

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 CO₂ 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’,

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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? 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. 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. 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.

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.

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.

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

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