



Interactive
Comment

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

M. deGrandpre (Referee)

michael.degrandpre@mso.umt.edu

Received and published: 27 September 2012

The authors describe simulations of pH and aragonite saturation using a Regional Oceanic Modeling System (ROMS) focused on the U.S. Pacific ocean margin. The model is very useful for elucidating spatial variability between different regions and also for predicting future trends in coastal pH and omega in this important coastal ecosystem. 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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



about the uncertainty of the predicted timing of undersaturation (i.e. in the Abstract).

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 discussion in this section also misplaces emphasis on changes in 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.

Other comments: 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).

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.

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.

Figures 9 and 10 the black trace (total?) is not explained.

BGD

9, C4361–C4363, 2012

Interactive
Comment

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



Interactive
Comment

C4363

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)

