

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-22

Anonymous Referee #2

Referee comment on "Evaluating the Arabian Sea as a regional source of atmospheric CO₂: seasonal variability and drivers" by Alain de Verneil et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-22-RC2>, 2021

The authors used a high-resolution regional model to quantify the annual mean and the seasonal cycle of air-sea fluxes of CO₂ in the Arabian Sea. The model results showed that monsoon-driven sea surface temperature variations strongly influence the seasonal cycle of air-sea fluxes of CO₂ in the Arabian Sea, except in upwelling regions. Here the supply of DIC seems to exert a main control on CO₂ fluxes across the air-sea interface. Overall, the model results imply that the Arabian Sea acts as a CO₂ source to the atmosphere, since the biological drawdown of CO₂ in surface waters failed to overcompensate the physical CO₂ supply. Additionally, strong winds increased the CO₂ fluxes into the atmosphere especially during the upwelling season in summer. The annual mean CO₂ flux into the atmosphere amounted to 160 TgCyr⁻¹.

In principle the paper is well structured and nicely written even though it includes quite extensive data descriptions, which could be shortened. However, I have three significant overarching objections due to which I recommend a major revision. The first objection regards the novelty of the presented work, the second point of criticisms is the way in which temperature and especially DIC and TA are discussed, while the last one refers to the model validation.

Novelty: The results obtained by the model were expected and not new, except the magnitude of the CO₂ flux into the atmosphere. This estimate by far exceeds estimates derived from field data and other models of up to approximately 90 TgCyr⁻¹. To me it remained elusive whether the presented estimate of 90 TgCyr⁻¹ is reliable or a model artefact. The authors used a high-resolution model to study the carbon cycle and the resulting air sea fluxes of CO₂ in the Arabian Sea. This is a new approach that according to the authors, eliminates shortcomings of coarse-resolution models and helps to overcome uncertainties caused by the low density of field data. In so far, the high CO₂ flux seems to be a new result showing that CO₂ fluxes from the Arabian Sea into the atmosphere have been underestimated in the past. However, due to differences between model data and field data (DIC and especially TA), it appeared to me that the authors

even considered the high CO₂ flux, at least partly, as an artefact. This aspect needs to be clarified since it reduces the novelty of the study and additionally raises doubts regarding the advantage of the used high-resolution model over previously used coarse-resolution models.

Discussion: The authors discussed temperature, TA and especially DIC changes as processes but these changes are the result of the interplay of different physical and in the case of DIC and TA, also biological processes. To my understanding, disentangling the role of these processes on the CO₂ flux should be the main aim of data evaluation and discussion. For instance, I would have expected a discussion about the impact of the marine carbon pumps on the CO₂ emissions into the atmosphere. Since changes in DIC, TA and temperature are the result of their interplay, a discussion, which is largely restricted to changes of parameters, circumvents the discussion on driving forces and that is what, to my opinion, matters.

Model validation: Model validation is a crucial aspect especially in the presented work as the model includes a variety of processes and parameterization, which to my opinion, are problematic. In the following I will present two examples to underpin this statement.

1) The model is based on nitrogen, and a fixed C/N ratio of 106/16 is applied to convert nitrogen into carbon and vice versa. In contrast to many other regions, denitrification and nitrogen fixation in the Arabian Sea strongly influence the nitrogen cycle and cause deviations from classical Redfield ratio. In previous studies, some of the co-authors used a similar or even the same model to investigate the nitrogen cycle in the Arabian Sea. These aspects should also be included into the current work as changes in the Redfield ratio and the availability of nitrogen both affect the carbon cycle and the resulting fluxes of CO₂ into the atmosphere.

2) Production, export and dissolution of CaCO₃ are further issues, which, to my understanding, need to be reconsidered especially if one considers the TA problem as mentioned before. The production of CaCO₃ was linked via a fixed ratio to primary production. Primary production rates were compared to satellite data but what about the production rates of CaCO₃? Furthermore, it was assumed that CaCO₃ sinks with a velocity of 20 m day⁻¹ and dissolves with a rate of 0.0057 day⁻¹ in the water column and 0.003 day⁻¹ in surface sediments. Does this approach reflect the distribution of carbonate in shelf sediments? In regions where oxygen-depleted mid-waters flushes the shelf, primary production is high and thus also the carbonate production, but low CaCO₃ concentrations within the underlying sediments indicate a high CaCO₃ dissolution. Does the model represent such processes? Furthermore, how does the constant carbonate dissolution rate agree to observations showing that the entire upper water column is oversaturated with

respect to calcite and aragonite and how do forams fit into the modelling approach? They are assumed to be major CaCO₃ producers in the Arabian Sea and their shells can sink with a speed of several hundred meters per day!

Even though the data density is low over the last decades, it is high during the JGOFS field studies and this data can be used to dispel the majority of my doubts regarding processes discussed before in the two examples. For instance Millero et al. (1998) presented water column profiles showing parameters characterizing the carbonate system such as saturation states of calcite and aragonite, TA, DIC, pH and AOU. Morrison et al. (1998) provided associated nutrient data including data on the distribution of dissolved oxygen in the water column. I strongly recommend to include data obtained from the entire water column into the model validation and show profiles of field and model data in one plot. See e.g. Figure A1 C in Segschneider et al. (2018). Such plots draw a clear picture and allow all readers to assess the reliability of model results.

Segschneider, J., Schneider, B., Khon, V., 2018. Climate and marine biogeochemistry during the Holocene from transient model simulations. *Biogeosciences*, 15, 3243-3266.

More specific comments:

Line 9-10: the authors wrote 'In the seasonal pCO₂ cycle, temperature plays the major role in determining surface pCO₂, except where DIC delivery is important in summer upwelling areas.' The first part of the sentence is correct in so far as temperature influences the solubility of CO₂ in water, but the sentence is confusing since temperature is no process. It is a physical quantity and results from the interplay of different physical processes. These processes control the surface pCO₂ via their impact on temperature and I would have expected to learn something about processes controlling the pCO₂.

Line 11: the authors wrote: 'We find that primary productivity during both summer and winter monsoon blooms, but also generally, is insufficient to off-set the physical delivery of DIC to the surface, resulting in limited biological control of CO₂ release.' To my understanding, it is the export production rather than primary production, which should at least partially offsets the physical delivery of DIC to the surface.

Line 17: Please clarify the term 'Reynolds decomposition'. In line 223 the term was also used and Doney et al. (2009) was cited but the name 'Reynolds' was not mentioned in this

paper.

Line 19 – 23: the author wrote: 'Since summer monsoon winds are critical in determining flux both directly and indirectly through temperature, DIC, TA, mixing, and primary production effects on pCO₂, studies looking to predict CO₂ emissions in the AS with ongoing climate change will need to correctly resolve their timing, strength, and upwelling dynamics.' Please clarify how wind relates to parameters such as temperature, DIC, TA and processes such as mixing and primary production.

Line 197- 198: What about upwelling and eddies? To my understanding the advantage of the high-resolution model was to resolve such processes.

Line 200 – 205: Since NPP stands for 'net primary production' (see e.g. line 231), I would suggest to replace 'PPNew+Reg' by NPP and to rename the term NPP-Remin. If I understood it correctly, the term 'NPP-Remin' represents the soft tissue and the alkalinity pump.

Line 206: Please name the two detrital pools. I guess that Det-remin represents bacterial respiration, is this correct?

Line 213- 215: Perhaps I misunderstood this part but deep DIC feeds the CO₂ emission into the atmosphere. Its annual cycle largely controls the air sea fluxes and without describing the deep DIC cycle there is no way to see whether the surface DIC dynamic operates correctly.

Line 220: Why the authors used this old 'K0' parameterization ?

Line 249 – 250: Since Biogeoscience has a wide readership and many scientists are interested in the topic presented by the authors, I suggest to present simple x-y plots in addition or instead of Taylor diagrams. Furthermore, I suggest to comment the results and say that the model results and field data reveal a weak correlation in spring and summer and do not correlate in winter and fall!

Line 255: This statement regarding the model performance refers only to surface data including primary production which poorly constrains the carbon cycle as it includes the regenerated production. As mentioned before, without getting an impression about the distribution of DIC, TA, and nitrate and oxygen in the water column, it is to my understanding impossible to judge the model performance.

Line 280: What is the role of sediments in this shallow water and acidic environments?

Line 328 – 330: Please rephrase this sentence and include all processes of relevance.

Line 332: and that is why the deep DIC cycle has to be included.

Line 338: This estimate agrees quite well to a result obtained from field data, which implies that organic carbon and CaCO₃ export removed 30 - 70% of the DIC introduced into the surface layer via physical processes. See Rixen, T., Guptha, M.V.S., Ittekkot, V., 2005. Deep ocean fluxes and their link to surface ocean processes and the biological pump. *Progress In Oceanography*, 65, 240 - 259.

Line 430: Yes, this is to my understanding the advantage of using a high repulsion model but apart from stating it, it should also be demonstrated.

Line 435: please consider also

Körtzinger, A., Duinker, J.C., Mintrop, L., 1997. Strong CO₂ emissions from the Arabian Sea during south-west monsoon. *Geophysical Research Letters*, 24, 1763-1766.

Goyet, C., Millero, F.J., O'Sullivan, D.W., Eiseid, G., McCue, S.J., Bellerby, R.G.J., 1998. Temporal variations of pCO₂ in surface seawater of the Arabian Sea in 1995. *Deep Sea Research I*, 45, 609-623.

Millero, F.J., Degler, E.A., O'Sullivan, D.W., Goyet, C., Eiseid, G., 1998. The carbon dioxide system in the Arabian Sea. *Deep Sea Research Part II: Topical Studies in Oceanography*, 45, 2225-2252.

Line 550: Is 120 correct? What about the 162.6 TgCyr⁻¹ as mentioned in line 393? Papers of relevance, which should have also been cited in this context, are:

Somasundar, K., Rajendran, A., Dileep Kumar, M., Sen Gupta, R., 1990. Carbon and nitrogen budgets of the Arabian Sea. *Marine Chemistry*, 30, 363-377.

George, M.D., Kumar, M.D., Naqvi, S.W.A., Banerjee, S., Narvekar, P.V., de Sousa, S.N., Jayakumar, A., 1994. A study of the carbon dioxide system in the northern Indian Ocean during the premonsoon. *Marine Chemistry*, 47, 243-254.