To Reviewer #2

This article presents the CMIP6 version of the MIROC Earth system model. I appreciate this work, because a detailed description and evaluation of the CMIP6 models helps to interpret their results and raises the scientific value of this major community effort. The article is written clearly and is quite extended (73 pages), but I guess that just a complete description of the complex model would require several hundred pages. Therefore, the authors concentrate on those aspects in the biogeochemical part of the model that are new in comparison to the CMIP5 version of the model. I completely agree with this approach

First, we greatly appreciate that you agreed to review our paper and we thank you for your positive comments on this work. Following consideration of your comments, together with those of the other referee, which we found very helpful, we have revised the manuscript accordingly. Please find below (in bold) our detailed responses to your specific comments.

it should be followed more consequently. I suggest two steps in this direction. First, the results of the physical model should be evaluated at one place (i.e. merge subsections 3.1.1 and 3.1.4). This can go along with a comment that these results are presented first as a basis for the assessment of the biogeochem-ical results and the climate-carbon cycle feedbacks. Second, I see some potential to shorten the subsection 3.1.4, e.g. fig. 7 (SST) can be skipped as fig. 5 (2m tem-perature) is very similar. Also the title may be adapted towards the biogeochemical focus.

Thank you for this observation on the structure of the paper. Following your suggestion, subsections 3.1.1 and 3.1.4 have been merged into one, titled "Global climate: atmosphere and ocean physical fields".

As you pointed out, we agree that the two maps (2 m temperature and SST) look similar in some part. However, the detailed discussion on ocean biogeochemical fields relies on the SST map, as discussed between Reviewer #1 and the authors. We believe its inclusion would be informative for the readers and thus we have decided to retain the SST figure in the revised manuscript.

Scientific questions

- section 3.1.1: For a more complete view on the simulated climate, please specify the strength of the AMOC and the amplitude of ENSO as these two features of the physical system also affect the simulated carbon cycle quite profoundly. Just mention the numbers.

Following your suggestion, we briefly mentioned the amplitude of both AMOC and

ENSO in the revised manuscript without any additional figures as follows:

"In addition to the radiation/temperature responses against historical external forcing, we briefly describe here the El Niño–Southern Oscillation (ENSO) and Atlantic meridional overturning circulation (AMOC) strength in MIROC-ES2L, both of which can affect interannual–multidecadal carbon cycle processes (Zickfeld et al., 2008; Pérez et al., 2013; Friedlingstein 2015). In HIST experiment, the standard deviation of monthly SST anomaly in the Niño-3 region (5°S-5°N; 90°–150°W) was 1.57 K in 1950–2009, which is larger than that of HadSST (0.94 K). This unrealistically large ENSO amplitude tends to influence the simulated interannual global temperature variability (Fig. 2b), which is suggestive of further effect on the interannual variability in biogeochemical fields (e.g., CO_2 flux in the tropics). The AMOC intensity, quantified by the North Atlantic Deep Water transport across 26.5°N, was approximately 13 Sv (1 Sv = 106 m^3 s^{-1}) as the 1850–2014 average, which is smaller than the observational estimates of 17.2 Sv (McCarthy et al., 2015). In the HIST run, the AMOC strength was weakened at a rate of 0.01 Sv yr^{-1} (i.e., reduction of 1.7 Sv during the 165 years of HIST), which seems slightly smaller than the recent estimates of AMOC weakening of 3 ± 1 Sv from the mid-twentieth century (Caesar et al., 2018)."

- line 527-530: Uncertainty in land carbon uptake (estimated from data) is smaller if calculated from the global carbon budgets (following CL = CE – CA – CO, s. line 435) than if it is calculated from the uncertainties in LUC emissions and the natural land sink. The uncertainty in CO is 20 PgC (s. line 550). The uncertainties in CE (derived directly from inventory data) and CA (derived from precise and representative measurements) are even smaller. Thus, the uncertainty in CL is much smaller than 90 PgC.

Thank you for the important suggestion regarding the range of uncertainty for CL. As you pointed out, if we calculate the possible CL range from the combination of CE (fossil fuel), CA, and CO, the CL range is changed to 15 ± 29 PgC, where the uncertainty range (±29 PgC) is much smaller than that of the bottom-up approach (i.e., the sum of the natural land sink + land use change, i.e., ±90 PgC). Thus, in the revised manuscript, we have mentioned both estimation ranges as follows:

**"*The possible range for CL can be changed if we estimate it as the residual of other global carbon budgets (i.e., CL = FF − CA − CO, where FF is the cumulative fossil fuel carbon emission). Using the estimated ranges of FF, CA, and CO reported by Le Quéré et al. (2018) (i.e., 400 ± 20, 235 ± 5, and 150 ± 20 PgC, respectively; the budget imbalance of 25 PgC is ignored here), the CL range is suggested to be 15 ± 29 PgC. In this case, the result of MIROC-ES2L (44 PgC) is still within the estimation boundaries*

***although it is at the upper end of the suggested range.*"**

- line 531-542: I'm not an expert in ocean biogeochemistry, but as far as I understand, a buildup of the ocean sediment reduces alkalinity in the ocean water, so that the ocean on the long term will outgas CO2 to the atmosphere and ocean water + ocean sediment looses carbon after the sedimentation process has been switched on (and the loss in alkalinity is not compensated by riverine input). By contrast, in the manuscript it is mentioned, that the ocean carbon uptake in the control run is partly explained by the sediment extracting carbon from the ocean bottom.

Thank you for your inquiry regarding the effect of the sedimentation process on the carbon cycle. As you indicated, $CaCO_3$ burial reduces alkalinity, leading to ocean carbon release. However, organic matter burial decreases DIC, resulting in the ocean carbon uptake. In our model, the ocean absorbed CO_2 during the spin-up period because the latter process is dominant.

- line 766-769: I don't understand, why the different treatment of the vertical SOC profile in the model and WISE30sec explains the large difference in the amount of SOC in the boreal range. I think, that has to do with permafrost. It should be mentioned here, if the model includes freezing in the soil and how this affects SOC.

We apologize for this confusion. Yes, as you correctly pointed out, the direct reason for the difference between WISE30sec and the model is the frozen carbon within the permafrost region; it is considered in WISE30sec but not in the model. We have clarified this point in the revised text as follows:

"This is likely attributable to different treatment of frozen carbon in deeper soils in permafrost regions, i.e., WISE30sec covers the total SOC down to 2 m depth including frozen carbon, while the model does not consider the frozen carbon and instead simulates only upper SOC as litter form and lower SOC as humus."

- section 3.2.2: The simulations NO-NR, NO-NRD, and NO-FD are only 100 years long. Do you've analyzed, if the signals that are based on these simulations and discussed in section 3.2.2 are already stable after 100 years? Or are they still very transient?

Thank you for your suggestion. In our model, NPP changes in response to the changes in nutrient input within several decades, consistent with Somes et al. (2016). We have added this information to the revised manuscript as follows.

"In the simulations, because changes in NPP and surface nutrient concentrations continued to change over several decades after the abrupt switching-off

manipulation, the average over the final 10 years is used for the following analysis. The rapid response of NPP to changes in nutrient input is consistent with that found in previous research (Somes et al., 2016)."

Minor corrections

- line 18: "article describes" instead of "study developed"

We have made this change as you suggest.

- line 176: Figure 1 is not helpful. Please specify for each model component whether it represents atmosphere, ocean, or land. You can also add for each model component the ele-ments that are handled prognostically (C,N for VISIT-e, C,N,P,Fe,Ca,O for OECO-v2?, Fe,S for SPRINTARS?) and indicating by labeled arrows which elements are passed from each component to others (e.g. N,P from VISIT-e to OECO-v2). I think, this would result in a nice overview schematic, how the components are coupled concern-ing biogeochemistry.

Thank you for this very good suggestion. The schematic has been modified accordingly and we agree that the revised figure is much more powerful and more informative for the readers.

- line 201: Please indicate the horizontal resolution of the ocean model (e.g. average size of a grid box in km or the number of grid boxes of the global field).

This has been documented in the revised manuscript as follows:

"The horizontal coordination for the ocean is changed from the bipolar system employed in MIROC5 to a tripolar system in MIROC5.2 that is divided horizontally into 360 × 256 grids. (To the south of 63°N, the longitudinal grid spacing is 1° and the meridional spacing becomes fine near the Equator. In the central Arctic Ocean, the grid spacing is finer than 1° because of the tripolar system.)"

- line 210: What is a "snow-derived wetland"?

In the revised manuscript, this has been clarified as follows:

"…wetland formed temporarily in the snowmelt season is newly considered to reduce…"

- line 257: "of vegetation (each represented on a separate tile) in each land grid box" instead of "of tile in each land grid"

Thank you for suggesting this correction. We have changed it as follows in the revised manuscript:

"The model assumes five types of land cover (each represented on a separate tile) in each land grid box (i.e., primary vegetation, secondary vegetation, urban, cropland, and pasture)…"

- line 277: "transported by rivers" instead of "transported rivers"

Corrected.

- line 305: The phosphorous cycle has also no analog to denitrification.

Following your comment, we have rephrased it as follows:

"The structure of the phosphorus cycle is generally similar to that of nitrogen except in two respects: 1) the riverine input of phosphate is the only process that introduces phosphorus into the ocean, and 2) there is no process of outgassing from the ocean, unlike the denitrification process in the nitrogen cycle."

- line 346: It would be nice to mention how the DMS affects the climate (I guess as sulfate aerosol in MATSIRO that affects radiation).

In response to your comment, we have added the following sentences at the end of this paragraph:

"This modification of the DMS emission scheme increases the sulfate aerosol amount, particularly over high-latitude oceans during winter and in regions where strong surface wind speed occurs. Solar irradiance of the surface decreases in such regions; however, this effect is not sufficiently significant to change the mean physical climate states."

- line 364, 372, 373: I would not use the word "detect" in that way. Please substitute it by e.g. "except that the prescribed CO2 increase affects only the carbon cycle processes".

Corrected.

- line 403: last 4 lines of table 1, NO-NRD Configurations "N depositions" instead of "Fe depositions", NO-FD Configurations "Fe depositions" in-stead of "de depositions".

Corrected.

- line 438: "coupled" instead of "entire"

The corresponding sentence has been reworded following the comment from Reviewer #1.

- line 448,449: The denominator should be T. The common unit of Gamma is PgC/K (s. also table 6).

Thank you for identifying this error. The equation was originally presented incorrectly and in the revised manuscript, "CA^{1PPY}" has been replaced with "$T^{1PPY-RAD}$".

-line 471: "deviations of the model results from HadCRUT4" instead of "discrepancies between the model result and HadCRUT4"

Corrected.

- line 555: "in the HIST run" instead of "at the end of the HIST run"

We are sorry but we cannot find the corresponding text in the first manuscript. We suppose you meant <"at the end of the HIST run" instead of "in the HIST run">. Based on this idea, we have rephrased L555 as follows:

"…this speculation is also supported by the diagnosed CO_2 concentration at the end of the HIST run…"

- line 606: "4.2 TgN yr-1" instead of "4.5 TgN yr-1" - compare with line 11 of table 3

This has been replaced with the correct number (4.3 TgN yr^{-1}) in the revised manuscript.

- line 644: "decay of biomass in the LUC-product pools" instead of "decay of LUC-product pools"

Corrected.

- line 701: "concentration minimum" instead of "con-centration peak"

Corrected.

- line 721: Please mention that the model (obviously) simulates no deep water formation in the Labrador Sea.

Thank you for your suggestion. The problem in relation to the Labrador Sea has been mentioned in the revised manuscript.

- line 738: "high" instead of "higher"

Corrected.

- line 755: "GPP in these regions is captured reasonably well by the model (Fig. 10a and

10b). Thus, the overestimation" instead of "Considering the GPP in these regions is captured reasonably well by the model (Fig. 10a and 10b), the overestimation"

Corrected.

- line 760: "products" instead of "product"

Corrected.

- line 784: unit of SOC is missing

The unit for SOC has been added (gC m^{-2}).

- line 875: I see also some regions north of the equator, where GPP is reduced by climate change (e.g. South Asia). Please remove "of the Southern Hemisphere".

Corrected.

- line 879-890: Please comment on the strong reduction of GPP by LUC in the tropics.

Following your comment, we have mentioned the GPP reduction in the tropics as follows:

"In the tropics, LUC reduces the non-crop GPP but weakly increases crop GPP, which results in net negative reduction of GPP as grid averages (Fig. 13j). Meanwhile, regions with intensive agriculture with nitrogen fertilizer input (e.g., Western Europe, East Asia, and parts of North America) show net positive change of GPP as grid averages, where increases in the crop contribution overcome reductions in the non-crop contribution (Fig. 13k and 12l)."

- line 917: I think, it would be good to mention that the NPP increase in the open ocean by N input from rivers mainly occurs in the Atlantic.

We agree with your comment. Significant NPP increase due to riverine N input in the Atlantic has been mentioned in the revised manuscript.

- line 973: "CO2-induced ocean acidification and warming-induced deoxygenation" instead of "warming-induced ocean acidification and deoxygenation"

The suggested change has been made.

- line 1002,1003: This sentence is better placed at the end of the para-graph.

The suggested change has been made.

- line 1022,1023: This sentence is just repetition. You can remove it to shorten the article.

The sentence has been removed from the revised manuscript.

- line 1026: “climate, carbon cycle, and coupled climate-carbon cycle sys-tem” instead of “climate, carbon cycle, and climate-carbon cycle system”

The suggested change has been made.

- line 1130: “confirmed to be captured well” instead of “confirmed captured well”

The suggested change has been made.