

Interactive comment on “CO₂-driven compromises to marine life along the Chilean coast” by E. Mayol et al.

Anonymous Referee #2

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To begin with, I was excited when I read the title of the manuscript, but after scrutinizing the work I think, as it stands now, it is too poor and pre-mature for publication in Biogeosciences.

This is a potentially interesting topic and the set-up presented here is acceptable.

However, there are few new results (if any?) from those works published by Torres for the coastal upwelling area off Chile or from other colleagues for other eastern boundary systems. Equatorial subsurface waters support elevated pCO₂, and obviously this implies a reduction in pH. These conclusions should not come as a surprise as they have been repeatedly shown for coastal upwelling areas worldwide (Torres et al. 1999 and specially Torres et al. 2002, Lefevre et al. 2008). Which is already new?, maybe the estimation of some values of saturation of aragonite by using the same standard

measurements.

In consequence, much of the presented results seem to be trivial, textbook information for an eastern boundary system with oxygen minimum zone and pCO₂ saturated waters.

I feel that there is vagueness in the treatment of this matter through all the manuscript and the paper contains little that offers new insight into how pCO₂ levels may impact marine life along the Chilean coast. However, the title of the manuscript makes mention to “compromises to marine life”.

First, when I read the title "CO₂-driven compromises to marine life along the Chilean coast", I was excited with the idea of seeing information about how such high levels of pCO₂ could impact the life along the coast of Chile. However, to begin reading the manuscript I was disappointed to see that information comes from one 15-station transect, located more than 200 miles offshore (see Figure 1). Accordingly, the first point is to change the title of this work to something like... "pCO₂ and pH along the Humboldt Current System during summer", because authors hardly might talk about how such levels of pCO₂ can impact marine life along the coast of Chile (maybe for coccolithophors it's OK).

The second is that this work is based on information taken during one single cruise of 11 days in the oceanic area off the coast of Chile, so the issue of seasonality is not considered. Coastal upwelling in some parts of the coast of Chile is seasonal (between 30 to 40°S), therefore the “big-picture” pCO₂, pH, and saturation of aragonite may change, and authors show just a ‘snapshot’ of this history.

pCO₂ is also strongly dependent on the productivity during the sampling period (production/respiration ratio), but I cannot see any basic environmental information, such as simple chlorophyll estimations or primary production.

For some inner shore areas off Central Chile there is probably other forcing that may

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intermittently cause suboptimal conditions for marine life, and probably much more important than coastal upwelling, for example, fresh water discharges. They are low in pH relative to the sea, and in case of rivers with high eutrophication levels (e.g. Maipo, Maule, Bío-Bío, Itata, Valdivia), it can be expected a high pCO₂ where acidic river water may mixing into the surface ocean and affect coastal chemistry on broad regional scales to an extent that could compromises to marine life more than expected 200 miles offshore. See Salisbury (2008), EOS 89(50). At this respect, recently Borges & Gypens (2010) have also shown that river nutrient delivery due to management regulation policies can lead to stronger changes in carbonate chemistry and their subsequent effect on coastal marine life, even more important than global ocean acidification.

At Page 8902, Line 26 “The additional stress to biota in the hypoxic water mass of the Humboldt Current System arising from the high pCO₂ levels has not been discussed earlier” and again, at, Page 8905, Line 9-10 “. . .may compress the habitat suitable for important commercial species, driving changes in the ecosystem in a high-CO₂ future.

However, no mention about which kind of important commercial biota they are speaking about at 200 nautical miles from the coast. . .!!!! fishes?, I can imagine that the authors are restricting their discussion to calcifying organisms, such as coccolithophorids?, but they are not commercial biota, at least for the moment. In addition, the broad discussion about oxygen limitation for marine life is trivial, and again, just textbook information for an eastern boundary system.

The general approach is good, and I think the work is interesting and has the potential to make an impact on this field. However, I kept getting the feeling that the authors knew a great deal about the data

In summary, I know some of the authors of the present MS very well, and I’m pretty sure they can do much better.

If the authors are able to review the MS in accordance with my comments (they should change the focus of the MS, including more modest objectives focused in the offshore

area off Chile), and include more and new data offshore and/or in shallow waters along the Chilean coast I recommend that the authors perform a major revision and resubmit this MS to Biogeosciences or more specific journal.

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