

Interactive
Comment

***Interactive comment on “Spatiotemporal
variability and long-term trends of ocean
acidification in the California Current System” by
C. Hauri et al.***

C. Hauri et al.

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Received and published: 29 October 2012

Dear Dr. deGrandpre,

Thank you very much for taking the time to review our manuscript. Your comments and corrections have helped to improve this article. Please find replies to your comments below. The manuscript has been edited accordingly.

Best regards,

Claudine Hauri and co-authors

Report of Revisions

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deGrandpre: “The model does not accurately predict these values as compared to observations, especially in areas where upwelling is most intense and transient. This disconnect is primarily attributed to use of long-term versus daily wind forcing. This leads to problems in predicting undersaturation thresholds and more should be said about the uncertainty of the predicted timing of undersaturation (i.e. in the Abstract).”

- We added an extra sentence in the abstract describing the uncertainty in our aragonite saturation state projections. In addition, this caveat has already been thoroughly discussed in the current version of the manuscript in Section 3 (Model evaluation) and the Discussion (Caveat #1 and #2).

Abstract: ...The aragonite saturation horizon of the central CCS is projected to shoal into the upper 75 m, within the next 25 years, causing near permanent undersaturation in subsurface waters. Due to the model's overestimation of Ω_{arag} , this transition may occur even earlier than simulated by the model....

deGrandpre: “The model is also used to breakdown the contributing factors to pH (and saturation state) variability. This section underutilizes the model results by grouping forcings such as advection and primary production as changes in DIC. DIC is not a forcing itself and therefore the meaning of the changes in DIC (and pH, etc) is lost. It would be far more interesting to use the model to dissect how DIC changes e.g. during an upwelling event. How long does DIC remain high and pH low – is the upwelled water swept away or does it remain long enough to have significant DIC drawdown, etc. “

- The physical and biological drivers of changes in DIC and Alk are detailed in equations A2 and A5 in the appendix. Instead of going into the complexity of the effect that production, nitrification, calcification, dissolution, advection, diffusion and sinking have on DIC and Alk, and then translating this into pH and omega, we opted to describe the changes in omega and pH as a function of the chemical variables that can be easily measured.

deGrandpre: “The discussion in this section also misplaces emphasis on changes in

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alkalinity as controlling pH. Production changes DIC mostly, with a factor of 6 smaller change in alkalinity, so DIC decreases and Alk stays relatively constant, increasing the pH and omega. Upwelling water has high DIC but the alk is not that different from surface water (salinity is similar to surface water except during runoff periods and Alk is pretty conservative on the Oregon shelf down to 200 m?), so changes are driven by DIC, not alkalinity. I don't understand the comment "upwelled alk enriched waters counter the effect of upwelled DIC rich waters" as the two come as one package."

- Both, observations and the model point to relatively strong vertical gradients in Alk (see Figure 4c). But overall we agree that this was poorly explained. We have changed the quoted text as follows:

Page10390/Line1 Delete: In the northern CCS, the upwelled Alk-enriched waters counteract the effect of upwelled DIC-rich waters, causing a peak of Ω_{arag} in April and May (Fig. 10a).

Add: Upwelling brings waters enriched in DIC and Alk to the surface. In the northern CCS, the increase in surface DIC is mitigated by primary production, while Alk is less affected by this process. Therefore, during spring the changes in Ω_{arag} are driven primarily by the upwelling of Alk (Fig. 10a).

Other comments:

deGrandpre: "Model predictions might be improved by using a coastal salinity- alkalinity relationship rather than the Lee et al. equations (see Gray et al. Mar. Chem. 2011)."

- Thank you for pointing this paper out to us. We will consider using Gray's coastal salinity-alkalinity relationship for future simulations.

deGrandpre: "I do not agree with the statement that DIC, Alk and T are " mainly altered by upwelling and eddies, which differ in magnitude and timing from region to region (Figs. 9 and 10)." Primary production plays a very important role in altering the upwelled water. Perhaps this is just a misunderstanding in the way it is stated."

- We agree with the referee and removed “mainly” from the sentence. The subsequent sentences (Page 10388/Line 8-11) in the current manuscript state that primary production also plays an important role in changing the chemistry of the upwelled water. The phrase now reads:

Page 10388/Line 6: These parameters are altered by upwelling and eddies, which diver in magnitude and timing from region to region (Figs. 9 and 10).

deGrandpre: “Isn’t the dominance of high frequency variability more apparent at location 2 because location 1 has such a large annual cycle? Location 1 has high frequency variability too but the discussion implies it does not.”

- Please note the difference in axis scales on figures 2 and 3. Despite the overwhelming signal from low frequency variability at location 1, the high frequency variability is stronger at location 2 than it is at location 1. To clarify this issue we reworded the description in the results accordingly:

Page 10381/Line 16: While in location 1 low frequency variability dominates (Fig. 2c, upper panel), high frequency variability prevails in location 2 (Fig. 2c, lower panel).

deGrandpre: “Figures 9 and 10 the black trace (total?) is not explained.”

Page 10419/Figure 9: Changed “to changes (relative to the annual mean) in pH” to “to changes (relative to the annual mean) in pH (black)”

Page 10420/Figure 10: Changed “to changes (relative to the annual mean) in pH” to “to changes (relative to the annual mean) in pH (black)”

Interactive comment on Biogeosciences Discuss., 9, 10371, 2012.

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