

Interactive comment on “A regional hindcast model simulating ecosystem dynamics, inorganic carbon chemistry and ocean acidification in the Gulf of Alaska” by Claudine Hauri et al.

Anonymous Referee #3

Received and published: 15 April 2020

General comments:

The manuscript by Hauri et al. evaluates a new regional marine biogeochemistry model COBALT-GOA. The study is well motivated, clearly structured and very readable. The key strength of this new modelling study is the coupling of the regional model to a hydrological model that is forced by reanalysis climate. Therefore freshwater influx is driven by internal variability. The authors discuss the consequences of freshwater influx on biogeochemistry, in particular the aragonite saturation. The model is helpful to learn more about biogeochemical seasonal cycle in a region with sparse data.

My largest comment concerns the presentation of the modelling results. How long was

C1

the model run? I have expected to see a 30 years + timeseries, especially as you state that you want to analyse the effects of inter-annual variability apart from the climate change signal. The comparison presented only covers 5 years. Why did you focus on these particular years? Why not longer, why not more recent? Did you do some spin-up before 1980?

Specific comments:

p1L10: try avoiding “perhaps”

p2L10: “make this region a challenge”. What is the challenge in this region?

p4L9: “we need . . . 5) multiple phytoplankton groups”. Why is this specifically needed here? It has been in the model already before I guess and also one bulk phytoplankton can produce high-nutrient low-chlorophyll regions

p5L1: what’s the resolution of the reanalysis? Do you need to downsample?

p6L5: why do you use a different reanalysis for the climate forcing than compared to the hydrological model?

p6L6: “precip does not dilute any other tracer”. Is this a standard procedure? Can you justify why this is legitimate? Can you cite other studies using this?

p6L23: I am surprised that you use the Mauna Loa seasonal cycle. At more northern latitudes the seasonal amplitude is much larger than in moderate latitudes. See Keppel-Aleks, Gretchen, James T. Randerson, Keith Lindsay, Britton B. Stephens, J. Keith Moore, Scott C. Doney, Peter E. Thornton, et al. “Atmospheric Carbon Dioxide Variability in the Community Earth System Model: Evaluation and Transient Dynamics during the Twentieth and Twenty-First Centuries.” *Journal of Climate* 26, no. 13 (January 14, 2013): 4447–75. <https://doi.org/10/f439zf>. Table4. Please explain why this is OK for your study.

p7Table2: confused by the ordering of values: min,mean,max is easier to grasp for me

C2

p7L3: “reproduce”. Please indicate [not shown]

p11Fig5: add explanation black line in f)

p12L6: “insignificant”: by what means insignificant? Some p-value analysis? Low compared to internal variability or seasonal cycle.

p13L4: “model’s bathymetric is too shallow”: How come the model’s bathymetry is too shallow? Cannot you change the model bathymetry?

p17Fig10: I was wondering whether you also analysed salinity-normalised DIC (sDIC as in Gruber & Sarmiento 2006)? How much of this seasonal cycle in DIC comes from salinity and how much from other factors?

p21L9: What do you mean by “endmembers”?

p21L10f: you may cite this new study about biogeochemical composition of freshwater <https://www.biogeosciences.net/17/55/2020/>

Technical comments:

I could not find a repository containing scripts to produce the figures shown. This would be helpful for reproducibility, i.e. understand how the plots were generated. https://publications.copernicus.org/services/data_policy.html other underlying materials: software and scripts availability.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-70>, 2020.