Response to Reviewer #1

Dear reviewer,
Please find below our answers to the constructive remarks you raised regarding our manuscript. They all have been carefully considered and will provide what we feel is a much improved manuscript. You will also find all of the modified figures of the new manuscript and Supplementary Material.

Comment #1 (C1): *The overall structure of the manuscript and occasional lack of clarity in some sections is a major shortcoming of the manuscript. For example, results from the model outputs are not fully integrated with proxy data and are rather independently summarized. Although, this manuscript presents an important dataset, which is of interest for the scientific community, some of the interpretations need to be significantly refined and I find few of them not convincing at all (see my comments on the discussion section below). The fact that only figures are provided as the supplementary information is also unhelpful and I believe a short text summary is warranted. To summarize, the manuscript in its present form does not meet the following CP peer review guidelines/criteria:*
1. Are substantial conclusions reached? Needs to be improved (see detailed comments below). 2. Are the results sufficient to support the interpretations and conclusions? In general yes but some of the interpretations need to be improved. 3. Is the overall presentation well structured and clear? Needs to be improved. 4. Is the language fluent and precise? In general yes but there is occasional lack of clarity in some sections. 5. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Some discussion sections can be combined to improve clarity and train of thought. 6. Is the amount and quality of supplementary material appropriate? The supplementary information lack sufficient information and needs to be significantly improved.

Reply #1 (R1): We agree with Reviewer #1 that despite the important dataset we provide in the manuscript, some changes in the structure of the manuscript might help describing our results more clearly, and improving our interpretations and conclusions. Now we clearly separate the result and discussion sections (new sections 4 and 5, respectively), and fully discuss the model outputs together with the empirical data in subsections 5.1 to 5.3 (see our Reply 6). This change relies on the relocation of figures from the Supplementary Material to the main core of the manuscript, and on the addition of new figures related to simulated Primary productivity (PP) in the results. The number of figures in the supplementary Material is thus reduced, and we significantly improved the explaining text for all of the remaining figures.

C2: *Line 30: The way the monsoon is currently defined need to be improved.*
Here, the monsoon is practically presented as a giant sea breeze that is responsive to changes in the land-sea thermal contrast alone and excludes the more complex aspects of the monsoon and its relation with the tropical ocean on seasonal to interannual to decadal timescales (e.g. ENSO and IOD).

R2: In addition to the simple 'sea-breeze' description of the monsoon, there is, indeed, a description that focuses on its energetic aspects and provides a broader overview of the mechanisms behind monsoon variability (Schneider et al., 2014). In the revised manuscript, we now mention both aspects, and added a few sentences to mention the interannual and decadal changes of monsoon related to ENSO and IOD variability, an important aspect of Indian Monsoon natural variability. It also echoes the seasonal and interannual PP changes we describe in the introduction. However, since the present manuscript chiefly deals with orbital to millennial climate changes, we chose to not fully detail this aspect in the introduction.

***C3:** Line 40–53: The subsequent section provides a detailed summary of the oceanographic setting in the Bay of Bengal and Andaman Sea. To be more articulate and improve clarity, it's probably best that comparisons within the Arabian Sea and differences with in the broader Northern Indian Ocean oceanography are presented in the introduction section.*

R3: We totally agree with Reviewer #1. Indeed, in this section, we highlight the specific patterns of PP in the Bay of Bengal and the Andaman Sea compared to the Arabian Sea. PP in the Arabian Sea is particularly high compared to the Bay of Bengal and the Andaman Sea during Summer Monsoon, due to the occurrence of important coastal upwelling that bring nutrients into the photic zone. To the contrary, summer monsoon is associated with important freshwater inputs in the Bay of Bengal that cause salinity-driven, water column stratification, resulting in a reduced nutrient input to the upper water column, and thus subdued PP. Such broad PP difference is an important aspect that we also highlight when discussing about past evolution (new section 5) and compare our results with previous works (Schulz et al., 1998; Ivanochko et al., 2005). It seems therefore very important to mention such modern pattern in the introduction.

Lines 40-53 might not be clear enough, particularly when dealing with acronyms such as the Andaman Sea or Arabian Sea. Since we don't refer to the Andaman Sea very often in the manuscript, we only use diminutives for the Bay of Bengal and the Arabian Sea. We also describe more clearly the relationship that actually exists between the upwelling system and PP in the Arabian Sea, adding a few sentences and references (Bartolacci and Luther, 1999; Wiggert et al., 2005; Liao et al., 2016) on this aspect. We are aware that in the western Arabian Sea, the summer upwelling system is quite complex, with for example, a branch that can transport nutrient to the central part of the Arabian Sea. However, we prefer to not mention PP distribution in a very detailed way, because we are not able to discuss its evolution and distribution with such details in the past due to a lack of high-resolution PP.

***C4:** 2) Site description and oceanographic setting*
This section provides a detailed summary of the oceanographic setting of the studied site and is well written.
Are there any notable differences in seasonal PP variability between the Bay of Bengal and the Andaman Sea? Perhaps a sentence or two addressing the above question will be helpful.

R4: Geographically and oceanographically speaking, our site is located at the junction between the northeastern Bay of Bengal and the northern Andaman Sea. These two parts represent open oceanic settings and are both influenced today by low SSS seawaters originating from the Irrawaddy river (Figs. 1g, f). They are both characterized by annual rates of PP around 100-140 gC m^{-2} yr^{-1} (Fig. 1h, i). Very high annual PP (up to 340 gC m^{-2} yr^{-1}) can be observed in coastal settings that are under the direct influence of river-driven nutrients, but these nutrients are actually consumed in these proximal environments and do not reach the studied site. Such configuration may have changed in the past particularly during the LGM when sea-level was relatively low (see Reply 8). However, there is no reason why the northeastern Bay of Bengal and the northern Andaman sea should behave in a completely different way under such conditions (Fig. 1), and the most likely forcing factor y forcing factor that might drive orbital and millennial PP changes is monsoon, modulated by sea-level, insolation and/or AMOC dynamics. Our core location is therefore suitable to test the relationships between these parameters. As suggested by Reviewer #1, we added a sentence to highlight such similarities between the northeastern Bay of Bengal and the northern Andaman Sea, in the new version of the manuscript.

C5: *3) Materials and Methods*
This section provides a detailed summary of the methodology and is generally well written. However, information provided on age model reconstruction is insufficient and citation of Figure 2 is not very useful either. I suggest that the authors provide a summary of the age model including changes in the rate of sedimentation etc. This can be included in the same section or in the form of a supplementary material.

R5: The age model used herein has originally been described in Marzin et al., (2013), and has latterly been used by Yu et al., (2018) and Ma et al., (2019). Indeed, Marzin et al. (2013) devoted an entire chapter to this chronological aspect (in their chapter 2.1), and already described all of the important information required herein, such as the sedimentation rate (represented in their Fig. 3). Therefore, we decided to refer to Marzin et al. (2013) but we added a figure including the sedimentation rates of the core within the Supplementary Material (new Figure S1). We discuss this part with extreme caution to avoid any confusions regarding the age model, and clearly demonstrate its robustness. The Figure 2 has been modified compared to the initial submission. It is now Figure 3 that includes relative abundance of coccoliths and reconstructed PP.

C6: *4) Results and discussions*
This section of the manuscript is poorly structured and in my opinion, the weakest part of the manuscript. For example, a large chunk of the text (e.g. section 4.3, section 4.3.1: lines 300 – 317) should have been included in the methodology section. This has made the discussion section overall very descriptive and lacking in substance, and most crucially hard to follow. One way of overcoming this predicament is to divide this section in to two separate sections (i.e., Results and Discussions). For example, the proxy data and model data results can be grouped into two subsections and the discussion section should focus on the dynamics of PP variability over the studied time interval. The Discussion section should also integrate both proxy data and model inferences to build a more coherent understanding of PP variability over the last 26 kyrs.
Looking at the PP record, it is clear that there are three distinct time intervals that can be discussed separately including the highly variable LGM (?), the last deglaciation period marked by an abrupt shift in PP centered around the BA and the Holocene period, which displays a more gradual change. Therefore, dividing the discussion section accordingly and zooming on these three distinct periods will significantly improve clarity

R6: We believe that the proposition made by Reviewer #1 regarding the structure of our manuscript will certainly clarify it, therefore helping to improve the description of our results as well as the interpretations. Therefore, we changed our manuscript in the light of the suggestions. An entire chapter is now devoted to the results. In the discussion, empirical data and model outputs are interpreted simultaneously, which is helpful to build a more coherent scheme behind PP variability. Our results are now discussed regarding the three time-intervals highlighted by Reviewer #1.

Below is the new structure of chapters 4 and 5:
4. Results
4.1. Coccolith abundances and reconstructed primary productivity over the last 26 kyrs
4.2. Simulated primary productivity and physicochemical profiles in the northeastern Bay of Bengal
5. Forcing factors behind PP variations over the last 26 kyrs: the inputs of model-data comparisons
5.1. During the glacial period
5.2. During the last deglaciation
5.3. During the Holocene

In detail:

- In section 4.1, we present and describe coccolith species abundances and reconstructed PP (Figure 1 of Author Response (Fig. AC1)).
- Section 4.2. relies on new IPSL-CM5A-LR figures dedicated to model results, that help understanding and improving model output interpretations i.e. i) simulated PP maps (Fig. AC2), and ii) simulated vertical profiles of potential temperature, salinity, potential density, and nitrate content of the northeastern Bay of Bengal under four experimental runs (Fig. AC3), that help discussing climate conditions for the LGM (LGMc), the Heinrich Stadial 1 (LGMf), and the Mid-Holocene (MH), compared to preindustrial (CTRL). We show the results of annual mean, summer seasons mean (from June to August, JJA) and winter seasons mean (from December to February) for all these specific time intervals, in order to evaluate PP changes during the monsoonal seasons.
- In sections 5.1 to 5.3., we compare our reconstructed PP signal with the published empirical records previously documented in Fig. AC4, and with TraCE-21 transient simulations of the upper water column stratification, SSS, SST and net precipitation (P-E), previously documented in Fig. 5. Merging our previous Figures 4 and 5 into a new figure (Fig. AC4), allows to better discuss PP variations in the monsoonal context. We also combine atmospheric and oceanic outputs of the four experiments run together with the simulated PP obtained by the IPSL-CM5A-LR model in order to better discuss and interpret our reconstructed PP during the last glacial period (section 5.1; Fig, AC5, AC6), the last deglaciation (section 5.2; Fig. AC7, AC8) and the Holocene (section 5.3; Fig, AC9), as proposed by Reviewer #1.

At last, we moved lines 300 – 317 and all the parts referring to the description of the chosen simulated variables to the section 3 (Material and Methods).

***C7:** Lines 205 – 208: the authors write 'at millennial-scale, large magnitude PP oscillations, are observed during the deglaciation (19–11 kyr BP), showing similar features than those found in the*

Greenland ice core δ18O record, representing the rapid climatic changes in north hemispheric high-latitude areas (Fig. 2; Stuiver and Grootes, 2000).'

But it is stated in section 3.1 that the age model, although primarily based on 31 AMS 14C dates, it was still tuned to GISP2 Greenland ice core δ18O curve. Can this be considered circular reasoning?

R7: We thank Reviewer # 1 for highlighting this peculiar aspect. Indeed, it might be seen as a circular reasoning. However, our micropalaeontological data are well in phase with numerous geochemical data obtained elsewhere in the Tropical Indian Ocean and the Chinese continent, based on sediment cores and speleothems with totally independent age models, respectively. They also match very well TraCE-21 and IPSL-CM5A-LR outputs. Besides, as mentioned above (Reply 5), the age model of core MD77-176 has already been used by Marzin et al., (2013), Yu et al. (2018), and Ma et al. (2019), i.e. papers discussing geochemical data at regional and global scales. All these highlights point to a robust age model and demonstrate that our micropalaeontological data can be properly be discussed in the light of the rapid climatic changes recorded in the northern high latitudes. To avoid any confusion, we rephrased this part of the manuscript focusing on the relationship that exists between PP and SSS of MD77-176.

C8: *Lines 255 – 229: the authors write, 'Several pieces of evidence suggest that millennial- scale variations of PP between 26 and 19 kyr (i.e. before the LGM) chiefly resulted from wind-driven mixing. First, high PP values are reached during intervals of low surface water salinity. If these PP variations (and upper water column stratification) were primarily driven by precipitation–evaporation changes, the opposite relationship would be expected, and PP would peak at periods of higher salinity because of the weaker barrier layer effect'.*

a) Can you independently verify if the wind-driven mixing in the Northern Indian Ocean was enhanced during the LGM?
b) Which are the intervals of low salinity during the LGM?
c) Isn't the LGM Andaman Sea significantly more saline compared to other periods such as the Holocene?
d) How does precipitation minus evaporation impact PP variability in general?
e) What inferences can be made on LGM PP variability from the LGM experiments?

R8: We appreciate these remarks that rise further questions and clearly help us improving our interpretations. We first answer your questions one by one and then develop a more detailed response that echoes question a–e.

a) We checked the modeling outputs of surface winds during the both monsoonal seasons. It shows stronger summer wind and weaker winter wind intensities over the Bay of Bengal and Andaman Sea during the LGM (Fig. AC6).

b) The short intervals of low salinity are shown by the SSS record of MD77-176. They are recorded at ~21 kyr BP and ~23 kyr BP (Fig. AC4). However, it is not possible to test such specific short-term intervals with model outputs that give mean states of chosen parameters during the LGM.

c) The modeling outputs show that generally, Bay of Bengal and Andaman Sea behave the same way. That is only in the northeastern Bay of Bengal, close to the coasts, that a significant difference may be seen. Indeed, according to these model outputs, they are both getting saltier during the LGM, while

the northeastern BoB is unchanged or a little fresher (Fig. AC5). The Andaman Sea doesn't appear specifically more saline than the BoB during that time interval.
d) According to IPSL-CM5A-LR outputs, it appears that if the net precipitation is lower during the LGM, the Bay of Bengal and Andaman Sea might get saltier and PP might increase due to weaker salinity stratification.
e) The LGMc experiment gives a mean state of PP during LGM. Generally, it shows higher PP in the BoB and the Andaman Sea. Under weaken AMOC condition, LGMf experiment shows higher PP compared to LGMc matching our reconstructed PP results from the LGM to the Heinrich 1.

General reply:

During glacial times (26–19 kyrs), high (low) PP intervals do match low (high) SSS ones, as shown by low (high) values in seawater oxygen anomalies recorded at the same site (Marzin et al., 2013; Fig. AC4).

There is no doubt that the South Asia and the North Indian Ocean are drier during the LGM due to relatively lower precipitation over the South Asia, as demonstrated by previous empirical data (Dutt et al., 2015; Contreras-Rosales et al., 2014; Kudrass et al., 2001) as well as numerical outputs here (Figs. AC4, AC5). However, the outputs of IPSL-CM5A-LR simulations, together with TraCE-21 ones show that, compared to preindustrial, weaker winter winds, stronger summer winds, and saltier sea surface conditions, generally prevailed in the Bay of Bengal and the Andaman Sea during the LGM (LGMc in Fig. AC5). These results suggest that the interpretation we have made for the last deglaciation and the Holocene, stating that a stronger summer monsoon and/or a weaker winter monsoon, induce increased precipitation, decreased SSS and thus, stronger salinity stratification and subdued PP is not always verified, and particularly during the LGM. In such a case, we cannot exclude that stronger and drier summer winds during that time interval (as suggested by model here), could eventually lead to enhanced sea-surface mixing, thus triggering upper water mixing, higher SSS, and higher PP as observed in the Arabian Sea today. However, as mentioned in the introduction of our manuscript, the Arabian Sea behave in a very different way than the Bay of Bengal, notably thanks to the development of massive upwelling on its western coasts, and the direct comparison of both basins may be questioned. Unfortunately, we cannot test such sea-surface mixing hypothesis with TraCE-21 or IPSL-CM5A-LR outputs, so far.

Spatial discrepancies of SSS are also found with model outputs. This is particularly the case when dealing with the northeastern Bay of Bengal and northern Andaman Sea areas. First, models in PMIP3 (Braconnot et al., 2012) show different results of SSS for the LGM: some models show fresher water, while others depict saltier conditions (Fig. AC10). Second, when dealing with the outputs of IPSL-CM5A-LR, such area (that include our core site) has very limited SSS increases during the LGM, if it doesn't show sometimes SSS decreasing trends (Fig. AC5). Such discrepancies have also been reported once by empirical data. Indeed, Sijinkumar et al. (2016) depict lower SSS in the northern Andaman Sea during the LGM. It may highlight the complex area that is the northeastern Bay of Bengal and northern Andaman Sea due to the Irrawaddy mouth influence. It might also partly explain the millennial-scale relationship documented at our core between SSS and PP at that time, i.e. under relatively low sea-level when site MD77-176 is located in a more proximal environment. Indeed, one cannot exclude that under such conditions, the PP increases (decreases) observed when SSS decreases (increases), reflect an increases (decreases) of nutrient together with freshwater inputs from the

Irrawaddy river, respectively. Such assumption is confirmed in Figures AC2 and AC3 where PP strongly increases (Fig. AC2), when vertical profiles clearly change from open ocean type to coastal one (Fig. AC3). Our scenario appears therefore to be a suitable explanation for the PP pattern obtained herein during the LGM.

However, in all cases, it seems difficult at that point, to deeply compare thoroughly (and discuss) the millennial PP changes obtained at our core site, to mean state simulations of local PP and SSS, obtained for the northern Bay of Bengal and Andaman Sea during the LGM. Additional high-resolution PP records and further numerical simulations are required in the area, in order to discuss this issue properly. As an example, a PP record further south in the Andaman Sea, i.e. far away from river mouth influences, (Zhou et al., unpublished) clearly shows higher PP from 30 to 19 ka, under saltier conditions, and does not show strong short-term fluctuations as recorded at site MD77-176.

The influence of drier and stronger summer winds together with the influence of nutrient and freshwater inputs from the Irrawaddy river behind PP variability during the LGM, are therefore evoked in the manuscript, but with extreme caution.

C9: *In section 4.1 (line 217): The authors write that, 'PP peaks are related to low SSS intervals before the LGM, and high SSS intervals over the last 19 kyr'.*
Although, PP did not significantly change over the course of the Holocene, there appears to be a clear discrepancy between the gradual monsoon intensification over the Holocene and PP variability. PP variability over the course of the last deglaciation and the Holocene are clearly different. Proxy data shown in Figure 2 suggest that estimated PP has lower valued during the Mid-Holocene (~90 gC m-2 yr-1) compared to late – Holocene (~130 gC m-2 yr-1). This, however, is not discussed in any detail and the way the discussion section is structured is at fault again.

R9: We agree with Reviewer #1. While the mechanisms controlling PP variations during the last deglaciation and the Holocene are similar and related to salinity stratification, PP variability is different over these two time intervals. They are characterized by rapid and large amplitude PP changes during the deglaciation, and rather gradual PP trends during the Holocene. Both periods are under the influence of insolation and AMOC forcing that impact land-sea thermal distribution over low latitudes, thus moderating monsoon strength, and controlling oceanic stratification and PP. However, to the different of the Holocene, rapid changes occur in the AMOC strength during the deglaciation, and they are clearly reflected in the Indian monsoon and PP dynamics at that time.

Therefore, such different PP patterns between the last deglaciation and the Holocene is clearly related to AMOC vs insolation imprints other the last 19 kyrs. Rapid changes in PP patterns during the last deglaciation clearly reflect the rapid changes in the AMOC strength. To the opposite, long-term changes in PP during the Holocene most probably reflect long-term changes in insolation and associated feedbacks with the ocean-atmosphere system. We now discuss the deglacial and Holocene PP variabilities separately, in our revised sections 5.2 and 5.3, respectively.

C10: *In section 4.1 (lines 204 – 205): it is briefly mentioned that PP variability shows 'an opposite trend compared to insolation (Fig. 4a, h)' and in section 4.3.1 it is stated that 'insolation is the main climate forcing factor during the Holocene'.*
Why do we have the monsoon peaking later during the mid-Holocene lagging maximum Northern

Hemisphere summer insolation by few kyrs then? The lagged response of the monsoon to insolation forcing suggests that orbital scale monsoon variability is more complex (see Clemens et al., 2003; Caley et al., 2011; Gebregiorgis et al., 2018). Having this in mind, I would therefore encourage the authors to have a more critical outlook on PP variability over the course of the Holocene. I also recommend including Figure S3 – S6 in the main text body and can be used to gain some unique insights on LGM, deglacial and Holocene PP variability. Perhaps Fig. 3 can be moved to the supplementary section.

R10: During the Holocene, our PP record shows a minimum at ~6–8ka, lagging of about 3.5–5.5 kyr, the maximum North Hemisphere august insolation curve. However, it is clearly in phase with geochemical records obtained in the area that document high PP in the Arabian sea (Schulz et al., 1998; Ivanochko et al., 2005) and high precipitation over South Asia during that time interval (Dutt et al., 2015; Contreras-Rosales et al., 2014; Fig. AC4). Such results show that during the Holocene, PP from the northeastern Bay of Bengal is highly related to monsoonal dynamic, and more particularly, precipitation. Summer winds triggers strong coastal upwelling and high PP in the Arabian Sea. They also transport moisture to the South Asia where the summer precipitation is strong. Such increase in precipitation causes strong salinity stratification over the northeastern Bay of Bengal and thus low PP.

The references cited in reviewer's comment argue for the hypothesis that tropical monsoon variability is dominated by, and responds directly to the North Hemisphere summer solar radiation, and point out the importance of internal climate forcing and oceanic feedbacks, such as latent heat export from the southern Indian Ocean. Clemens et al., (2003) particularly point out that the minima of SST in the southern subtropical Indian Ocean are synchronous with the maxima of summer monsoon, and the moderating effect of ocean thermodynamic features on monsoon circulation is important. In all cases this aspect is an (usually) inexplicable issue. We have mentioned this lag in the revised section 5.3 of the manuscript, and interpret the Holocene period with caution. The modifications of supplementary figures are explained in Reply 25.

***C11:** Line 57: What 'fast changes'? Please rephrase.*

R11: We have rephrased to 'abrupt changes'

***C12:** Line 61: PP record or paleo-PP record. Stick with one for consistency.*

R12: We Stick with 'PP record'.

***C13:** Line 61: Da Silva et al., 2017 is a relevant reference here.*

R13: We have cited this.

***C14:** Line 62: 'tropical ocean ecology' is very broad and I am not sure this is accurate as well. Perhaps Northern Indian Ocean ecology is more appropriate.*

R14: We agree with this suggestion and made the changes in the light of the comment.

C15: *Line 73: 'High-time-resolution' or 'High-temporal-resolution'? 'High-resolution' is a perhaps a better phrase.*

R15: We have rephrased to 'high-resolution'

C16: *Line 74: Why is it important that the 'studied period covers a complete precession cycle'? This sentences need to be qualified or delete otherwise.*

R16: We've removed this sentence.

C17: *Line 80: 'interpret' is perhaps a better word here than 'analyze'.*

R17: We agree with this suggestion and made the changes when necessary.

C18: *Line 199–200: 'At orbital scale' – remove.*

R18: It has been done.

C19: *Line 202: use 'maximum or minimum Northern Hemisphere (NH) summer insolation' instead of low or high insolation with no reference to the latitude or the season.*

R19: It has been done.

C20: *Line 205: 'On millennial timescale...'*

R20: It has been done.

C21: *Line 211: 'Synchronous vs. asynchronous' rather than 'Negatively vs. positively correlated' and of course 'correlation' being a statistical term.*

R21: We agree with this suggestion and made the changes in the light of the comment.

C22: *Line 290–291: Rephrase or remove*
'During the Holocene, insolation is the main climate forcing factor since other forcing (i.e. greenhouse gas, ice volume, coastlines, vegetation) are relatively stable after the deglaciation.

R22: We have removed this sentence.

C23: *Line 291–292: Rephrase. Perhaps, a sentence along these lines will do: 'the response of the Indian monsoon to changes in orbital insolation has previously been examined using both AGCMs and ocean–atmosphere general circulation models. . .(Refs).)'*

'The mechanisms that force monsoon climate to change were studied by many modeling works (Refs).'

R23: We have removed this part, because of the new manuscript structure.

C24: *Figures S1, S5, S6 are not cited in the main text and please add supplementary text to the supplementary information. Also make sure that the figures are in chronological order.*

R24: We decided to keep all of the maps of TraCE-21 outputs in the Supplementary Material, and show all of the maps from our IPSL-CM5A-LR results in the main text. They have been slightly modified to match the new structure/discussion of the manuscript. All of the figures presented in the Supplementary Material are summarized, within detailed captions.

R25 (modification of supplementary figures 3 to 6):
1) Fig. S3 have been modified and moved to the main text (Figs. AC5, AC6, AC8, AC9). We show four groups of maps, which are the CTRL results as well as the differences between LGMc and CTRL, LGMf and LGMc, MH and CTRL. The variables are annual net precipitation (precipitation minus evaporation), annual SSS, annual potential gradient between 200 and 5 m, JJA surface wind speed and DJF surface wind speed.
2) For Figs. S4 to S6, we have removed the results of ORB simulation as they are similar to the FULL simulation, and removed the results of MWF_BA minus MWF_HS1 as well, since they are similar to the results of TraCE_LGM minus MWF_HS1. Therefore, for the maps of TraCE-21 simulations, we show five groups of maps in the Supplementary Material which are the LH, and differences between MH and LH, LGM and LH, BA and HS1, MWF_HS1 between LGM. We show the same variables as IPSL-CM5A-LR. The modified supplementary figures can be seen from Fig. AC11 to AC16.

References

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Figures

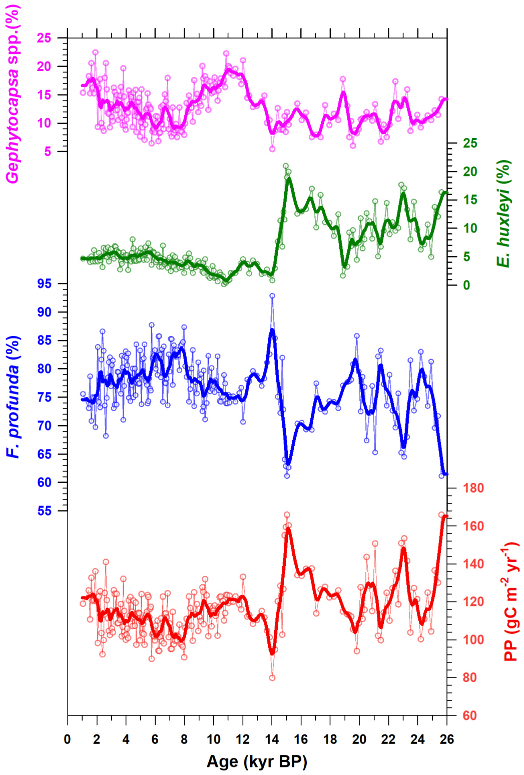

Fig. AC1. Relative abundance changes of main coccolith species and reconstructed PP.

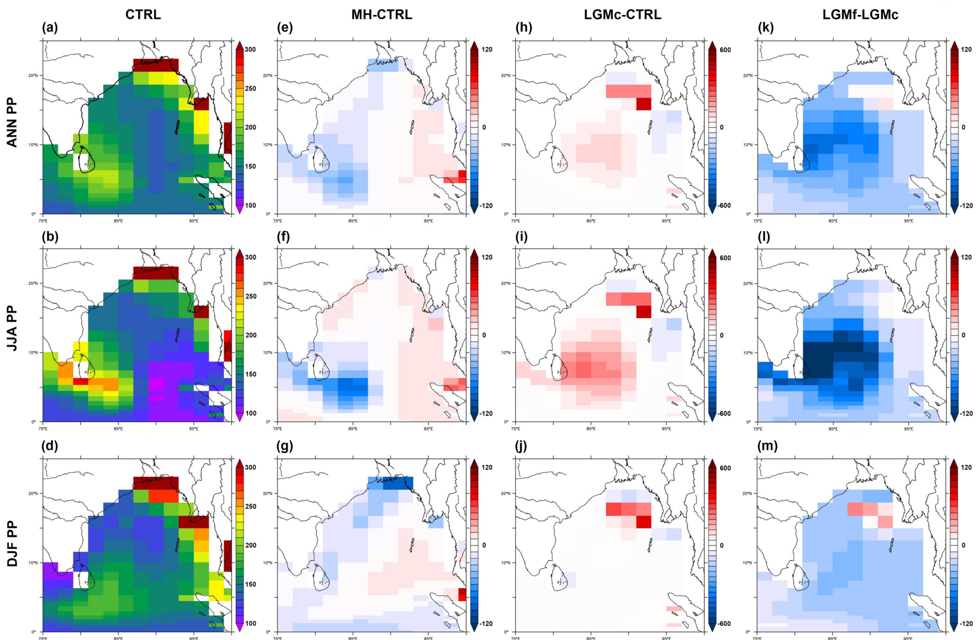

Fig. AC2. Simulated PP of CTRL and PP differences between MH and CTRL, LGMc and CTRL, and LGMf and LGMc. Results of annual mean, JJA mean and DJF mean are shown. PP is in gC m^{-2} yr^{-1}.

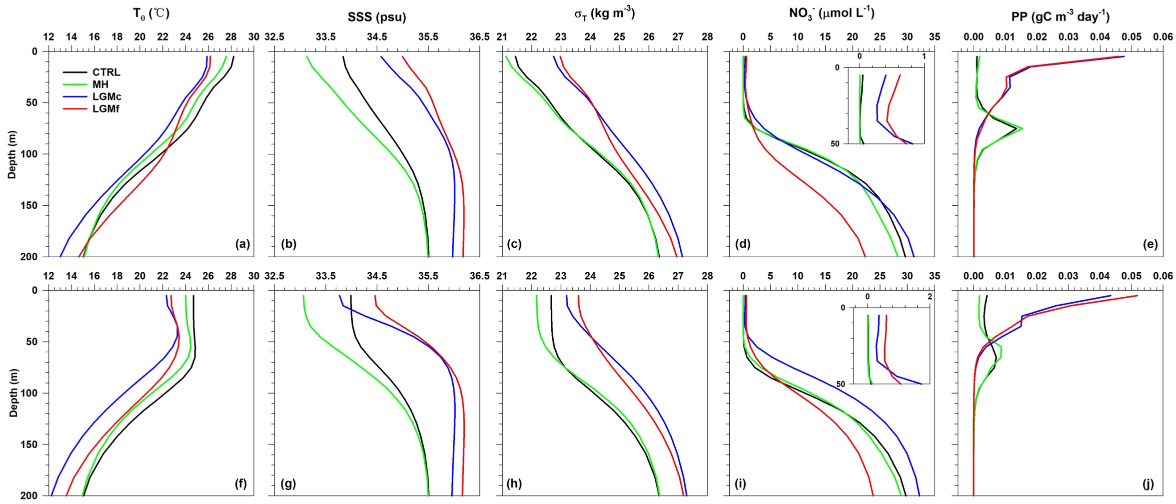

Fig. AC3. Simulated ocean profiles in four experiment run with IPSL-CM5A-LR. (a)–(e) Results of JJA mean. (f)–(j) Results of DJF mean. Grid of data extracting see Fig. AC12. The parameters shown here are potential temperature (T_θ), sea surface salinity (SSS), potential density (sigma-t, σ_T), nitrate content of seawater (NO_3^-) and total primary productivity (PP).

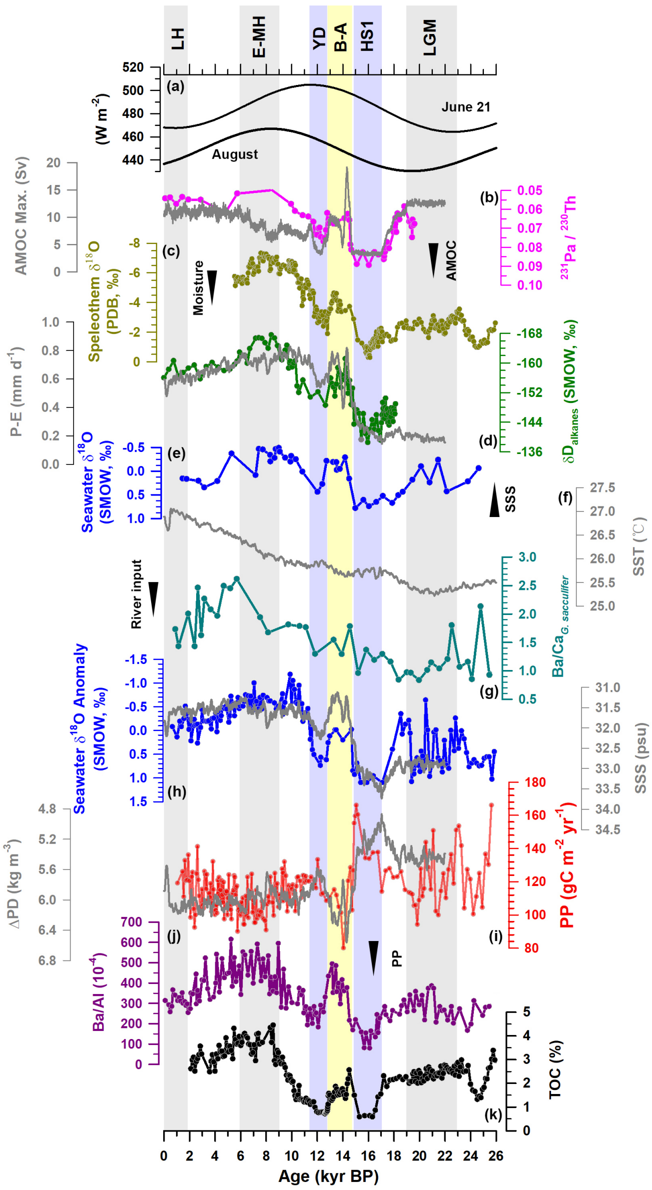

Fig. AC4. (a) August mean insolation and at 25°N. (b) AMOC strength indicated by $^{231}Pa/^{230}Th$ ratio of marine sediment from the western subtropical Atlantic Ocean (in pink, McManus et al., 2004). The changes of the maximum in the AMOC stream function below 500 m (AMOC strength) in TraCE-21 (in gray). (c) Mawmluh Cave speleothem $\delta^{18}O$ (Dutt et al., 2015). (d) Alkane δD in marine sediment, core SO188-342 (in green, Contreras-Rosales et al., 2014) and simulated precipitation minus evaporation of TraCE-21 (in gray). (e) Seawater $\delta^{18}O$ record of core RC12-344 (Rashid et al., 2007). (f) Simulated SST in the NE-BoB. Grids of data extracted see Fig. S2. (g) Ba/Ca ratios derived from mixed layer foraminifer species Globigerinoides sacculifer from core SK 168/GC-1(Gebregiogis et al., 2016). (h) Seawater $\delta^{18}O$ anomaly record of core MD77-176 (Marzin et al., 2013). (i) Estimated PP record of core MD77-176 (this study, in red) and simulated potential density gradient between 200 and 5m of TraCE-21 (in gray). (i) Ba/Al ratio of marine sediment, core 905 (Ivanochko et al., 2005). (j)

Total organic carbon weight percentage of marine sediment, core SO90-136KL (Schulz et al., 1998). Core locations of all these records above are marked in Fig. 1a. TraCE curves are shown using 100-yr averaged results.

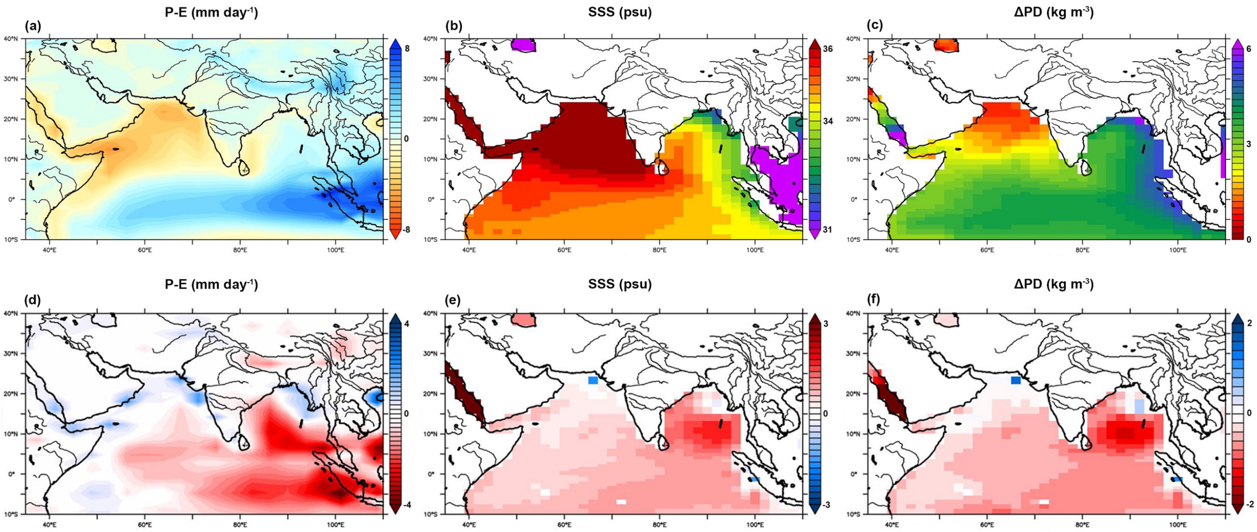

Fig. AC5. (a)–(c) Annual mean precipitation minus evaporation (P-E), sea surface salinity (SSS) and potential density gradient between 200 and 5 m of CTRL. (d)–(f) Differences of the same parameters between LGMc and CTRL.

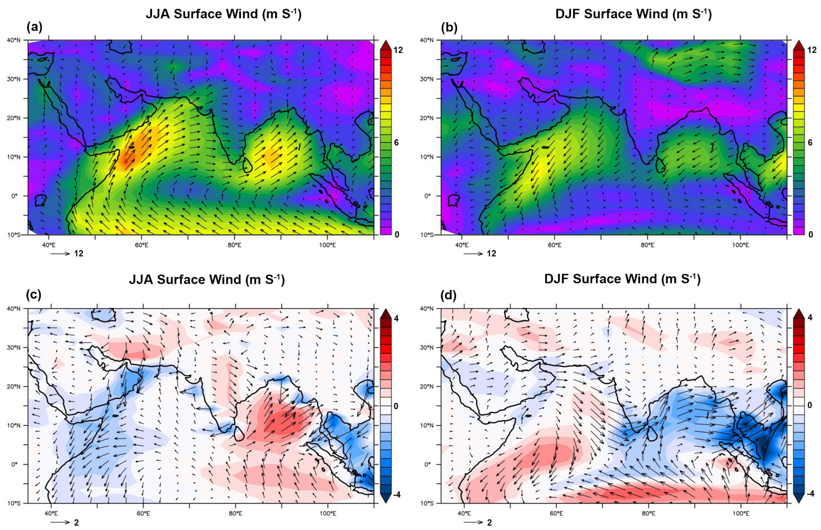

Fig. AC6. (a) and (b) JJA and DJF mean surface wind speed and vectors of CTRL. (c) and (d) Differences of the same parameters between LGMc and CTRL.

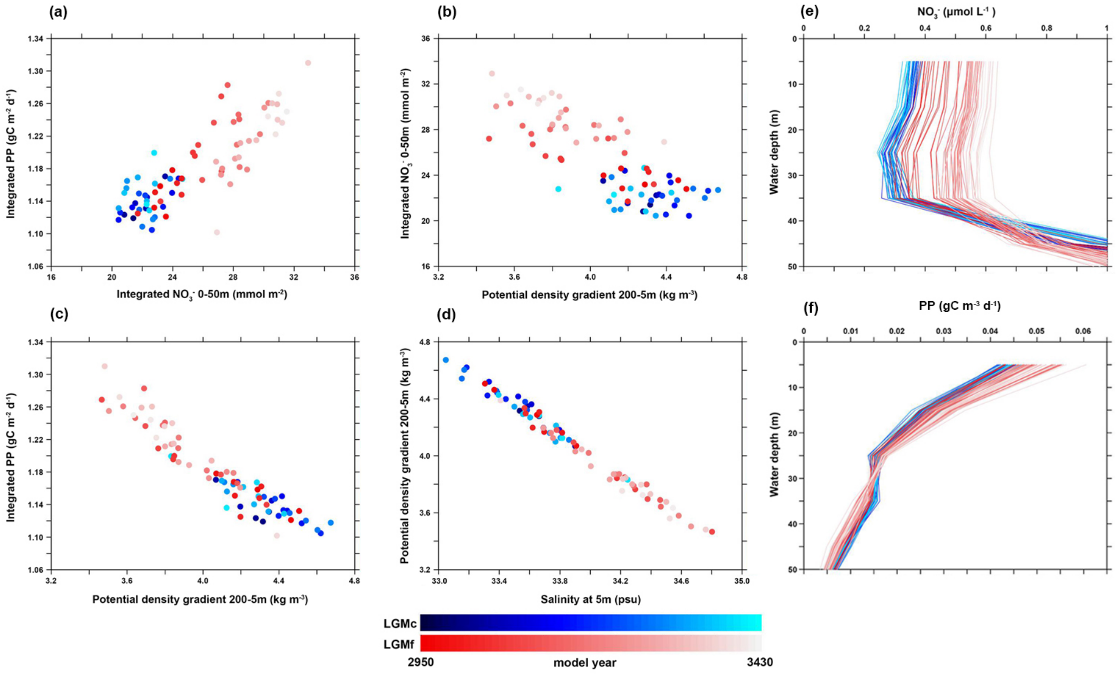

Fig. AC7. (a)–(d) Crossplots between different oceanic parameters of LGMc and LGMf (grids of data extracted see Fig. AC12). (e) and (f) Vertical profiles of nitrate content and PP of LGMc and LGMf (grids of data extracted see Fig. AC12). All the results are DJF mean and every curve represents an average of ten model years.

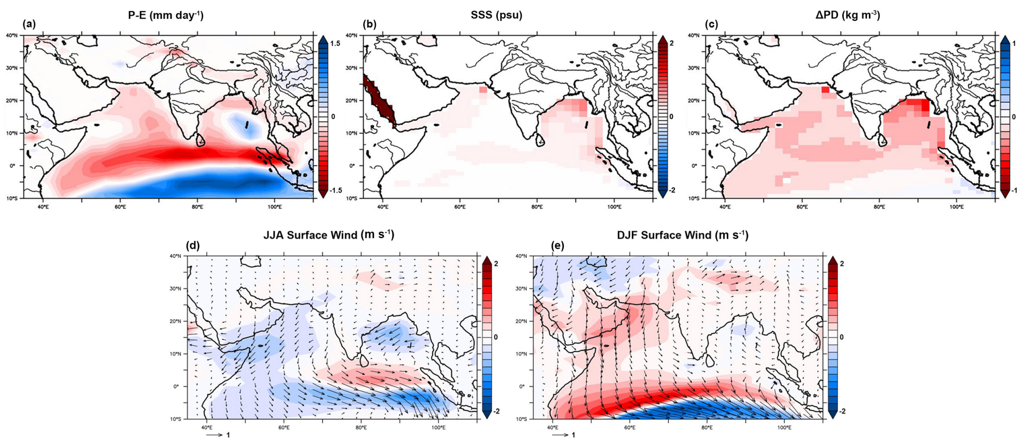

Fig. AC8. As in Fig. AC5 (d)–(f) and Fig. AC6 (c) and (d), but between LGMf and LGMc.

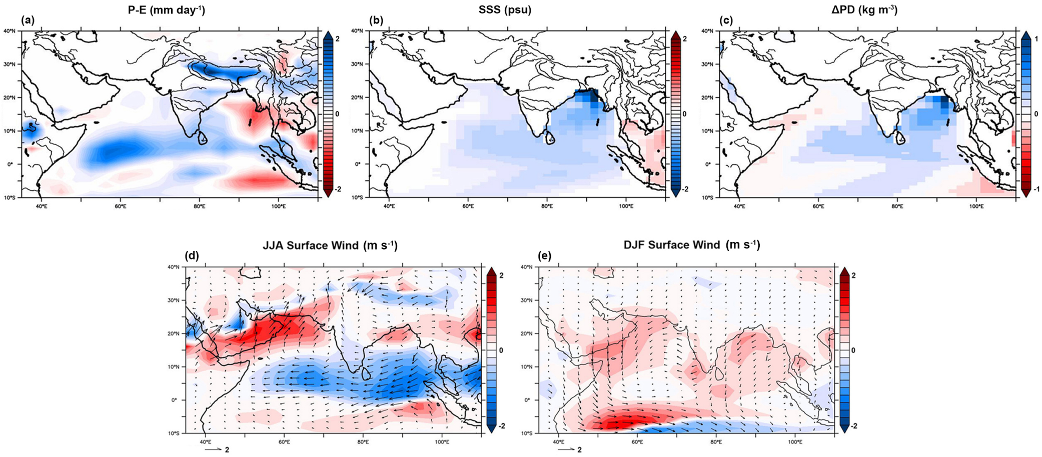

Fig. AC9. As in Fig. AC5 (d)–(f) and Fig. AC6 (c) and (d), but between MH and CTRL.

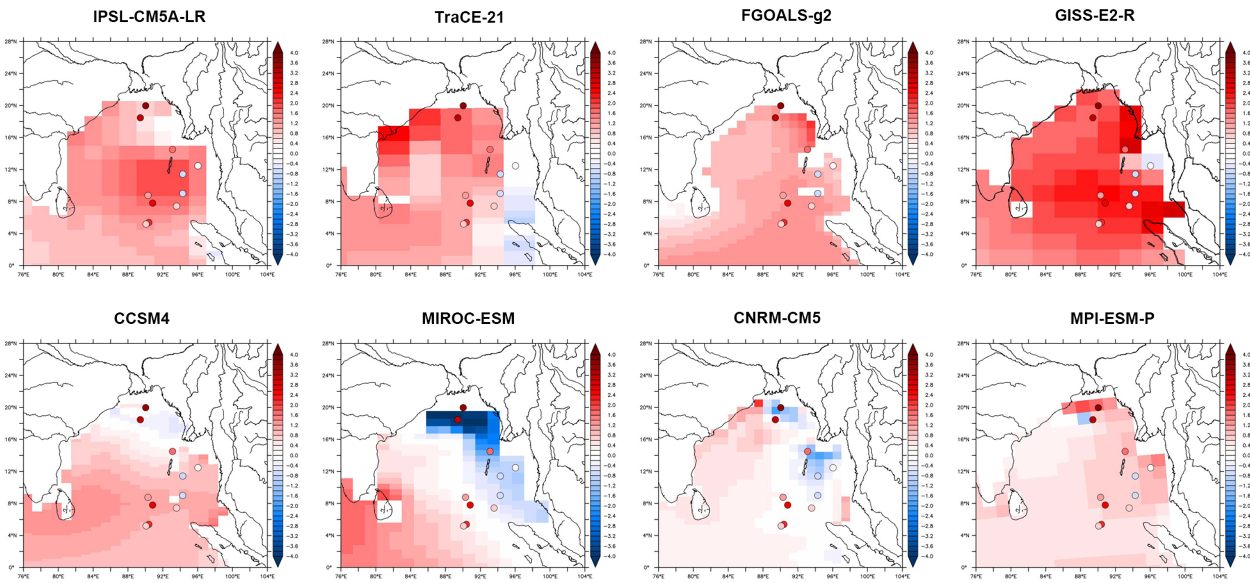

Fig. AC10. PMIP3 models and TraCE-21 outputs. Result of SSS difference between LGM and CTRL (late Holocene for TraCE-21) of PMIP3 models and TraCE-21. PMIP3 data source is Earth System Grid Federation (https://esgf-node.ipsl.upmc.fr/projects/esgf-ipsl/). The dots mark the results of reconstructed SSS (see Sijinkumar et a., 2016)

Supplementary Figures

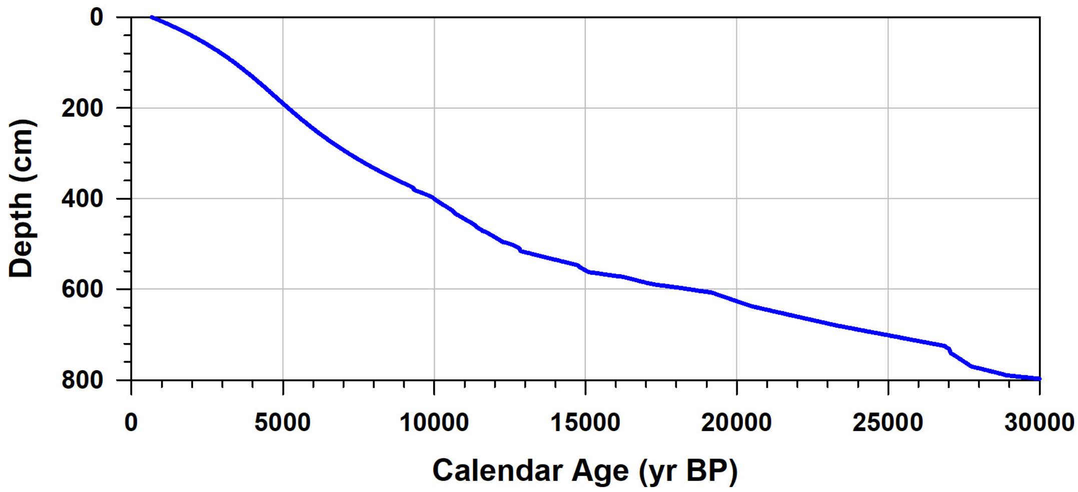

Fig. AC11. Tuned age model of MD77-176. The age model used in this studied is a tuned model constructed by Marzin et al., 2013. Details can be found in that article.

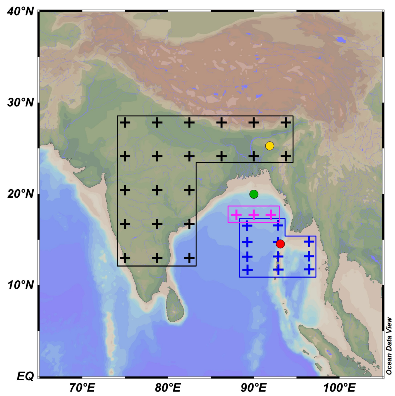

Fig. AC12. The grids of data extracting. Black cross are grids for TraCE-21 atmospheric outputs. Blue grids are for TraCE-21 oceanic outputs. Pink cross are grids for IPSL-CM5A-LR oceanic and biogeochemical outputs

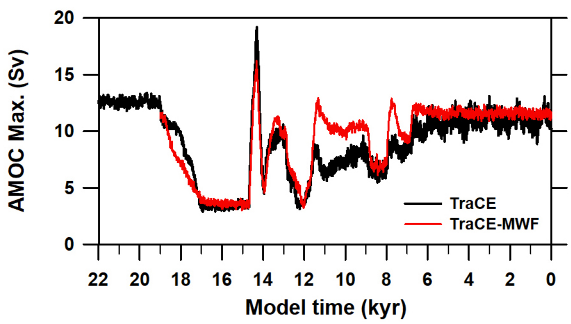

Fig. AC13. Changes of the maximum in the AMOC stream function below 500 m (AMOC strength) in TraCE and melt water of ice sheets single forcing simulation (MWF).

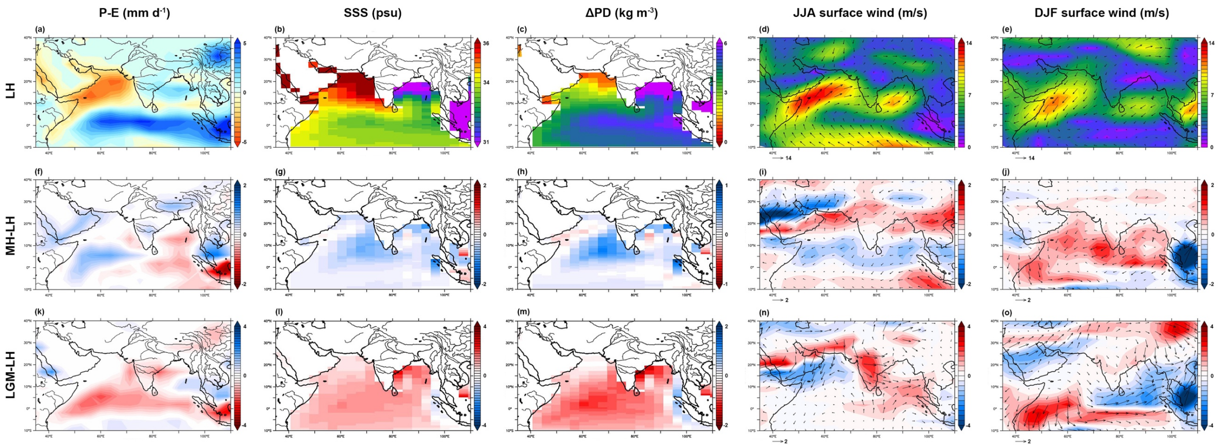

Fig. AC14. Results of TraCE-21 of three periods mean are shown: late Holocene (LH, from 1 kyr BP to presen), middle Holocene (MH, from 6.5 to 5.5 kyr BP), and the Last Glacial Maximum (LGM, from 23 to 19 kyr BP). (a)–(e) Annual mean precipitation minus evaporation (P-E), sea surface salinity (SSS) and potential density gradient between 200 and 5 m, JJA mean and DJF mean surface wind of LH. (f)–(j) Differences of the same parameters between MH and LH. (k) –(o) Differences of the same parameters between LGM and LH.

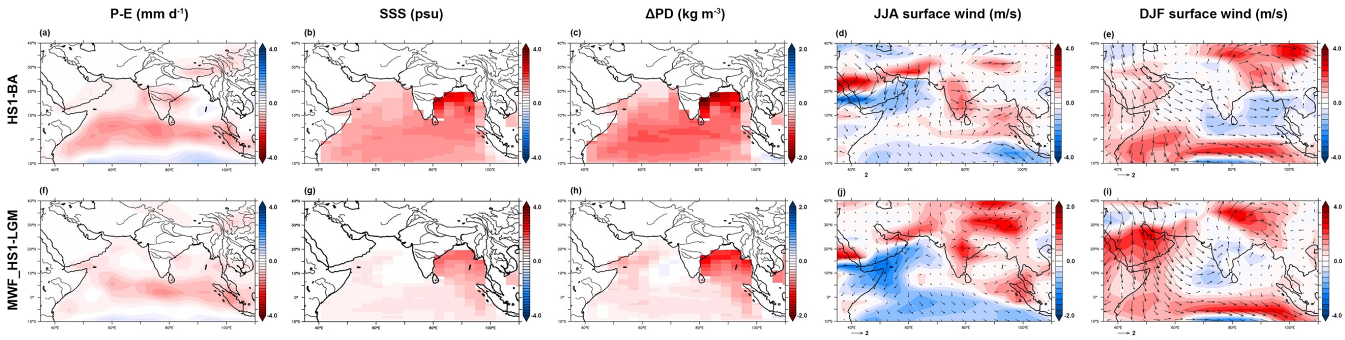

Fig. AC15. Results of TraCE-21 of three periods mean are shown: Bølling-Allerød (BA, from 14.5 to 13 kyr BP), and Heinrich Stadial 1 (HS1, from 17 to 15.5 kyr BP), and the Last Glacial Maximum (LGM, from 23 to 19 kyr BP). (a)–(e) As in Fig. AC13 (f)–(j), but between between BA and HS1. (f)–(j) As in Fig. AC13 (f)–(j), but between HS1 (melt water single forcing simulation) and LGM (full simulation). We can see the results are similar to the differences of the same parameters between LGMf and LGMc (see Fig. AC5, AC6).

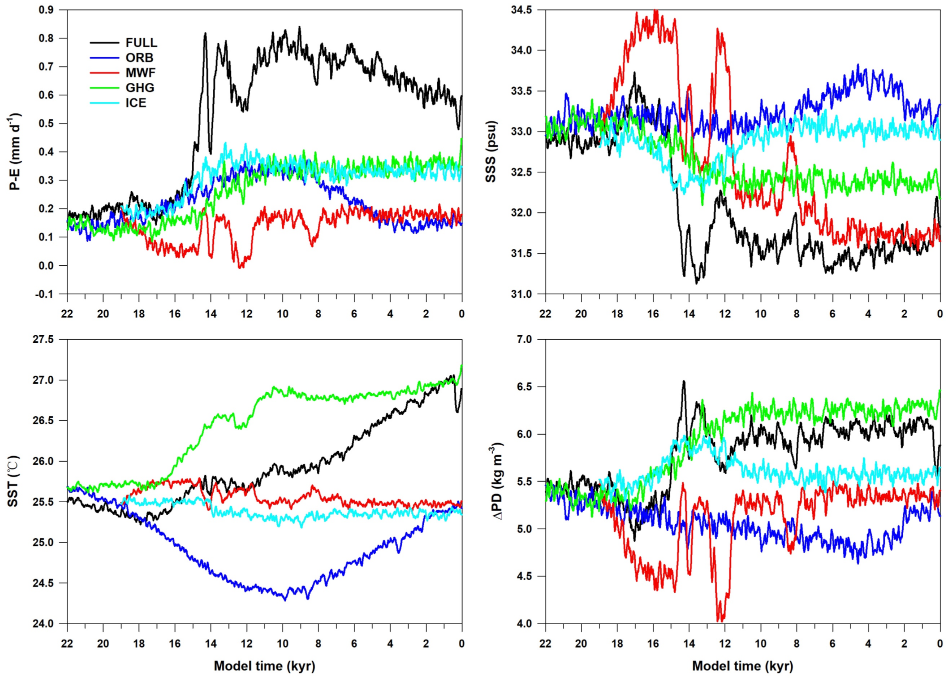

Fig. AC16. Annual mean results of precipitation minus evaporation, SSS, SST and potential density difference between 200 and 5 m (ΔPD) in TraCE-21 simulation (FULL) and single forcing experiments. The single forcing experiments are with other forcing fixed at their values at 19 kyr BP and forced by changing orbital insolation (ORB), green-house gas concentration (GHG), meltwater flux (MWF) and ice sheet (ICE). During the last deglaciation from 19 to 11 kyr BP, we can see that the millennial-scale variations of these parameters are mainly contributed by MWF forcing which moderated AMOC strength. The changes of SST during the deglaciation is very limited.