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Interactive comment on "Seasonal patterns of surface inorganic carbon system variables in the Gulf of Mexico inferred from a regional high-resolution ocean-biogeochemical model" *by* Fabian A. Gomez et al.

Anonymous Referee #1

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In their manuscript Gomez et al expand their Gulf of Mexico biogeochemical model (Gomez et al., 2016) to include carbonate chemistry. After some validation, focused on this new part of the model, the authors describe the annual climatological spatial and temporal patterns of the modeled (or derived) carbonate chemistry variables and discuss the air-sea CO2 flux in their model in comparison to former global or regional studies. The manuscript is well written, the method apropriate, the figures are good quality and the conclusions supported by the results. My main general comment is to expand the discussion of your results. Currently it is not clear what is the novelty of

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your estimates in air-sea CO2 flux in comparison to the previous regional work. Your discussion mostly focus on showing that your model is able to simulate the regional carbon dynamics. Could you also provide some discussion what your results mean in term of the Gulf regional carbon dynamics, i.e. for each of your regions. For instance you mention that there is a need "to identify coastal ecosystem susceptibility to ocean acidification" but this is not discussed further, i.e. with respect to your results. A few specific comments are provided below.

L106-107: is it 40/9 years of the same annual cycle? Is 9 years sufficient for the carbon system, e.g. for the deep Gulf it seems short, and how did you assess that the carbon system was spun-up?

L109: It would have been interesting to see model output for the period January 1981 to November 2014.

L144-145: a more accurate statement would be "Overall, simulated and observed pCO2 patterns agreed with observations"

L146-147: is it possible that your model generally overestimates surface primary production, resulting in a lower surface pCO2?

L144-151: There is obviously a very large difference in the shape of the observed versus modeled pCO2 time series in NGoM (Figure 3b). This should be discussed. Why is there a strong dip in pCO2 in March in the observations and why it doesn't occur in the model? January-February observations are odd. Figure 3b also shows that there is a 1 month delay in the modeled pCO2, which tend to follow more the temperature cycle. Can you discuss these discrepancies?

L152-160: in Figure 4 caption can you add location information, i.e. off Tampa (upper panel?) and off Louisiana (lower panel?) and refer to Figure 4a and Figure 4b when appropriate.

L155-156: the 0-200m difference in DIC and TA is quite large. You need to provide

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more discussion here to gain confidence in the results presented below. What is the source of this discrepancy?

L230: "This is not the case..."

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