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Biogeosciences Discussions
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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 #2

Received and published: 30 November 2019

General Comments

This is a very well written manuscript, with good figures, and scientific arguments that are interesting and well-constructed. The authors appear to have made a significant advance in understanding the regional and temporal variability of seawater CO2 chemistry and air-sea CO2 fluxes in the Gulf of Mexico. There is potential for expanding the discussion (see specific comments). However, one of the aspects I appreciated of the manuscript as it stands was the concise length: the authors should try to limit text additions in response to reviewers and balance with (careful) trimming. Overall, I con-

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gratulate the authors for an excellent submission and recommend only minor revisions.

Specific Comments

"air-sea flux": Most readers will read this as AIR-TO-SEA flux. Therefore it should be positive when the ocean is a sink and negative when the ocean is a source (as in e.g. Xue et al., 2016). I recommend that the authors either: a) reverse the signs on all "air-sea" fluxes, or b) speak of "sea-air" fluxes, which is an alternative convention.

L112-113: The carbonate chemistry equilibrium constants from Mehrbach et al. (1973) refitted by Dickson and Millero (1987) might not be optimal for the salinities <20 psu explored by this study (cf. Fig. 6; see Millero 2010, and stated validity ranges in the van Heuven code).

L148-150: Looking at Fig. S6 it seems that the mismatch may be primarily due to interannual variability and temporal undersampling. The observations in Fig. S6 do suggest a strong decrease in pCO2 on the northern GoM shelf during JFM, but they appear to be from single cruises and perhaps a single year (?), while the model results are averaged over 10 years. Perhaps clarify about this.

L272-273: I am left wondering exactly why, compared with the Xue et al. model, the present model apparently simulates stronger biological DIC uptake and associated pCO2 decrease in the MARS region, sufficient to turn this region into a year-round CO2 sink (cf. present Fig. 11 vs. Xue et al., 2016, Fig. 7). Is it possible that the apparent improvement in fit to surface pCO2 observations (present Fig. S6) could be for the wrong reasons? Can the stronger biological uptake be corroborated with other observations (e.g. nutrient drawdown)? Also, it seems that the large-scale seasonal variability in CO2 flux, here driven primarily by temperature, is stronger than in Xue et al. Is this true, and if so, why?

Millero, F.J. 2010. Carbonate constants for estuarine waters. Marine and Freshwater Research 61(2) 139–142

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Technical Corrections

L40: ON the Louisiana-Texas shelf

L55: No comma after ‘timescales’

L63,64: Flip signs on 0.32 and 1.04

L78: Delete ‘and briefly detailed below,’

L83: derived USING

L84: Maybe you can save some space by removing “Supplement” (10 instances) and just referring to Section S1, Fig. S1, etc. Check journal requirements.

L88,89: Hyphenate ‘third order’ and ‘fourth order’

L97,98: ‘Stets’ not ‘Stet’

L98,99: I do not see a description of how to calculate river DIC from river (pH, TA, T) in the Stets et al. (2014) listed in the References. Is this the correct reference?

L106: FOR 40 years

L124: . . .temperature and salinity, changes. . .depend on [CO3 2–] and are. . .

L125: TA:DIC

L134: as a data package

L147: ON the northern GoM SHELF (assuming the statistics are restricted to the region shown in Fig. 1).

L148: MAY BE due. . .

L156: . . .ranges DURING JUNE-AUGUST 2000-2014.

L190: maximum values in SUMMER and minimum in WINTER

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L202-203: Refer to regions exactly as defined in Figure 1, not omitting “shelf”

Figures

Figure 2: Assuming this is practical (not absolute) salinity, I disagree that it should be “unitless”. You could neatly specify which definition is used with the unit [psu], which for me is a perfectly valid and informative dimensionless unit.

Figure 3: Add sentence to caption saying what the red/blue lines are (presumably mean values over all model grid points and observations within the regions defined in Fig. 1). Reference should be to Fig. S5 not S4.1. Refer to northern GoM SHELF (assuming that the statistics are restricted to the shelf region).

Figure 4: ON the Mississippi and Tampa lines

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