Reply to Reviewer comments for:
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Here we address reviewer comments (black text below). Our rebuttal text is in red. Text quoted from the modified manuscript is indicated with quotation marks, " "; new text is underlined; deleted text is shown with a ~~strikethrough~~; and new references are highlighted.

Interactive comment on "Including high frequency variability in coastal ocean acidification projections" by Y. Takeshita et al.

Anonymous Referee #1

Received and published: 22 June 2015

General Comments

This paper uses data from existing long term monitoring programs (sensors) and cruises and combines it with known empirical relationships for the region as well as atmospheric CO_2 records to achieve modeled carbonate system projections for different habitats in the Southern Californian Bight. This is an excellent example of how existing datasets can be used for interesting habitat specific projections of CO_2 chemistry. Using existing data and relationships has the disadvantage that there are some basic differences between e.g. sensor deployment heights (2, 3, 0 and 12m above the surface) and regional empirical relationships are not exactly matching discrete samples for TA. The study also uses a lot of estimates (phosphate concentrations p7136, line 19-20); _DICdiseq (p7137, lines 7-10) instead of values based on measurements. As the paper is based on existing data and published empirical relationships, it is not really novel, although the combination and comparison of different habitats and the estimate of future pH variability in these zones are interesting and merit publication.

The authors would like to clarify that only 2 of the 4 sensor data presented here are "existing" long term monitoring programs (Scripps Pier and Del Mar Mooring); 3 of the 4 sensor data have not been published elsewhere. A main objective of the cruise data presented in this paper was to link hydrographic inorganic carbon data with near-shore sensor data. Furthermore, the empirical relationship for phosphate was developed for this project and presented in this manuscript. For these reasons, we argue that there are many novel aspects of this paper.

Specific comments

Page 7133-paragraph 2.3 – Honeywell Durafet III pH sensors are reported to have excellent stability for months or even years, with an estimation for this study (line 15) of better than 0.005 (units). This is very likely but not verifiable from the text as the authors do not mention if the calibration samples were taken at the beginning of the time series or/and at the end and do not report (absence of) drift.

The following text has been added to L207-209 to clarify:
"All pH measurements were calibrated based on discrete TA and DIC samples taken alongside the sensor, at minimum at the beginning and end of each sensor deployment (n > 4 for every site), as recommended by the best practices (Bresnahan et al. 2014)."

How was accuracy estimated?

The accuracy was estimated based on best practices for calibrating autonomous pH sensors (Bresnahan et al. 2014). It is mainly limited by how well pH can be constrained using DIC/TA to calculate pH (accuracy of ~0.01).

The section of TA estimation (line 20 and below) is difficult to read without the citation (Alin et al. 2012) detailing the regional empirical relationship. It would be useful to know the relationship is based on temperature and salinity for instance. The abbreviation RMSE (root mean square error) is not explained. It is unclear if the offset used to account for differences between measured values (discrete samples) and the empirical relationship for TA for the larger system applied to all three subsurface sensors. If so, what could have caused the discrepancy between the relationship in previous years (2005-2011) and 2012?

The +8 µmol kg^{-1} offset was applied for all subsurface sites. The discrepancy was not limited to 2012, since the discrete samples were collected between 2010 and 2013. Therefore this offset was most likely a persistent feature due to a regional (San Diego) source of TA that was not accounted for in the empirical model (Alin et al. 2012) that was designed to cover large portions of the California Current System. The following text was modified to improve clarity:

L214: "For the three subsurface sensors, TA was estimated (TA^{est}) using a regional empirical relationship developed for the CCS, with temperature and salinity as inputs (Alin et al. 2012)~~.~~; an offset of +8 µmol kg^{-1} was applied to TA^{est} based on comparisons to discrete samples (root mean squared error (RMSE) = 6 µmol kg^{-1}, n = 25). This offset was a persistent feature over multiple years (2010 – 2013), thus most likely reflecting a regional surface TA influence that is not incorporated in the empirical relationship developed for the whole CCS."

Uncertainty for resulting DIC, pCO2 and Ar is pH-dependent (line 26), but how much uncertainty is introduced by using estimates based on an empirical relationship that needs to be corrected with an offset?

The uncertainty of the estimated TA using T & S is 6 μmol kg^{-1}, based on the RMSE between estimated TA and discrete samples. We do not believe that the offset introduces error, but in fact improves the fit, since the disagreement between the estimated TA and discrete samples would be larger without this offset. The uncertainty reported for DIC, pCO_2, and Ω uses ± 6 μmol kg^{-1} for TA in the calculations.

Page 7134 – paragraph 2.4.1 – future and pre-industrial TA is fixed at 2012 levels. How realistic is this? E.g. will there be no change in watershed dynamics in the coastal zone or is there little influence of river runoff?

The reviewer is correct in that these models assume constant TA, but that it is possible that TA may be different in the future (or have already changed since pre-industrial times). However, there is no study observing long-term changes of TA in the CCS, thus is extremely hard/impossible to parameterize. These concerns are included in the Discussion, and we do a simple error analysis by changing TA conditions to assess the first-order uncertainty to the projections (in Model Assessment, section 4.3).

Page 7135 – lines 5-6: "Although large deviations from equilibrium conditions are often observed in the coastal ocean due to upwelling and biological production (Hales et al., 2005), the mean pCO2 calculated from sensor data at the surf zone was 394 _atm, suggesting that the surface water at the study site was near atmospheric equilibrium." Biological production can cause large fluctuations that still can have an average close to atmospheric equilibrium. How variable was the pCO2 at the surf zone? The authors later give this information in Table 3 but it would be nice to have it here.

Text on L253 has been modified to: "…the mean pCO_2 calculated from sensor data at the surf zone was 394 <u>± 43 (1 s.d.)</u> μatm (Hales et al. 2005), suggesting that…"

Page 7139 & 7140 – It is surprising that the mean daily range of the canyon edge was higher than in the kelp forest. The authors discuss that this could be due to amplification of tidal energy but still it seems odd primary production and respiration in a kelp forest would not cause larger daily fluctuations. In fact, the mean daily range in pCO2 for the kelp forest is the lowest for the four sites (Table 3). Supposedly the sensor was placed within the canopy (3m above the seabed), it would be helpful if the authors could comment on the processes behind the fact that different water masses brought in by tides cause larger daily fluctuations compared to a productive kelp forest. Would this be a representative situation for the region? For sites with upwelling?

The pH sensor was deployed near the benthos and not in the canopy for this manuscript. The pH variability within the La Jolla kelp forest has been published in detail in a previous publication (Frieder et al. 2012), but some of the relevant results will be summarized here. Frieder et al. 2012 demonstrated that the diel variability (driven by production/respiration) was significantly greater near the surface (7 m depth) relative to near the bottom (17 m depth). This is because 1) the highest level of production occurs at the top of the canopy, and 2) stratification prevents the chemical signature from the surface to reach the bottom parts of the kelp forest. Furthermore, the pH variability on 'event' time scales (several weeks) is driven by respiration under the thermocline which gradually reduces pH and O_2, and mixing events that ventilate the water column, bringing pH and O_2 to near atmospheric equilibrium.

It cannot be stated with certainty that large diel fluctuations of pH occur on all submarine canyon edges, due to lack of sensor observations. However, amplified tidal energy and internal bores along upwelling margins have been documented at other similar environments (Navarro et al. 2013, Swart et al. 2011), thus we expect pH variability to be high at these sites as well.

The following paragraph has been added to section 4.2:

<u>It may be surprising that the mean diel range of pH was the smallest at the kelp forest (Table 3), as one might expect a large diel cycle driven by photosynthesis and respiration in a highly productive kelp forest. This is most likely because the sensor was deployed near the benthos, below the most productive region of the forest. Frieder et al. 2012 observed significantly larger diel pH variability closer to the surface (7 m depth) compared to near the bottom (17 m depth; same as this study), demonstrating that the biologically driven diel cycle diminishes with increasing depth within the canopy. Therefore it is important to keep in mind that the results presented here are not reflecting kelp forest production dynamics, rather, reflect conditions that are experienced by benthic dwelling organisms inside the kelp forest.</u>

Page 7141-7142 – lines 27,28 &1-3: "all habitats studied here have left, or are about to leave, the pCO2 pH, and Ar conditions that were experienced during preindustrial times. This is significant as organisms at these sites are now surviving in conditions that are significantly different than the conditions under which their ancestors evolved."

Agreed that the average conditions are significantly different than the conditions under which their ancestors evolved. However, the daily range given in Table 2 is larger than 1 SD, so organisms living in these sites must have developed certain flexibility and adaptations to cope with these fluctuations. Figure 8 and 9 nicely show how greater extremes will be reached, and the amount of time spent in these situations will increase, which is more important for organisms than the higher average conditions. However only one site is shown (Del Mar Buoy) and the interesting comparison between different habitats is not visually plotted although figure 10 gives a reasonable summary. It would be nice to reproduce figure 10 more prominently, or add a similar figure as 9 for the kelp forest and discuss the differences between vegetated and non-vegetated habitats more explicitly.

We have added another figure comparing 2012 and 2100 levels of pCO_2 and Ω_{Ar}. Following text has been added on L413:

“Similar patterns were observed in the kelp forest as well, where both the mean conditions and variability of pCO_2 increased, and Ω_{Ar} decreased (Figure 10). The largest variability at the kelp forest occurred on timescales of days to weeks, and high frequency (< 1 day) variability was significantly smaller than at the shelf break. Therefore benthic organisms at the kelp forest would experience elevated CO_2 conditions for prolonged periods of time, with only intermittent exposure to near-atmospheric conditions.”

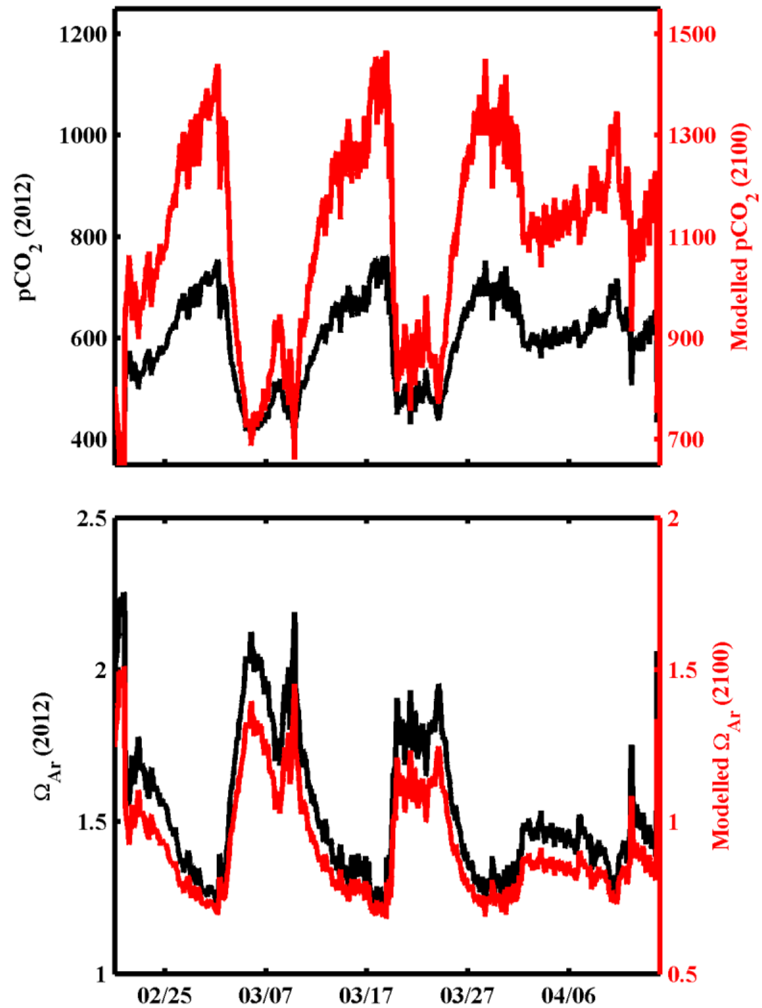

Figure 10: Observed (black) and modelled (red) pCO_2 (top) and Ω_{Ar} (bottom) at the La Jolla kelp forest (17 m). Modelled values correspond to projected values in 2100 using the IPCC RCP6.0 scenario. Note that the range, but not the absolute values, of the vertical axes for each figure is the same.

We have also added a paragraph in section 4.2 to expand the discussion on the variability in the kelp forest (see above).

Page 7146 – lines 19-21 – "temperatures observed in 2012 were used to parameterize the model. Sea surface temperature has increased over the past century due to climate change (Smith et al., 2008), and is expected to continue." Temperature increases do not only affect the chemical reactions of the carbonate system but can have a big influence on biological responses. For instance temperature increases might greatly affect the productivity of plankton or kelp forests. The biological signal due to respiration/productivity and thus carbon uptake might change in areas where vegetation can influence daily and seasonal patterns.

Totally agree. We have added the following text on L555 to discuss this source of uncertainty: "However, it should be noted that this simple error analysis does not include any biological feedbacks that increased temperature or CO_2 may induce. For example, phase shifts from kelp-dominated to algal turfs might be an outcome of sea surface warming and acidification (Connell and Russell, 2010), with implications for habitat-scale biogeochemical cycling Likewise, higher temperature may increase remineralization rates along the path of the subducted water (Rivkin and Legendre, 2001), further enhancing acidification."

Connell, S. D. and Russell, B. D.: The direct effects of increasing CO2 and temperature on non-calcifying organisms: increasing the potential for phase shifts in kelp forests., Proc. Biol. Sci., 277(1686), 1409–1415, doi:10.1098/rspb.2009.2069, 2010.

Rivkin, R. B. and Legendre, L.: Biogenic carbon cycling in the upper ocean: effects of microbial respiration., Science, 291(5512), 2398–2400, doi:10.1126/science.291.5512.2398, 2001.

Interactive comment on "Including high frequency variability in coastal ocean acidification projections" by Y. Takeshita et al.
Anonymous Referee #2
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Takeshita and co-authors have prepared a well written manuscript, presenting results from four regions with "unique-habitat specific CO_2 variability and ocean acidification trajectories". Further, based on pH sensor data and hydrographic data they have developed a simple model that is applicable to each location to determine future trends. In my opinion, the authors have done a tremendous amount of work with the data at hand, have provided excellent figures, and I am sure this manuscript will be a valuable contribution to the scientific community. However, I strongly believe that the manuscript will benefit from additional analysis and clarification, so please find my comments below.

General comments:
In general, I am missing a discussion about the hydrographic data. It is mentioned in the text that at some sites, 'physical processes' are dominant, including 'tidal bores', but there is no presentation of e.g. temperature data that would support these statements. Since the data at hand are high resolution, this should be possible. E.g., when 'tidal bores' are influencing the sites, is the timing right for decrease in T and pH with the tides?

Yes, the timing between Temperature (or density) and pH is aligned in a way that supports internal waves and bores. A supplementary figure demonstrating the correlation between these variables at the surf zone and canyon edge has been added.

Following text has been added to section 3.1 clarify:

"In the surf zone, the conditions were near atmospheric equilibrium, with intrusions of higher pCO_2 waters through internal tidal bores, a common feature observed in shallow, upwelling environments (Booth et al., 2012, Pineda, 1991); temperature and pH were correlated during these events (Supplementary Figure 1)."

"Temeprature and pH were correlated on these shorter timescales (Supplementary Figure 1), further supporting the fact that the variability was dominantly driven by intrusion of cold, deep waters from the canyon."

Further, for all sites, I would suggest to strengthen the point of exactly how each habitat is "specific" in terms of CO2 variability. This can be done in different ways, but a common one is to calculate the hydrographic (e.g. diurnal variations in T) portion of e.g. the pCO2, the difference could be to a first order assigned to biological production/respiration.

We believe that we have described the specific pH variability signature in each site/habitat in great detail, as the entire section 3.1 was devoted to this topic. For example, we explain: 1) intrusion of internal waves dominates the variability at the surf zone, 2) event scale variability driven by a combination of stratification, upwelling, and respiration drives the biggest variability in the kelp forest, and 3) amplified tidal energy drives large diel variability at the canyon edge. Perhaps the disagreement arises from the reviewer defining "unique habitat" as biologically driven CO_2 signatures, whereas we define both physical and biological drivers as a "habitat". Since physical dynamics were a significant driver for CO_2 variability at all sites studied here, we decide to include both physical and biological drivers into the definition of a "habitat", since the organisms living there will experience unique CO_2 variability regardless of whether they were of biological or physical origin. To clarify this point, we have added the following text:

Distinct, habitat-specific CO_2 signatures were observed at the four deployment sites (Figure 5, Figure 6, and Figure 7). Here, we define habitat-specific CO_2 signatures as how CO_2 conditions varied in that habitat, regardless of biological or physical origin.

Regarding the suggested analysis from the reviewer, we believe that this approach is not appropriate for the data presented here. This sort of analysis is only applicable when sensors were deployed in the surface mixed layer, where the diurnal temperature swings are dominantly driven by solar heating, and different water masses are not present, e.g. surface moorings like in

Leinweber et al. 2009. However, much of the variability is driven by physical processes bringing deep water to the deployment site, thus violating the assumption of a constant water mass. Therefore we cannot obtain a first-order estimate of the biological contribution from this approach. One exception may be within the kelp forest, where respiration drives pH low. However, this analysis has been performed at the same site in detail elsewhere (Frieder et al. 2012), and has been referenced multiple times throughout this manuscript.

In addition, there are not many publications out there that discuss diurnal pCO2 (or pH) variability, hence the manuscript would benefit from adding results presented in Leinweber et al., (2009), where diurnal pCO2 data in the Santa Monica Bay are discussed. Here, the authors might find further useful information on how to make the 'unique-habitat specific CO2 variability' for each of the four habitats more clear.

We have added the following sentence on L359:
"The mean diel range of pCO_2 at the surf zone was significantly higher than measurements made by a surface mooring located off shore in the SCB (Leinweber et al., 2009)

Leinweber et al., (2009). Diurnal carbon cycling in the surface ocean and lower atmosphere of Santa Monica Bay, CA. GRL. VOL. 36, L08601, doi:10.1029/2008GL037018

It should also be discussed how much of the 'uniqueness' of each habitat is the result of the different depths. Overall, at least to me, the term 'unique habitat' implies that the main driver in the CO2 variability is the biological production/respiration, and not the physical characteristic and its associated inorganic carbon dynamic, that make each habitat special. So, I feel that this needs clarification.

We have defined "habitats-specific CO_2 signatures" as: <u>Here, we define habitat-specific CO_2 signatures as how CO_2 conditions varied in that habitat, regardless of biological or physical origin.</u>

Agreed about clarifying the influence of depth. The mean conditions, and the trajectory of mean conditions vary with depth, due to naturally elevated CO_2 with increasing water depth. However, the higher frequency variability is specific to each habitat. To clarify this point, we have added (on L123):

"…this drastic difference in increased mean pCO_2 is driven by different buffer factors <u>due to depth</u> differences among the sites"

The authors further strengthen the importance about upwelling in (future) ocean acidification impacts on marine ecosystems. As of now, there is no representation from e.g. the cruise data that the data are really taken during an upwelling event. June/July is often too late to capture coastal upwelling in the Bight, but could be easily verified using wind data in combination with the cruise data. The authors are citing Bograd et al. (2009) that the Bight has 'weak upwelling year round'. In this paper, the UI is discussed. The region north of Point Conception has a strong upwelling and a strong downwelling phase compared to the region south of Point Conception; leading to an interpretation that there is 'year round weak upwelling' at 33 N. But I am not sure that this is what the authors from this manuscript are trying to support. Some clarification, maybe even rethinking if the strong focus on the upwelling discussion is even needed, seems appropriate.

We have removed the section, and reference to Bograd et al. 2009 as the reviewer suggested.
~~The Southern California Bight experiences a steady but weaker degree of upwelling compared to the northern regions of the CCS, where upwelling events are more pronounced (Bograd et al., 2009). These regions could experience more extreme conditions regularly, as well as significantly higher variability of carbonate conditions (Harris et al., 2013). However, such dynamics are poorly understood, and more high-frequency observations of carbonate parameters along this system are needed. Source water properties must be characterized through hydrographic surveys.~~

We have added another citation Nam et al. 2015 to demonstrate that the upwelling season in this region of the SCB does in fact last through July, as inferred through upwelling favorable winds. The wind data at the Del Mar Buoy, between 2009 and 2013 demonstrate that there are equatorward winds (upwelling favorable) throughout the year, and it strengthens between March and August.

Nam, S., Takeshita, Y., Frieder, C. A., Martz, T. and Ballard, J.: Seasonal advection of Pacific Equatorial Water alters oxygen and pH in the Southern California Bight, J. Geophys. Res. Ocean., 120, n/a–n/a, doi:10.1002/2015JC010859, 2015.

Throughout the text, adding the standard deviations to the reported values seems necessary. Although listed in a table, this information belongs to the values in the text as well.

s.d. values were added to various locations in the manuscript (e.g. L253, L344, L381, L396)