Response to Reviewer 2's Comments

Alain de Verneil Zouhair Lachkar Marina Lévy Shafer Smith

June 16, 2021

We all would like to thank Reviewer 2 for providing their comments in reviewing this manuscript (MS). It is very clear from the detail and thoughtfulness of the comments and questions that a lot of time and effort were spent in the review, which is both fair and will help produce a better paper. Below, we reproduce the review (lines preceded with >), with our responses interspersed within along with updated text from the MS in italics. Changes made to the original text show red and blue for deletions and additions, respectively.

>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.

Indeed, the Reviewer is perceptive and has focused on the primary tension in the paper. Regarding the base claim, that the high-resolution model is superior to a more coarse-resolution model, at the very least this is true if one glances at Figures 3, 4, and 7 from the metanalysis for RECCAP by Sarma et al. (2013). In that study, we see how the coarse-resolution models under-estimate flux. By comparison, the estimate in this study, despite its limitations and high flux value, we argue is more in line with observational studies. For example, in Sarma (2003), the statistical fits for DIC and TA from the JGOFS data are quite impressive, and the ultimate CO_2 flux estimate is 90TgCyr^{-1} for the Arabian Sea (AS) *north of 10^{o}N*. Since our domain also includes the AS from the equator to 10^{o}N, which by all accounts is also a net outgassing region, arriving at a CO_2 flux >100TgCyr^{-1} does not seem quite as unreasonable.

For our part, the greatest uncertainty is the balance between slightly under-estimating pCO_2 in the summer, and over-estimating it the rest of the year. First, our under-estimation of pCO_2 is not as bad as the climatological products such as L15, and so estimated summer fluxes will be better. Second, for the rest of the year, when we over-estimate pCO_2, winds are not as strong, so excess CO_2 flux is limited. What the overall balance is, whether the over-estimate of CO_2 flux outside of summer is larger than the under-estimate during the summer, remains our greatest uncertainty.

Furthermore, an unappreciated factor to consider is the model's high-resolution, which increases the total area considered. For example, when we restricted our model output to the 1^{o}x1^o grid of the World Ocean Atlas, GLODAP, and the L15 climatology, this reduced the flux from 160 to 120 TgCyr^{-1}. Reviewer 1 commented on the discrepancy between these two values (and it's commented on below, as well), and so in the revised MS this will also be highlighted.

In summary, all this is to say that, despite our hedges, we are still confident that previous CO_2 flux estimates are probably too low, and that the AS from the equator northward probably outgasses >100TgCyr^{-1}.

The revised MS, then, will clarify that despite sources of uncertainties, the model result should be considered seriously as an indication that previous CO_2 estimates are too low.

```
>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.
```

The wish to discuss processes such as primary production, surface heating, and upwelling advection, more than "parameters" or "variables" such as TA and DIC, is understandable and is also mentioned by Reviewer 1, as well. As written in our response to that review, we are amenable to conducting a similar analysis for TA, temperature, etc. as has been done for DIC, so that all aspects of how underlying processes impact the parameters determining pCO_2 can be ascertained. The complete analysis will have to be completed as part of the revision, but below please find a figure similar to Fig. 8 but for TA:

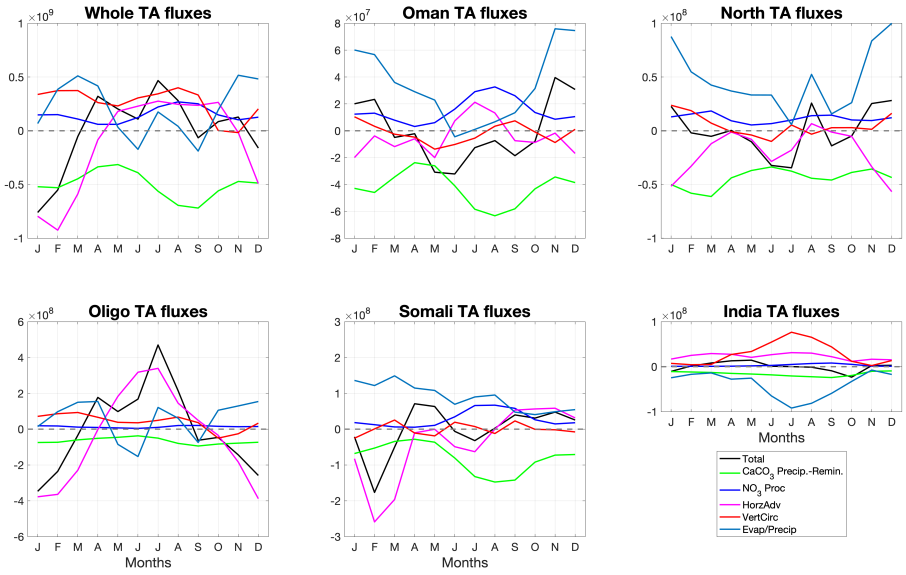

Fig. R2-1. TA budget similar to Fig. 8 for domain (top-left) and the five sub-regions.

```
>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.
```

Yes, the Arabian Sea is an interesting region because denitrification in low-oxygen environments and nitrogen fixation (mostly at the surface) impact the nitrogen cycle. The biological model uses a nitrogen currency, with a fixed C/N ratio, and explicitly includes denitrification, but unfortunately not nitrogen fixation. Since this remark is included as an example regarding model validation, we will include a supplementary figure comparing model NO_3 (which is impacted by denitrification) with *in situ* data (see below):

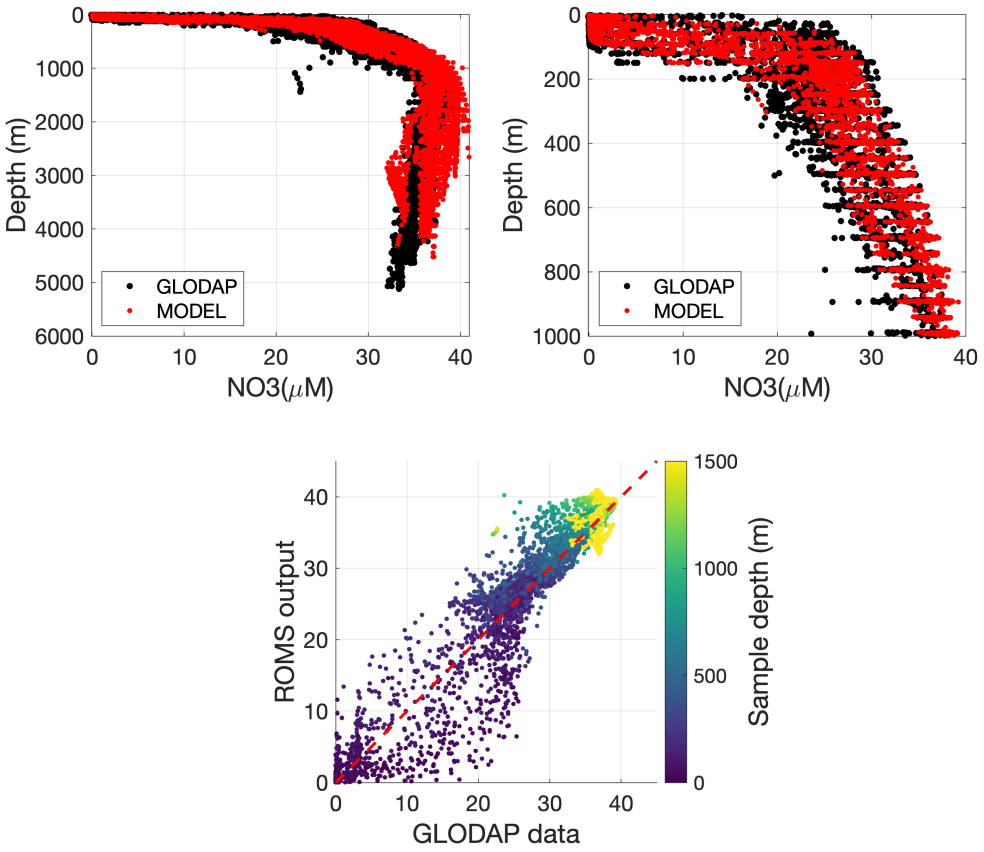

Fig. R2-2. (top-left) NO_3 profiles from GLODAP database (black) and ROMS model output (red) interpolated to same lon/lat/depth/month, and (top-right) the same but zoomed into the top 1000m. (bottom) Model output plotted against data, with 1:1 line (red dashes) and colored according to sampling depth.

It's true that processes such as denitrification and nitrogen fixation will impact Redfield ratios in the water column, especially the N:P ratio. Unfortunately, the model does not resolve Phosphate, and so aspects such as a change of limiting nutrient is not possible. C is not likely to be a limiting nutrient, as reflected in the inorganic pools, DIC and DIN (NO_3+NO_2), where typical deep values of DIC vs NO_3 (the major component of DIN) are 2300:35μM, giving a C:N ratio closer to 65:1 than the Redfield 6.6:1. If the reviewer has a specific dataset or quantity with which to help better validate the model, we are more than happy to include it as part of the Supplementary material.

```
>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!

We appreciate this focus on $CaCO_3$, especially since resolving issues here can help with the model's bias in total alkalinity. The reviewer notes that primary production rates were compared to satellite data but not $CaCO_3$ production rates. Having looked into the literature, we were able to find $CaCO_3$ production rates that date from the JGOFS years in the database located at https://doi.org/10.1594/PANGAEA.888182. The median production rate for this dataset is 0.03 $mmolCm^{-3}day^{-1}$. Let's extend this rate to the top 50m of the water column, which is close to the measured euphotic zone depth (~1% surface PAR in the $CaCO_3$ dataset). This results in $1.5mmolCm^{-2}day^{-1}$, or $6.57gCm^{-2}yr^{-1}$. Using the 7% fixed ratio to NPP, this means a median primary production rate of $94gCm^{-2}yr^{-1}$. Most of the sampling locations (being JGOFS) are near the Arabian peninsula and moving slightly into the central AS (see figure below):

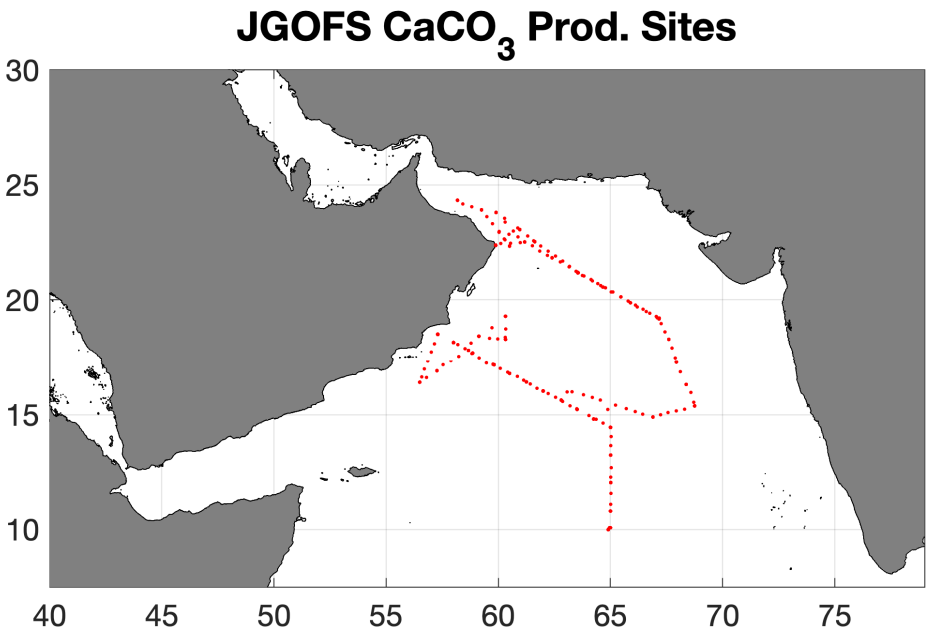

Fig. R2-3. Locations of $CaCO_3$ carbon production from the Poulton et al., 2018 dataset.

Judging from the NPP rates in Figure 1, a median primary production rate of $94gCm^{-2}yr^{-1}$ might be hard to justify for the central oligotrophic region, but in the productive zones this would be not difficult to achieve. As a result, the ratio is not inconsistent with the implied primary production that the $CaCO_3$ rate measurements suggest.

Still, it is true that the model is too simple in other ways, and perhaps can be better calibrated

to *in situ* data. The current formulation of the model has constant rates for many processes, and does not take into account ambient conditions such as $CaCO_3$ solubility in acidic waters. If the reviewer has a relevant dataset at hand with carbonate sediment distributions and concentrations, it would be much appreciated for further model refinement and validation should the step be taken to explicitly include these processes.

Also, with regard to Forams and their particular sinking speeds, it may be worthwhile to investigate the magnitude of these fluxes and impact on TA to judge whether including them specifically has merit. We the authors have no particular qualm with forams, but as it is, the model has only one component each for all the complexity of the phytoplankton and zooplankton assemblages in the AS, so each additional component's numerical expense must be compared to its relative value in resolving biogeochemical processes.

```
>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 asses 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.
```

Thank you for providing a reference from which we could produce similar Supplementary Figures to aid in the better validation of our model. Please find in the three figures below comparisons between the GLODAP dataset (which includes the available JGOFS data for DIC, TA, and O_2) and our model output, which will be added as Supplementary Figures to our revised MS.

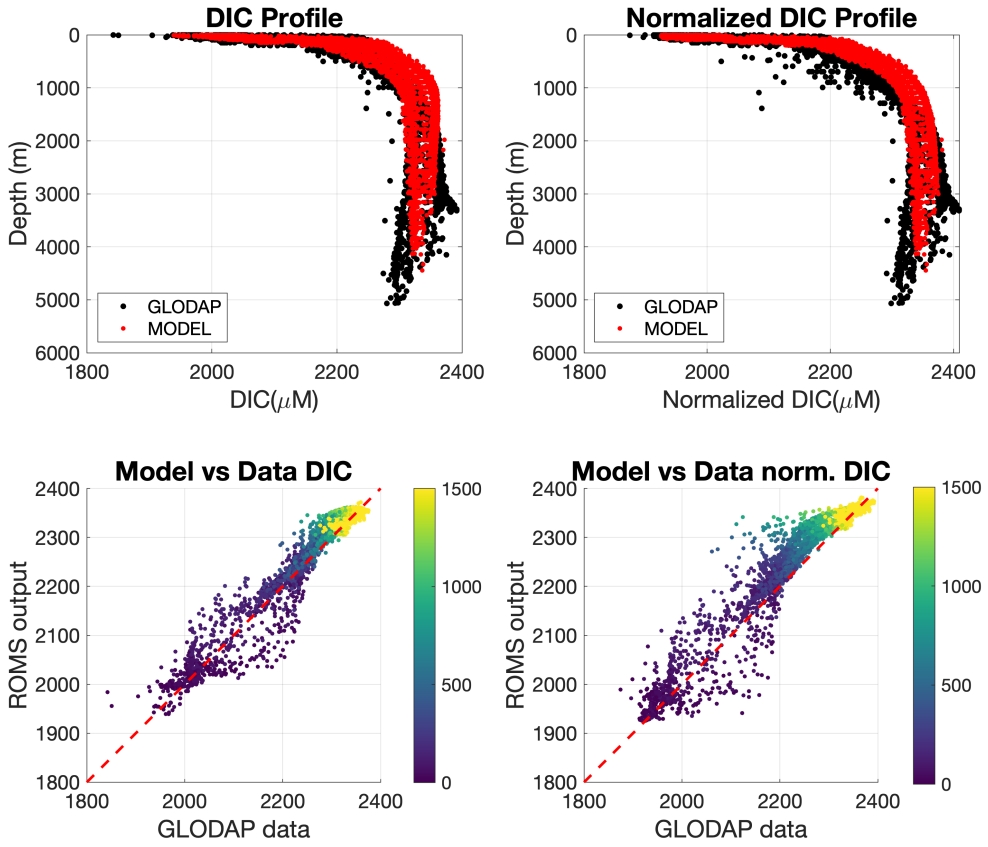

Fig. R2-4. (top-left) DIC profiles from GLODAP database (black) and ROMS model output (red) interpolated to same lon/lat/depth/month, and (top-right) the same normalized to S=35.. (bottom-left) Model output plotted against data, with 1:1 line (red dashes) and colored according to sampling depth, and (bottom-right) same but for normalized data.

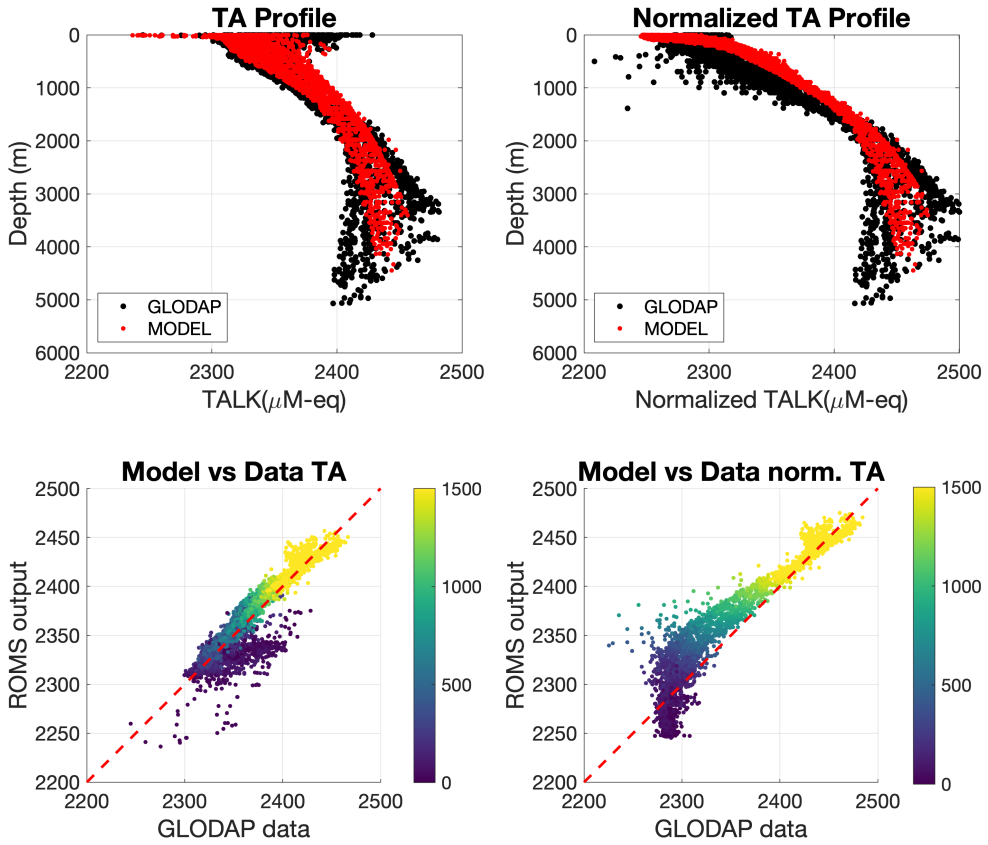

Fig. R2-5. (top-left) TA profiles from GLODAP database (black) and ROMS model output (red) interpolated to same lon/lat/depth/month, and (top-right) the same normalized to S=35.. (bottom-left) Model output plotted against data, with 1:1 line (red dashes) and colored according to sampling depth, and (bottom-right) same but for normalized data.

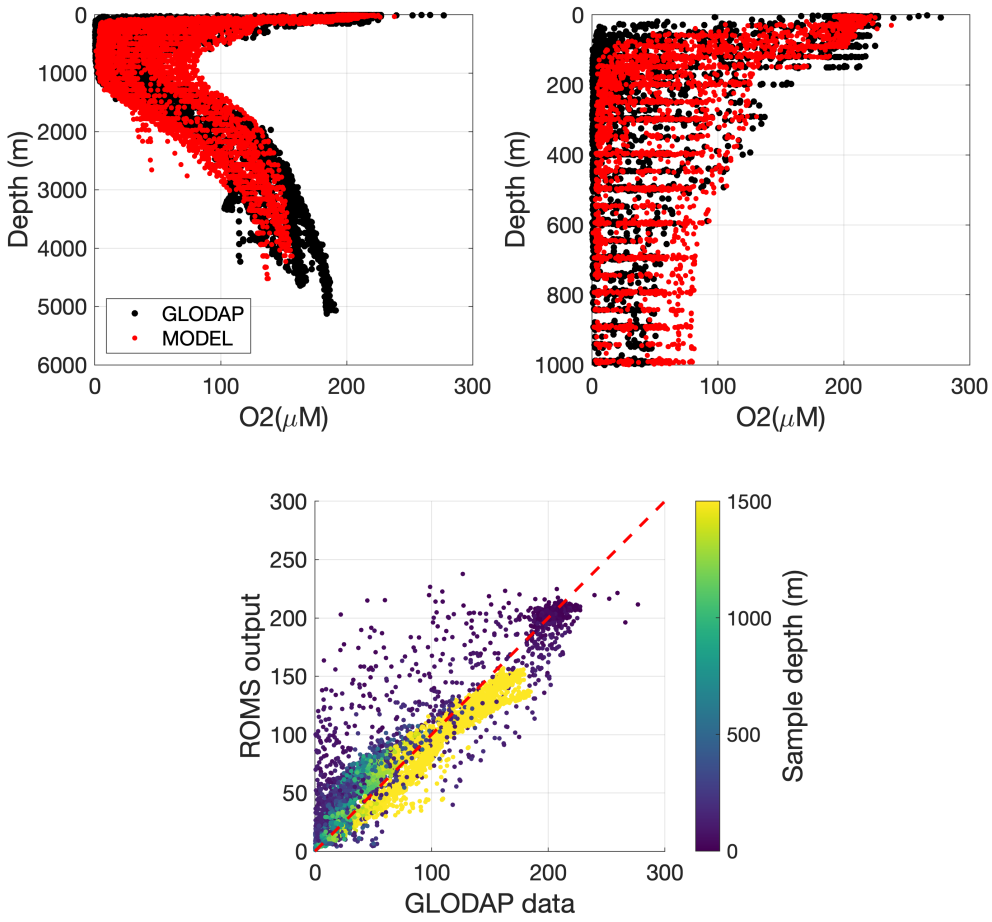

Fig. R2-6. (top-left) O_2 profiles from GLODAP database (black) and ROMS model output (red) interpolated to same lon/lat/depth/month, and (top-right) the same but zoomed into the top 1000m. (bottom) Model output plotted against data, with 1:1 line (red dashes) and colored according to sampling depth.

Hopefully showing the model output at depth in comparison with the available *in situ* data helps allay some of the reviewers' fears regarding some of the model's weaknesses in TA and DIC.

```
>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.
```

Yes, as already mentioned Reviewer 1 also raised the concern that the word "process" is used inappropriately, and towards that end, again we propose to extend our DIC analysis to TA, SST

and sea surface salinity (SSS) to explicitly account for how all the "processes" translate into changes of variables impacting pCO_2.

>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.

Yes, in the global sense of the entire water column, export production is what removes fixed C from the DIC pool and moves it to deeper depths. Since our control volume for the decomposition of pCO_2 variables and DIC processes are entirely in the surface layer we focused on the first step, that of fixation via primary production, and not the export component from the euphotic zone. Of course, some of this production gets remineralized before it can get exported, which is included in the DIC budget presented in the MS. In the revised MS, we will to include particulate C export in the discussion to provide an estimate of how much primary productivity is ultimately exported in the model.

>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.

Thank you for pointing this out, we should more explicitly explain. Reviewer 1 also had a question about the "five-year average," which is necessary because in Reynolds averaging fluctuations are around a specific time-average, and and as a result in order for the equations to be exact so we used the entire model timeseries. We will add a quick addition to the revised MS outlining what we mean:

...here we use a Reynolds decomposition (Doney et al., 2009). Briefly, a Reynolds decomposition takes a timeseries and divides it into a temporal mean and fluctuating component. When applied correctly, multiple terms can be produced in isolation showing their fluctuating contribution to the total.

>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 temperature, DIC, TA and processes such as mixing and >primary production.

Yes, we did not explicitly state how winds play a role in the multiple variables impacting not only air-sea CO_2 flux, but also SST, DIC, etc. While in lines 352-355 we suggest that wind forcing both upwells subsurface DIC and provides mixing, also enhancing DIC. However, it must be stated that these same processes (upwelling and enhanced mixing) will entrain colder waters from below and result in reduced SST and increased SSS.

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

Yes, it is true that one of the purposes of having a high resolution model is to be able to include phenomena such as coastal upwelling and mesoscale eddies. The "vertical circ." term in our budget encompasses the coastal upwelling, and its impact should be visible for the Oman sub-region in Figure 8.

As for eddies, a previous study cited in our MS, Resplandy et al. (2011), found that eddies are an important export mechanism of nutrients into the central, oligotrophic region of the AS. In order to treat eddy flux explicitly, we would have to choose another analysis method to identify mesoscale structures and follow them in time, possibly across multiple years in our 5-year time-series. However, since the horizontal advection term of the DIC budget (see Fig. 8) was never the largest magnitude flux, focusing on mesoscale export did not figure into the present analysis, and eventually in lines 504-507 we hypothesize that possibly the timescale for DIC uptake and removal is short.

>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.

Thank you for the note, it will be clearer to the reader if a consistent notation of NPP is used, and this will be changed in the revised MS. The NPP-Remin (Biology) grouping in Eqn. 6 does include the soft tissue pump, as well as the alkalinity ($CaCO_3$ component) pump.

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

The two detrital pools are designated "large" and "small," primarily to accommodate for faster sinking of larger particles, as well as for aggregation of smaller particles into larger ones. Reviewer 1 has requested that we include the model parameters used, and so the differences between these two detrital pools will be clearer in the revised MS. The reviewer is correct, Det-remin represents bacterial respiration of biomass, which will be clarified in the revised MS:

and Det_{remin} is remineralization of both detrital pools, i.e. bacterial respiration.

>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.

The reviewer did not misunderstand, ultimately some part of deep DIC will find its way to the surface and feed CO_2 emission. However, determining how deep, and from where, is a worthwhile follow-on study in itself that would probably require using Lagrangian methods. The reviewer is right to be curious about the ultimate sources of AS CO_2 emission, but at present this is beyond the scope of this study.

Hopefully, with the addition of deep *in situ* data as part of our validation, the reviewer can make an educated judgment regarding whether the surface DIC dynamics operates correctly.

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

True, at this point the Wanninkhof 1992 parameterization is dated, and in other studies the 2014 formulation from the same author is presently being used. Similar to our response to Reviewer 1's questioning of our use of older climatological datasets, we chose the old K0 representation so that our model results could be compared to previous studies in the region. In particular, Sarma et al. (2003) included the 1992 K0 formulation. We will add an explanatory sentence in the revised MS:

...positive outward from the ocean. The choice of Wanninkhof (1992) for the solubility parameterization was for direct comparison with previous studies, despite the fact more recent formulations are available, such as Wanninkhof (2014). The objective...

```
>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!
```

The reviewer is correct, Biogeosciences has a wide readership, and so accommodations must be made sometimes to make results digestible for scientists from other specialties. Due to the comments from Reviewer 1, we are already adding more Taylor diagrams, but including another subpanel showing data on an x-y plot would not be difficult; we will do this for the revised MS. Also, the reviewer makes a good point, we should comment on how the Taylor diagram shows weak correlations for some seasons but not others. In the revised MS will include this in the Results, including especially the number of observations, etc.

```
>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.
```

Yes, thank you, we hope that by adding the plots shown in our responses above that we have addressed this concern.

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

We presume that the author is referring to the west coast of India, since most of the other coastlines in the region have limited continental shelves. It is true, a lot of remineralization must occur in the sediments, and is a source of DIC (considering how shallow, a large part of the "vertical circ" is probably the mixing of sediment-derived DIC to the surface. We will both add an estimate of total annual DIC production in the sediments within the "India" subregion of our model analysis and include it in the discussion of the revised MS.

```
>Line 328 - 330: Please rephrase this sentence and include all processes of
>relevance.
```

Yes, very well, in the revised MS this sentence will read:

Whereas SST and its effect on pCO2 is controlled by the physical processes of surface forcing, mixing and advection, DIC reflects both physical and biological processes because it is also impacted by photosynthesis, respiration, remineralization, and shell calcification.

~~Whereas~~ *SST's* ~~and its~~ *effect on* pCO_2 ~~is controlled by the~~ *reflects physical processes* ~~of~~ *like surface* ~~forcing~~*heating and cooling, mixing and advection.*~~,~~ *DIC, by contrast, reflects both physical and biological processes because in addition it is also impacted by photosynthesis,* $CaCO_3$ *shell formation and dissolution, zooplankton respiration, detritus remineralization (bacterial respiration), and* ~~shell calcification~~ *air-sea exchange.*

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

Duly noted, we understand why the reviewer would like to see a budget that includes these deeper processes. As mentioned previously, analysis of the entire water column may serve as a follow-up paper of the entire Carbon cycle of the AS, but begins to draw away from the focus of air-sea CO_2 flux, which is the primary reason for this study.

>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.

We thank you for mentioning this reference, we will include it in our revised MS, and hint towards follow-up work including the entire water column.

>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.

We undestand that we should explicitly demonstrate how increased resolution results in a better representation of upwelling. The correct comparison would be with a model of $\sim 1^o$x1^o. Unfortunately, at these scales the mesoscale is completely absent, and it becomes necessary to parameterize their impact on tracer mixing. Since ROMS as a regional model is not designed to do this, it would require either coding up the paramterization, or using another more traditional GCM model. We know this is not satisfying, but all we can suggest is to compare with the model outputs presented in Fig. 4 of Sarma et al. (2013), which we will again emphasize in our revised MS.

>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., Eischeid, 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., Eischeid, G., 1998. The
>carbon dioxide system in the Arabian Sea. Deep Sea Research Part II: Topical
>Studies in Oceanography, 45, 2225-2252.

Thank you for these references to demonstrate this point; while we use both Goyet et al. 1998 and Millero et al. 1998 elsewhere, we are also happy to include Kötzinger et al. 1997 as well.

>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.

Reviewer 1 also noted the 120 vs 160 $TgCyr^{-1}$. The reason we report 120 $TgCyr^{-1}$ in this part is because in order to compare between pCO_2 sources, we had to use the same grid/surface area. Therefore, when we interpolate the model from the natural grid to the 1^ox1^o WOA/GLODAP/Landschützer grid, it results in a net loss of almost $40TgCyr^{-1}$! Again, thank you for the relevant citations, we will include them in the revised MS.
