect" with "foundation species" in the revised manuscript

Page 2, lines 25-27: Important points are made here. It would be helpful to clearly return to these in the discussion section.

We do return to these points in section 4.2. We will add additional text to section 4.2 to more clearly indicate our return to these discussion points.

Section 2.4 Satellite derived estimates: Estimating kelp canopy biomass is notoriously difficult. The authors have done a good job estimating relative changes in biomass over time but I found no indication in the text or figures to indicate error in this estimate. The inclusion of error estimates would be helpful.

We estimated giant kelp biomass using empirical relationship between diver estimated kelp canopy biomass and Landsat kelp fraction. We have generated 95% confidence intervals about this relationship at each time point, for each site, and will include these error estimates in the revised manuscript.

Section 3.4: Carbon systems variables differ between surface and bottom, consistent with the intrusion of CO_2 enriched water at bottom and photosynthetic activity at the surface. Here or in the discussion it could be helpful to mention that the observed surface-to-bottom variation suggests that benthic calcifiers appear neither to be influencing TA nor do they appear to be benefitting from the effects of photosynthesis on water chemistry, which seem to be confined to surface waters. Moreover, understory seaweeds, which can achieve substantial biomass in kelp forests, don't appear to affect water chemistry appreciably (tho this was not tested). A fuller discussion of these considerations could be helpful.

Thank you for bringing these points up. We will expand section 4.1: 'Mechanisms of observed variability' to include the discussions of TA variability, or lack thereof, as well as understory production.

Section 4.2: The discussion of "space-for-time substitutions" is reasonable, but in my opinion is less compelling than other arguments that can be made concerning kelp forest ecosystems in an era of global change. I'd encourage the authors to open the discussion with the most compelling inferences that can be drawn from their data.

We respectfully disagree with the reviewer on this point. We believe that the most impactful results from our study point to the importance of considering small-scale spatial variability in kelp forests. Small-scale spatial variability potentially creates differential organismal responses to local conditions, which may then scale to create observed ecosystem-level responses to environmental conditions. Therefore, understanding the organismal response to small-scale variability is critical to predicting the ecosystem response. The variations in DIC create opportunities for looking at how organismal responses differ between high and low DIC regions of the kelp forest. This "space-for-time" approach encompasses observational and manipulative work that will help build our understanding from small-scale spatial variability to organismal response to ecosystem response.

Page 11, lines 10-18: The discussion of refugia could be refined. Assuming that photosynthesis within the canopy modulates stress due to high CO2/low pH, it's difficult to think of very many organisms (especially calcifying organisms) that can take advantage of this. These are likely to be limited to epibionts on kelp blades and perhaps a few canopy-associated fish species. A much larger number of calcifying taxa are associated with the benthos, where water conditions are likely to be less conducive to calcification and growth when omega is low. Consequently, the potential refugium created by the canopy is spatially unassociated with the bulk of benthic species. Moreover, the persistence of refugia in such a dynamic system is questionable.

We agree that benthic organisms are unlikely to benefit from any biogeochemical refugia created by the photosynthetic activity in the kelp canopy. However, our results point to consistent vertical biogeochemical gradients during times of high kelp biomass. Therefore, any mobile organisms sensitive to high CO_2 may increasingly find refuge in the surface waters. Testing this idea is beyond the scope of our paper (and we don't have any data with which to do so). However, we believe it is important to mention this hypothesis in the Discussion section so that others can use our data as motivation to more formally test for the presence and importance of biogeochemical refugia in such dynamic systems.

Page 11, lines 25-34: Comments about water quality criteria are reasonable: for instance, it's important to point out that the variability observed in this study exceeds that of water quality criteria now in existence. However, the paragraph doesn't seem particularly nuanced, beyond references to Boehm et al, Weisberg et al, and Chan et al. I encourage the authors to more fully consider the implications of their data for water quality criteria.

We use this paragraph to compare our observations to existing water quality regulations. However, we believe that an extensive review of water quality regulations along the west coast of North America, and how they interact with biogeochemical variability in a kelp forest, is beyond the scope of this paper. Extensive additions to this section of the paper would further extend the Discussion section of the paper. Reviewer #1 already suggests reducing the length of the Discussion and we believe further lengthening would take focus away from the main scientific points of the paper.

Page 11, line 28: replace "supervising agencies" with "regulatory agencies".

We will replace "supervising agencies" with "regulatory agencies" in the revised manuscript

Page 12, line 8: distinguish between "fully constrained" and "over-constrained" with respect to carbon system variables.

Thank you for identifying this subtle, but very important, difference. We will clarify our language here to emphasize the crucial importance of fully constraining the carbonate system (measuring 2 carbonate system parameters), with a goal of over-constraining the carbonate system (measuring 3 or more carbonate system parameters).

Page 12, line 18: replace “harvesting” with “kelp harvesting”.
We will replace “harvesting” with “kelp harvesting” in the revised manuscript

Page 12, line 20: should read “effects of the kelp canopy”.
We will use “effects of the kelp canopy” in the revised manuscript

Page 12, line 22: replace “chemically homogenize” with “reduce gradients in”.
We will replace “chemically homogenize” with “reduce gradients in” in the revised manuscript