Response to Reviewer's Comments

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We are pleased that the changes made to the manuscript were found to be sufficient for Reviewer 1 and mostly satisfactory for Reviewer 2. We thank both reviewers and the editor for the time and work put into improving this manuscript. As in the previous iteration, below we reproduce the reviewer's suggestions (lines preceded with >), with our responses in plain text and updated text in italics. Changes will be in red and blue for deletions and additions, respectively.

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>Comments:

>Abstract
>Line 16: I would suggest to avoid the term 'Reynolds decomposition' in the
>abstract as this term is not familiar to many readers and requires an
>explanation which the authors provide in line 199 only.
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True, and considering that the emphasis within this paper is not specifically this technique, we agree to remove this from the abstract:

*... versus **6%** for pCO_2 ~~in a Reynolds decomposition~~. In comparison with other...*

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>Introduction
>Line 32: was Lachkar et al., 2016 this first and only one who discovered that
>the world's thickest oxygen minimum zone was located in the Arabian Sea? See
>e.g. Acharya and Panigrahi, 2016, in Deep Sea Research Part I, Vol. 115 Pages
>240-252.
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We did not mean to imply through the reference that this was the first time that it was stated that the Arabian Sea's OMZ is the world's thickest. We will add these references:

...in addition to unique features such as the world's thickest oxygen minimum zone (OMZ) (Morrison et al., 1999; Acharya and Panigrahi, 2016; Lachkar et al.,2016) and corresponding Carbon Maximum Zone (CMZ) (Paulmier et al., 2011).

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>Line 51 : please clarify the meaning of 'similar story'
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Understood, by 'similar story' we were trying to make the point that surface pCO_2 data in the AS is also dated, with most data (>98%) before 2000. We will add this clarification in the revision:

...more recent than 1998, with a similar ~~story~~ >98% of data predating 2000 for pCO_2.

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>Methods
>Line 117: is this consistent with the latest IPCC report?
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The reviewer is right, in CMIP6 (the most recent iteration of the models used in the IPCC) the historical models now run until 2014, as opposed to 2005 in CMIP5. We will correct the MS to reflect this detail:

The year 2005 is chosen for the model's xCO_2 concentration because it is the end of the historical period for the Intergovernmental Panel of Climate Change (IPCC) models used in its 5th report published in 2014.

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>Line 118/119: please rephrase this sentence which is really difficult to
>understand
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We will rephrase the sentence as follows:

As a result, we assume there is ~~in using them this present analysis both assumes the existence of and attempts to quantify~~ a baseline seasonal cycle of pCO2 and air-sea CO2 flux ~~cycle that~~ which has held ~~has remained~~ stable over the past decades.

To read:

As a result, we assume there is a baseline seasonal cycle of pCO_2 and air-sea CO_2 flux which has held stable over the past decades.

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>Line 153: Due to high sinking speeds of foram-shells and their significant
>contribution to the carbonate fluxes in AS sediment traps (e.g. 1) , I would
>suggest to include at least a short note saying that this low sinking is a
>simplification. Since the authors, furthermore, state that the TA bias cause
>the overall positive pCO2 bias (see line 340) and enhanced carbonate export
>via foram-shells might have solved these issues, I would also recommend to
>include the aspect of too low sinking speeds into the discussion of the TA
>bias in line 340.

>[1] Curry, W. B., Ostermann, D. R., Guptha, M. V. S., and Ittekkot, V. (1992)
>Foraminiferal production and monsoonal upwelling in the Arabian Sea: evidence
>from sediment traps, In Upwelling Systems: Evolution Since the Early Miocene
>(Summerhayes, C. P., Prell, W. L., and Emeis, K. C., Eds.), pp 93-106,
>Geological Society Special Publication.
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We will first add a note to line 153 as suggested:

In addition to usual physical transport and mixing, $CaCO_3$ is allowed to vertically sink at 20m day^{-1}. The chosen sinking rate is a simplification in that it does not include the faster rates observed for foraminifera shells (Curry et al., 1992), which as a biological group are not resolved by the biological model due to numerical constraints. Organic carbon...

We will also add to line 340:

As a result, while the DIC model bias lowers pCO_2, the stronger bias in TA is the most likely cause for the model's overall positive pCO_2 bias, which may in part be due to the unresolved fast sinking rates of foraminifera in the model.

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>Line 167 Please delete 'in this study' and change 'chnaged' into 'changed'.
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Thanks for spotting the typographic errors, the changes have been made in the MS.

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>Line 150 delete 'i.e. remineralization'
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This deletion has been made in the MS to 'i.e. remineralization', although this occurs (in the marked-up version that appears to be used so far) at line 259:

...is remineralization in both detrital pools~~, i.e. remineralization~~

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>Line 188: Please describe this in more detail. For instance, Figures R2-3 and
>2-4 show that model data reflects general trends but there are also
>deviations between GLODAP and model data. These deviations are of great
>relevance as they are most pronounced in water-depths of approximately <500m.
>Processes within this depth-range strongly influence the pCO2 in the surface
>waters.
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We assume the reviewer is referring to line 288 (in the latest mark-up version), which references depth profiles of NO_3, O_2, DIC, and TA. We will add the following:

Depth profiles of nitrate, oxygen, DIC, and TA are similarly conserved (Fig.S3-S6). Nitrate, DIC, and TA all show their usual nutrient-like profiles, while oxygen is its minimum within the OMZ. The deviations seen between in situ data and model output are greatest at depths less than 500m. Deviations in near-surface NO_3 (Fig. S3) can be large for intermediate values (5-20μM) but overall do not show a systematic bias. DIC (Fig. S5) also has large deviations ($\sim$50μM) in the top 500m and with a slight positive bias. It is in TA (Fig. S6) that deviations, while similarly $\sim$50μM-eq, show a consistent near-surface underestimation. The~~S~~surface currents in the model also demonstrate...

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>Results
>In general: please check the grammar and the readability. In many cases
>additional information has been squeezed into sentences which reduces the
>readability of the manuscript and especially the results section.
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Yes, we understand that in trying to add all the quantitative values reviewer 1 requested that the readability of the MS has suffered. Please find changes in the updated MS and mark-up version.

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>Line 294 please check grammar
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Thank you for the pointer, we checked the following sentence for issues on grammarly.com, and while there do not seem to be specific grammar issues, the sentence is certainly not concise. We propose the following:

Sampling dates for~~The monthly distribution of~~ pCO_2 ~~sampling~~ (Fig. 2b) ~~also~~ show~~s~~ that ~~the majority of data ($\sim$70%) come~~ are from the summer monsoon months (June-September, JJAS)~~.~~. Most ~~and that most~~ observations similarly date from the 1990s, with 1995 and 1997 alone accounting for 96% alone~~coming from the years 1995 and 1997 alone.~~

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>Line 404 -418. Please rephrase this paragraph. It is difficult to follow and
>to understand from where the numbers are coming from.
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Please find our preposed rephrasing below:

All calculations have their peak CO_2 flux sometime in the summer, confirming the role of winds in CO_2 flux timing. ~~After calculating total flux for both the entire AS and the Sarma (2003) reduced domain, this study~~ This study's model consistently produced on of the higher estimates with

~~120TgCyr^{-1}, less than 162.6~~ 120 TgCyr^{-1} ~~due to area reduction in the~~ (reduced from 162.6 due to re-gridding ~~process (however, GLODAP in the reduced domain emits 65TgCyr^{-1}~~) and 57 TgCyr^{-1} ~~; Fig.9b). This~~ north of 10^{o}N. The only estimate higher than the model is GLODAP data in the region north of 10^{o}N with 65 TgCyr^{-1} possibly driven by summer monsoon sampling bias. The high model estimate is perhaps unsurprising, considering the pCO_2 bias. The ~~ratio between the largest and smallest~~ range in estimates of total ~~CO_2 flux is 2.1 (57 vs 120)~~ CO_2 flux is 57-120 TgCyr^{-1} ~~) for the whole domain, and 5.3 (~~ , resulting in a ratio of 2.1x variability. In the reduced domain of the AS north of 10^{o}N, estimates range from 12.3 ~~vs~~ to 65.6 ~~TgCyr^{-1}) for the reduced domain. It should be noted that application of~~ , resulting in a ratio of 5.3x variability. The 5.3x ratio is quite high, and is in part driven by the low estimates from the Sarma (2003) model, which are 12.3 and 17.6 using tracer data from WOA and ROMS, respectively. Indeed, the Sarma (2003) model ~~resulted in negative ΔpCO~~estimates have negative CO_2 ~~values. While some negative values were reported~~ flux for some months, which is not observed in the original publication, ~~the total fluxes calculated here (12.3 or 17.6 TgCyr^{-1}) were~~ and the fluxes are quite smaller than the 70 TgCyr^{-1} reported ~~using the same parameterization from Wanninkhof (1992). Removing the two Sarma models from the reduced domain analysis produces~~ in the original publication. If the two lower estimates are removed, the range in air-sea CO_2 flux in the domain north of 10^{o}N is 41-65 TgCyr^{-1}, providing a ratio of 1.6 ~~between the largest and smaller flux values, more in line with the whole domain's variability. Additionally, the GLODAP data, with no temporal variability in~~ similar to 2.1 for the whole domain. Even considering the model's pCO_2 ~~, probably over-estimate pCO_2 and hence flux due to sampling bias near Oman during the summer monsoon, and so the ratio between flux estimates may indeed be smaller. As a final note, another interesting detail concerns how the original estimate of~~ bias, as mentioned the GLODAP estimate supersedes it in the region north of 10^{o}N, as does the original Sarma (2003) ~~at~~ estimate with 70 TgCyr^{-1} ~~is larger than both the model, which had 57.1 TgCyr^{-1}, and GLODAP data~~. Thus, while ~~the model pCO_2 bias makes us~~ we may think the model over-estimates flux, it is still within the range of previous studies in the AS.

>Discussion
>Line 559: heading is missing

In the mark-up version we are referring to, it is not clear if there is a heading missing. However, it is possible that in the mark-up process of "latexdiff" the appearance of headings may not reflect the final version. For example, on line 705 the section number is 4.3.1, whereas in the updated MS it is 4.3.3 (line 594), as it should be.

>Line 655 'in' ?

This sentence will be rewritten to be more clear:

Indeed, in a scenario where the ~~one of the largest~~ cross-term contribution~~s~~ is at its maximum amplitude, the ~~in~~ Omani upwelling region during ~~the~~ summer (Fig. 8b), ~~but this clearly is~~ the cross-term is not strong enough to sway the direction of the flux anomaly.

>Line 684: please clarify this sentence

Yes, the way the sentence is written, it reads as if vertical circulation and biological processes are not important. Rather, we meant to simply say that they are not as dominant as in the DIC cycle. We will re-write it as follows:

A preliminary TA budget of the model (Fig. S12) shows that ~~unlike~~ while vertical circulation and biological processes strongly dominate the seasonal cycle of near-surface DIC, TA ~~does not have vertical circulation and biological processes as the dominant~~ has multiple forces influencing its time evolution. However, ~~Additionally~~ the magnitude of the...

>Conclusion
>Line 822 Please clarify of what is a ‘pH relevant biological threshold’.

We will change the sentence to include an example of a biological threshold related to pH:

...whereas other important indicators such as pH and aragonite saturation, Ω_a, which at important thresholds have deleterious impacts for various biological taxa (Doney et al., 2009; Bednarsek et al., 2019; Bednarsek et al., 2021) ~~and its relevant biological thresholds~~ will be less so.