Response to the referee #1's comments on "Importance of El Niño reproducibility for reconstructing historical CO2 flux variations in the equatorial Pacific" by Watanabe et al.

Reply to referee #1:

Thank you very much for invaluable comments and suggestions on our original manuscript. According to the comments, we have revised the manuscript. In addition, following the comment we received personally from the National Oceanographic Centre, UK. (For their comments and our reply, see Pages 7–16), we have combined Section 3.2 and 3.3 in the original manuscript and reorganized. We think the revised manuscript is now more readable. We hope that the revised manuscript meets your approval and will be more suitable for publication in the journal.

Reply to comments:

(Referee #1) "The authors present an interesting work to compare the relationship of air-sea carbon fluxes to ENSO in the equatorial Pacific simulated by the two earth system models with assimilation and without assimilation, which are developed by the same institute. What's more interesting in this paper is that the old earth system model with assimilation generated an incorrect upwelling during the El Nino period, which led to a great problem in the simulation of carbon fluxes, but the new earth system model did not. Although this work has not made much contribution to the study of the response mechanism of carbon fluxes to ENSO in the equatorial Pacific, it will be very helpful to the people who are interested in assimilation or the model development, especially to those who are interested in the simulation of the carbon cycle process in the equatorial Pacific, if we can find out why the old and new models have different performances after the same assimilation method is added. The content of the article fits within the scope of OS, but much work needs to be done before publication to refine the theme of the article, highlight key points, and give a more detailed discussion on the conclusions."

Thank you for taking the time to review the manuscript. We have revised the manuscript in accordance with the following comments. We will answer them point by point.

Major points:

1 Abstract Great changes are needed to reduce the description of the study significance and increase the discussion of the final result.

We have completely rewritten the abstract. First, the first two sentences in the abstract in the original manuscript have been removed for conciseness and clearness on the purpose of this study. We then have described in more detail what led to the failure of reproducing the observed anticorrelation between SST and CO2F in the eastern equatorial Pacific with the ESM with smaller-than-observed amplitude of ENSO.

2 Some descriptions need to be supplemented, such as the vertical range of assimilation. Are the temperature and salinity at the bottom of the mixed layer assimilated? That another content needs to be added is to compare the differences in the simulation of ENSO between the two models with assimilation and without assimilation, such as the periodicity and amplitude of ENSO.

In this study, the observed anomalies are assimilated into the ocean models at depths between the sea surface and 3000 m. We have rewritten Lines 103–104 of the revised manuscript as follows:

”In addition, the IAU was applied at depths between the sea surface and 3000 m, with the values of $\tau = 1$ day and $\alpha = 0.025$ (Tatebe et al., 2012).”

The original manuscript did not clearly state that we used “anomaly assimilation”. We have added the following sentences in Lines 100–103 in the revised manuscript:

“For $X^a(0)$ and $X(0)$, we used anomalies from monthly mean climatology during 1961–2000 in observations and models, respectively. Such a scheme often called ‘anomaly assimilation’ or ‘anomaly initialization’ is used in many previous studies (e.g., Smith et al., 2007; Keenlyside et al., 2008; Pohlmann et al., 2009; Li et al., 2016, 2019; Sospedra-Alfonso and Boer, 2020).”

We have added Table 2 showing intensities and periods of ENSO for NEW, OLD, and observations in the revised manuscript. The discussion on the intensities (periods) of ENSO has been added in Lines 181–183 (243–247) in the revised manuscript. The ENSO intensities and periods for NEW-assim and OLD-assim are the same as the observed values because of data assimilation.

3 In the old model with and without assimilation, the response of 10-m wind speed over the sea surface to NINO3-SST does not change significantly, but the response of sea water vertical velocity to NINO3-SST changes greatly (Figure 4). Dose the meridional wind change significantly?

In OLD-assim, the warming due to data assimilation procedure during El Niño periods reduces density at the depth of the thermocline in the eastern equatorial Pacific, leading to enhancement of upward vertical velocity. The patterns of zonal and meridional wind speed variation in OLD and OLD-assim are similar. To describe the mechanism causing vertical velocity anomalies more clearly, we have added Figure S3 as a supplement and modified Lines 233–237 in the revised manuscript as follows:

“The wind feedback in OLD-assim is 0.48 m s^{-1} K^{-1} (Table 3), which is the same as in OLD, and the map of the wind speed anomalies shows a similar pattern to that of the OLD (Figure S3e–h); however, the warming due to data assimilation procedure during El Niño periods reduces density, leading to low-pressure anomalies. This results in anomalous cyclonic circulation and convergence, and thus enhancement of upward vertical velocity at the depth of the thermocline (Figure 5d)”

4 Line 196, "howerver, the strong heating causes upwelling of DIC rich waters in the subsurface layers (Figure 6b)." Why does this strong heating occur? Is the simulated value of sea water temperature in the old model during the El Nino period lower than the data used for the assimilation? Please discuss in detail the reasons for the abnormal upwelling during the El Nino period in the old model with assimilation.

In this study, the observed anomalies are assimilated into the ocean models at depths between the sea surface and 3000 m (see Reply to Major points 2). In OLD, the temperature variations in the eastern equatorial Pacific is smaller than observed (Figure 3b and 3c), so that the correction term on the governing equation of the ocean temperature, which is introduced in the data assimilation procedure, forces to raise the equatorial water temperature during El Niño periods in order to realize observed temperature variations. To clarify this process, we have modified Lines 230–233 in the revised manuscript as follows:

“In OLD, the temperature variations associated with ENSO at the depth of the thermocline in the eastern equatorial Pacific is smaller than observed (see Figure 3b and 3c), so that the correction term forces to raise the equatorial water temperature by 0.16 × 10^{-6} °C s^{-1} during El Niño periods in order to realize observed temperature variations (Figure 5b). ”

In order to describe the process in which warming causes the enhancement of upward vertical velocity more clearly, we have rewritten Lines 235–237 as follows:

”the warming due to data assimilation procedure during El Niño periods reduces density, leading to low-pressure anomalies. This results in anomalous cyclonic circulation and convergence, and thus enhancement of upward vertical velocity at the depth of the thermocline (Figure 5d).”

5 After assimilation is added to the new earth system model, the response of upwelling anomalies to NINO3- SST is weakened (comparison of Fig. 6 with Fig. 8). This change in the response is actually similar to that in the old model. Does this mean that the current assimilation method is not suitable to the earth system model?

This study points out that, before discussing the assimilation methods, the performance of the model itself needs to be improved. We think that the reproduction of the observed anticorrelated relationship between SST and CO2F in the equatorial Pacific in NEW-assim indicates the usefulness of the MIROC-ES2L and the data assimilation method we used in this study.

However, we have to admit that MIROC-ES2L and the data assimilation method we used is not perfect. As the referee #1 pointed out, the assimilation scheme modifies the distribution of vertical velocity. At 140W, the upward vertical velocity anomaly during El Niño periods was -7×10^{-6} m/s in New, but it changed to about -5×10^{-6} m/s in the NEW-assim. The change in the upward vertical velocity from NEW to NEW-assim may be due to the fact that the ENSO intensity is stronger and the period is longer than in the observations, and response of vertical velocity in ENSO is still distorted by the temperature analysis increment in NEW-assim. To point out that the response of vertical velocity in ENSO is still distorted by the temperature analysis increment in NEW-assim, we have added the following sentences in Lines 243–248:

“As already discussed, the intensity of ENSO in NEW is slightly stronger than observed (Table 2). In addition, the period of ENSO, which is defined as the peak of the power spectrum of one-year running mean NINO-SST, is 5.0 years in NEW, which is longer than 3.5 years of observations (see Table 2). Because the ENSO characteristics in NEW are somewhat inconsistent with observations, model nature, namely responses of vertical velocity and DIC concentration in ENSO, are still distorted by the temperature analysis increment even in NEW-assim. This indicates that further model improvements are needed.”

We think that the further development of ESM and the use of more advanced assimilation methods may improve the performance of the model. Further investigation is required to identify the best suitable method and why.

Minor points:

1 Line 119, “three ensemble members”. How were the ensemble experiments conducted?

Were the initial fields of these experiments different?

Both of NEW and OLD are the exactly same as the historical simulations designated by CMIP6 and CMIP5 protocols, respectively, and they have three ensemble members which are bifurcated from arbitrary years of the corresponding preindustrial control simulations. NEW-assim and OLD-assim are bifurcated from NEW and OLD at the year 1946, respectively. We have added the following sentences in Lines 108–111 in the revised manuscript:
"Both of NEW and OLD are the exactly same as the historical simulations designated by CMIP5 and CMIP6 protocols, respectively, and they have three ensemble members which are bifurcated from arbitrary years of the corresponding preindustrial control simulations. The ocean data assimilation experiments, NEW-assim and OLD-assim, are bifurcated from NEW and OLD at the year 1946, respectively, and they are integrated up to the year 2005."

2 The statement of Line 149-151 is error. $(\partial pCO2/\partial T)\Delta T$ is not the term of changing the solubility of CO2.

The phrase "in CO2 solubility" in Lines 149–151 in the original manuscript has been removed.

3 How was the "temperature increment" calculated?

The phrase "temperature increment" in the original manuscript has been changed to "temperature analysis increment" in the revised manuscript. The method for calculating the temperature analysis increment is described in Sect. 2.1 in the revised manuscript, but here is a brief introduction. The analysis increment during the analysis interval from $t = 0$ to $t = \tau$ is calculated from $\Delta X^a = X^a(0) - X(0)$, where $X^a(0)$ is the analysis and $X(0)$ is the model first guess at $t = 0$; this term is held constant during the analysis interval $\tau = 1$ d. The monthly objective analysis data of ocean temperature and salinity (Ishii and Kimoto, 2009) were interpolated linearly to form daily analysis data, X^a.

4 Line 236-238, "The correlation between SST and CO2F in the equatorial Pacific is consistently represented only in the case where the ocean temperature and salinity observations are assimilated into NEW." This statement is ambiguous, because both OLD and NEW experiments can produce the relationship between the SST and CO2F.

We realized that the first paragraph of Discussion and Summary section, as in abstract (see

Reply to Major points 1), should be a concise statement of what we found in the study. We have removed the description of the background from the first paragraph of Discussion and Summary in the original manuscript and rewritten it so that there was no ambiguity. The relevant sentence has been changed to:

"In the case where the ocean temperature and salinity observations were assimilated into the other ESM with rather realistic ENSO representation, anticorrelated relationship between SST and CO2F was reproduced." (Lines 259–260)

5 Overall, the manuscript needs to be improved, including some language errors. "

We have reviewed the entire manuscript and revised it in accordance with the comments. Thank you again for your comments.

Reply to comments from the National Oceanographic Centre, UK.:

Thank you very much for invaluable comments and suggestions on our original manuscript. According to the comments, we have revised the manuscript. Here, we copied all your comments and answered to all your comments point by point using red font.

Reply to comments:

Main comments:

"Overall the study is important for the improvement of CMIP models and their ability to reproduce ENSO variability. The story is in good shape and we do not think new simulations are required. There are many points that need clarification and the wording needs tightening to avoid confusion in places. Some unanswered questions detailed below would improve this study and make it more widely applicable to other CMIP models. Overall, we think the study needs minor revisions.

Thank you for taking the time to review the manuscript. We have revised the manuscript in accordance with the following comments.

Minor Comments:

Abstract

Abstract needs to make it clearer what the research question - and answer is. Place the question clearly, perhaps phrase lines 13-15 with question. It is important to communicate in the abstract that the OLD model does not reproduce observations (it is assumed the reader already knows this), and the newer models does. The final line of the abstract is vague and could be (tersly) summarised with "new model is better than old". What are the consequences of this? Where does this work lead and what are the immediate implications?

Thank you for your comments. Following the comments, we have completely rewritten the abstract. To more concisely and clearly state the purpose of this study, the first two sentences in the original manuscript have been removed. We then have described in more detail what led to the failure of reproducing the observed anticorrelation between SST and CO2F with the ESM with smaller-than-observed amplitude of ENSO, and pointed out that the performance of the model is important when initializing an ESM.

Introduction

Introduction is long and takes a while to get to the main problem with the CMIP5 model. The main point of the paper, discrepancy between the observations and MIROC-ESM for El Niño amplitude and associated CO_2 flux, should be identified in the first paragraph more clearly rather than the end of paragraph 4 (lines 52-54).

Thank you for your advice. We have totally rewritten and shortened Introduction section in the revised manuscript. The main point of this manuscript described in fourth paragraph in the original manuscript has been moved to the first paragraph in the revised manuscript.

Lines 64-72, do we need the history of data assimilation in climate models to understand this paper?

We have removed the description of the history of data assimilation in climate models in the revised manuscript.

Lines 76-89, the last paragraph of the introduction should specify the question this paper will answer and set out the structure of the paper. The question is unclear. Instead, this paragraph has text about assimilation that should be in the methods section.

To clarify the purpose of this study in Introduction, we have moved some sentences describing ESMs and data assimilation methods in the original manuscript to Methods section in the revised manuscript.

Methods

Lines 99-104, the grid is very irregular was it interpolated? Is the model sensitive to grid (add ref)?

To the south of 63°N, spherical coordinates are used. Analyses of SST and CO_2 flux variations in the Niño3 region were performed using data from the original grid. In order to compare the air–sea CO2 flux of NEW-assim and OLD-assim to SOM-FFN dataset (Landschützer et al., 2016), model output is linearly interpolated into the SOM-FFN grid. To describe the interpolation, we have added the following sentence in Line 146 in the revised manuscript:

"The model output data were ensemble mean and linearly interpolated into the SOM-FFN grid."

Lines 99-104, the NEW model is deeper 5300 vs 6300. Is the model sensitive to this (add ref)? How is the model partitioned vertically? Sigma/z/hybrid coordinate? This is important since the stratification is a key part of your results.

The vertical level both in MIROC-ES2L and MIROC-ESM are in a hybrid σ–z coordinate system. We have added the phrase "in a hybrid σ–z coordinate system" in Line 82 and Line 85 in the revised manuscript, respectively. Since this study focuses on processes near the surface, we do not think the change in maximum depth has had much of an impact. Rather, increasing vertical resolution within the upper 500 m of depth has an impact. In order to describe the vertical resolution, we have added the following sentences in Lines 85–88 in the revised manuscript:

"The resolutions in MIROC-ES2L are higher than in MIROC-ESM. In particular, 31 (21) of the 62 (44) vertical layers in MIROC-ES2L (MIROC-ESM) are within the upper 500 m of depth. The increased number of vertical layers in MIROC-ES2L has been adopted in order to better represent the equatorial thermocline."

Line 118, the ensembles are only mentioned here. We need more detail. How are they different? Maybe use a table.

Both of NEW and OLD have three ensemble members which are bifurcated from arbitrary years of the corresponding preindustrial control simulations, and NEW-assim and OLD-assim are bifurcated from NEW and OLD, respectively. We have mentioned this by adding the following sentence in Lines 108–111 in the revised manuscript:

"Both of NEW and OLD are the exactly same as the historical simulations designated by CMIP6 and CMIP5 protocols, respectively, with three ensemble members for each which are bifurcated from arbitrary years of the corresponding preindustrial control simulations. The ocean data assimilation experiments, NEW-assim and OLD-assim, are bifurcated from NEW and OLD at the year 1946, respectively, and they are integrated up to the year 2005"

Line 133, equation is repeated in the text. Instead, define the variables here. For example, what is $(\partial pCO2/\partial Alk)\Delta Alk$?

In the revised manuscript, we have defined $C(X)= (\partial pCO_2/\partial X)\Delta X$ (X=T, S, DIC, Alk), as

pCO_2 change due to the change in X, and stated that Res. in Eq (3), which includes second-order terms (Takahashi et al., 1993), was estimated so that the left-hand side and right-hand sides in Eq. (3) are equal. (Lines 128–130 in the revised manuscript).

Methods is missing description of the boxes NINO3 and NINO4 for someone not familiar with ENSO analysis. Why are the boxes picked? A map would be useful here.

We analyzed CO2F in Niño3 region because this region shows maximum variability region for CO2F. We have added the map for standard deviations of CO2F anomalies derived from observation-based CO2F dataset SOM-FFN (Landschützer et al., 2016) (Figure S1). In the revised manuscript, to clarify why we focus on CO2F in this region, we have added the following sentences in Lines 136–139:

"It shows significant interannual variation of CO2F in some specific regions such as the equatorial Pacific and high latitudes of both hemispheres (Figure S1). In Sect. 3, we focus on the CO2F in the Niño3 region (5°S–5°N, 150°W–90°W) which shows notable variation of CO2F in the equatorial Pacific. This region is also the maximum variability region for SST (Gill, 1980)."

Niño4 region is the maximum variability zone for westerly wind. To show this, we have added the map for 10 m zonal and meridional wind anomalies (Figure S3), and added the following sentence in Line 199 in the revised manuscript.

"Niño4 region is the maximum variability region for the equatorial trade wind (Figure S3)."

We have added the boxes indicating Niño3 and Niño4 regions in Figure 1a and 1c in the revised manuscript.

Results 3.1

Terminology in results sections needs tightening up throughout. There are cases where increase and decrease are used when the positive and negative phase of ENSO should be referenced. More specific use of El Niño or La Niña would be helpful instead of ENSO signal.

The anomalies shown in Figures 2–5 show the ones during El Niño. Therefore, we have decided to discuss anomalies during El Niño from the climatic field in the revised manuscript. To make it clearer, we have added the following sentence in Line 165 in Section 3.1 in the revised manuscript:

"In the following, we describe the anomaly during El Niño periods, while the opposite applies during La Niña periods."

In addition, in order to describe more clearly our results, we have rewritten Sect. 3 entirely.

Line 149-156, this feels important but difficult to follow, please rephrase.

To more clearly describe the pCO2 change associated with the changes in DIC concentration and in temperature, we have rewritten Lines 149–156 in the original manuscript as follows:

"In NEW-assim, NEW, and OLD, pCO_2 decreases because the effect of the decrease in pCO_2 with decreasing DIC concentrations is larger than that of the increase in pCO_2 with warming (Figure 2). In OLD-assim, however, the effect of the increase in pCO_2 with warming is larger than that of OLD, and the decrease in pCO_2 with decreasing DIC concentrations is smaller than that of OLD, resulting in an increase in pCO_2." (Lines 166–169 in the revised manuscript)

Results 3.2

Line 179-180, this is confusing, enhanced SST anomaly during both positive and negative phases of ENSO?

In the revised manuscript, Sections 3.2 and 3.3 in the original manuscript have been combined into one and reorganized. (See reply to comments on Sect. 3.3) We have moved the description of the vertical velocity feedback in Lines 179–180 in the original manuscript to Line 227 in the revised manuscript. To make it clear that the positive value of vertical velocity feedback indicates the enhancement (weakening) of upward vertical velocity during El Niño (La Niña) periods, we have rewritten Lines 237–239 in the revised manuscript as follows:

"The positive value of vertical velocity feedback indicates the enhancement (weakening) of upward vertical velocity at the depth of the thermocline during El Niño (La Niña) periods, which is inconsistent with observations."

Line 196, why does strong heating cause upwelling? This needs better explanation.

The correction term in the governing equation of the ocean temperature leads to decrease in density during El Niño periods, causing enhancement of upward vertical velocity. In order to more clearly state this, we have rewritten the manuscript as follows:

"the correction term forces to rise the equatorial water temperature by 0.16×10^{-6} °C s^{-1} during El Niño periods in order to realize observed temperature variations (Figure 5a)." (Lines 232–234) “the warming due to data assimilation procedure during El Niño periods reduces density, leading to low-pressure anomalies. This results in anomalous cyclonic circulation and convergence, and thus enhancement of upward vertical velocity at the depth of the thermocline (see Figure 5d).” (Lines 235–237).

Line 198, is it upwelling like in La Niña or is it upward mixing that means a smaller SST increase than would be expected for El Niño. The terminology needs to be tighter here. Please check phrasing like this throughout.

The positive value of vertical velocity feedback, discussed in Line 198 in the original manuscript, indicates the enhancement (weakening) of the upward vertical velocity during El Niño (La Niña) periods, which in fact should not be occurring. To make it clear that the enhancement (weakening) of upward vertical velocity was occurring during El Niño (La Niña) periods in OLD-assim, we have rewritten Lines 237–240 as follows:
“The positive value of vertical velocity feedback indicates the enhancement (weakening) of upward vertical velocity at the depth of the thermocline during El Niño (La Niña) periods, which is inconsistent with observations.”

We have rewritten Sect. 3 to make it clear that enhancement of the upward vertical velocity is occurring during El Niño periods in OLD-assim.

Line 200, does OLD-assim, with the temperature amplitude increase, suggest a future forecasts of increasing global temperature using OLD would not give realistic results?
The OLD-assim results here need to be discussed especially carefully with the right terms. Be sure about whether it is giving a result that is the same direction but less strong or a result that is the opposite direction i.e. La Niña like conditions during expected El Niño.

In the Niño3 region in OLD-assim, an upward CO2F anomaly is found when the SST shows the positive anomaly, which is opposite to observations (Figure 1b in the revised manuscript). This is because, in MIROC-ESM, the amplitude of the seasonal–decadal scale variations in ocean temperature in the upper layer of the eastern equatorial Pacific is too much smaller than in observations (Figure 3), so that the correction term on the governing equation of the ocean temperature in OLD-assim forces to raise the equatorial water temperature in order to realize observed temperature variations, leading to an unnatural variations in the vertical velocity. We do not think that this result in OLD-assim means that the future projection by MIROC-ESM, where things are determined by the physics in the model, are unrealistic. In fact, the estimates of global warming by MIROC-ESM is not extremely different compared to other models. Friedlingstein et al. (2014, J. Climate, doi:10.1175/JCLI-D-12-00579.1) evaluated the twenty-first-century global surface warming defined as the 2081–2100 average relative to the 1986–2005 average under the concentration-driven RCP8.5 scenario in CMIP5 models (their Table 3). In MIROC-ESM, the global surface warming was estimated to be 4.7 °C, which is larger than the inter-model mean, 3.7 °C, but same with HadGEM2-ES.

Results 3.3

Section 3.2 and 3.3 would be better merged and restructured.

Thank you for your advice. We have combined Section 3.2 and 3.3 in the original manuscript and reorganized.

Please explain what makes the stratification in NEW setup better than OLD? Could it be applied to other CMIP models than are bad at reproducing ENSO?

In Lines 185–194 in the revised manuscript, we have described the two updates in model configuration in MIROC-ES2L. One is implementation of an updated plume model for cumulus convection with multiple cloud types where lateral entrainment rate varies vertically depending on the surrounding environment. The other is reduction of numerical diffusion by introducing highly-accurate tracer advection scheme in the ocean and by increasing vertical resolutions. We think these can be applied to other ESMs.

Does the different vertical depth levels/max-depth between NEW and OLD affect stratification and DIC storage in deeper water column?

In order to better represent the equatorial thermocline, the increased number of vertical layers in MIROC-ES2L has been adopted. Please see the reply above one. As a result, the stratification and DIC storage in the deeper layers may also change. However, we have to note that these may have also been changed by changing the advection scheme (Lines 190–193) and the model spinup time (Watanabe S. et al., 2011; Hajima et al., 2020).

Discussion

Key messages could be that one model in CMIP is not enough since they can be biased by misrepresented processes such as ENSO.

As you pointed out, each ESM has a model-specific bias, so that in future predictions multiple models need to be used and evaluated along with the uncertainties. To state this, we have rewritten Lines 267–269 as follows:

"There are many ESMs where the ENSO characteristics and/or the SST-CO2F relationship are inconsistent with observations. Causes of this discrepancy should be addresses in future studies through, for example, multi-model analysis, and also process-based uncertainty estimation will be further required in initialized climate and carbon predictions as well as projections by ESMs."

Line 255-260, not really needed here, we suggest to remove.

We removed the last paragraph in the original manuscript.

Figures

Figure 1, add R-value. Add the same graphs for NEW and OLD without assimilation.

We have added R-values in Figure 1 in the revised manuscript. The timeseries of SST and air–sea CO_2 flux in the Niño3 region simulated with NEW and OLD has been added as Figure S2.

Figure 2, x-axis label is not attractive, we suggest the authors use a colour-coded legend for the whole figure.

To make it easier to compare the magnitude of each term in Eq. (3) in each experiment, we have redesigned Figure 2.

Do you really need Figure 4, maybe a table would be better or stating the values in the text.

The results, which were presented in Figure 4 in the original manuscript, are now shown in Table 3 in the revised manuscript.

Combine Figures 5 and 7 for side by side comparison. Same for 8 and 6.

We have combined Figures 5 and 7, and Figures 6 and 8 as suggested.

Clarify the meaning of Figures 5-8. What does the colour scale mean? How should it be interpreted? Do we need to know timescale of response? Is it all calculated on monthly data?

The variation shown in Figures 2–5 in the revised manuscript represents the one during El Niño periods, and the opposite applies during La Niña periods. To make it clearer, we have added the following sentence in Line 165 in the revised manuscript:

"In the following, we describe the anomaly during El Niño periods, while the opposite applies during La Niña periods."

In the original manuscript, it was not clearly stated that we were discussing the anomalies from the climatic state. To clarify the processes occurring in our experiments, we have totally rewritten Sect. 3 in the revised manuscript. For example, we have rewritten the description on the process occurring in NEW-assim as follows:

"The maximum absolute value of the equatorial temperature analysis increment in NEW-assim is found at 10–40 m depths in the eastern equatorial Pacific, shallower than the depth of the thermocline (Figure 5a)." (Lines 218–220)

"The westerly wind anomalies in NEW-assim leads to weakening of upward vertical velocity along the equator during El Niño periods (Figure 5c)." (Lines 222–223)

"The weakening of upward vertical velocity causes lesser supply of the DIC-rich subsurface water to the surface layer, leading to the decrease in surface DIC concentration (Figure 5e)." (Lines 229–230)

The period of ENSO in NEW is longer than observations, and the data assimilation procedure can partially distort the model nature even in NEW-assim, so that we think that reproducing the observed timescale of ENSO is important along with the intensity of ENSO. We have added Table 2 showing the intensities and periods of ENSO in NEW, OLD, and the observation, respectively, and the following sentences in Lines 244–248 in the revised manuscript:

"In addition, the period of ENSO, which is defined as the peak of the power spectrum of one-year running mean NINO-SST, is 5.0 years in NEW, which is longer than 3.5 years of observations (see Table 2). Because the ENSO characteristics in NEW are not perfectly consistent with observations, model nature, namely responses of vertical velocity and DIC concentration in ENSO, are still distorted by the temperature analysis increment even in NEW-assim."

Figures 3–5 in the revised manuscript were drawn using monthly mean anomalies. To make it clear, the corresponding parts have been rewritten. For example, Lines 175–177 in the revised manuscript have been rewritten as follows:

“A cross section of the monthly ocean temperature anomaly regressed onto monthly mean NINO3-SST anomalies along the equatorial Pacific is presented in Figure 3, together with the climatological annual mean depths of the 18, 20, and 22 °C isotherms.”