Response to Editor's comments

We thank the editor for the positive assessment of the revised manuscript and for the remaining comments, which have helped us to clarify the model description and simplify the interpretation of the millennial-scale mechanisms. We have revised the manuscript and the response to reviewers accordingly.
The editor's comments are shown in purple, our responses in black, and the modified text in green.

1) Details missing on the LOVECLIM experiments

Please be clearer about the LOVECLIM experiments. Are they those described in Yin et al., 2021 or are they different? If they are those used in Yin et al., then replace "performing a series of experiments" with by "analysing…" on L., 102.

We thank the editor for pointing out that the description of the LOVECLIM simulations was not sufficiently clear. The simulations used in this study are those described in Yin et al. (2021). Accordingly, we have replaced "performing a series of experiments" with "analysing transient simulations performed with the LOVECLIM Earth System Model described in Yin et al. (2021)" in the Introduction.

Also, L. 102 and 222 refers to a "set of transient experiments" but the only description is "driven by time-varying insolation and CO2 across two precessional cycles over MIS 11 (Yin et al., 2021)"; and L. 223 further state "This experiment". If it is a set, then clearly state the different experiments that are performed, if not then simply refer to "the experiment". Please also include the time period covered by the simulation as well as the references for the orbital and CO2 forcing.

We have revised the sentences to "we compared the pollen-based reconstructions with two LOVECLIM1.3 transient simulations described in Yin et al. (2021) (Fig. 4f). The simulations span the interval from 424 to 382 ka BP, covering two precessional cycles over MIS 11. One simulation is driven by time-varying insolation (Berger and Loutre, 1991) and CO_2 (Lüthi et al., 2008); the other is driven by time-varying insolation only, with CO_2 being fixed at 280 ppmv. In both simulations, the ice-sheet configuration is prescribed at present-day conditions."

Please also note that the reviewers' questions on the climate simulations were not answered properly (p4 of your answers to reviewers' comments).

We thank the editor for pointing this out and we also apologize for the incomplete response to the reviewers' questions on the climate simulations in the previous response file. This was an oversight during the consolidation of the author responses.
Please find below a clarification addressing the points raised by the reviewers:

Reviewers: 3) Climate model simulations

L192-194: Do the model simulations cover the full study interval, i.e. MIS 11? If so, please present the full simulated time span (370–428 ka) in Fig. 4. Notably, I am surprised that no millennial-scale variability or AMOC reduction is evident in the current modelling results. As shown in Fig. 1 of Yin et al. (2021), pronounced abrupt AMOC reductions have been documented. Therefore, if the present manuscript indeed uses the same model outputs as Yin et al. (2021), one would expect to see an associated abrupt precipitation change around ~398ka. Please clarify this apparent inconsistency.

The model simulations used in this study were performed by Yin et al. (2021). They focus on MIS 11c and cover only the interval from 424 to 382 ka BP, as presented in Fig. 4f.

Indeed, Yin et al. (2021) simulated an abrupt weakening of the AMOC at 398.7 ka BP, but no abrupt change in precipitation is simulated over the CMZ at the same time. This could be explained by two factors: (1) the abrupt AMOC weakening at the initial stage of glacial inception is relatively weak (~30%) and may therefore be insufficient to overcome the strong precessional control on CMZ precipitation; and (2) the AMOC changes simulated by Yin et al. (2021) occur on multi-decadal to multi-centennial timescales, over which internal variability in CMZ precipitation may be strong enough to obscure the impact of the AMOC. As shown by Yin et al. (2021), abrupt changes in precipitation and vegetation associated with AMOC weakening are simulated in some regions, for example northern Eurasia. Therefore, the expression of abrupt AMOC-driven changes is likely regionally dependent.

L434-456: Nearly all statements in this section are inferences without direct support from the modelling results, which makes the purpose of including the modelling component unclear. In particular, LOVECLIM includes relevant experiments that could help disentangle the respective roles of CO_2, ice sheets, and insolation in orbital-scale ISM variability. As noted earlier, the full MIS 11 simulation output should be shown in Fig. 4. Furthermore, if CO_2 and ice sheets are indeed the primary drivers of orbital-scale variability in the Indian CMZ, one would expect a relatively weak precessional signal in the record. Please explicitly separate the orbital-scale signal and confirm this. Finally, in Fig. 4f, why is the simulated PI level much weaker than during MIS 11, especially given that CO_2 levels are lower than PI values?

L464-478: The rationale for the modelling experiments remains unclear and somewhat confusing. The modelling results do not appear to support the pollen-based interpretation that emphasises the roles of CO_2 and ice-sheet dynamics in driving orbital-scale vegetation changes in the Indian CMZ. Under these circumstances, the inclusion of the modelling component may not strengthen the manuscript's conclusions, but instead risks confusing readers about the main findings. Please clarify the purpose and added value of the modelling experiments.

We thank the reviewer for these two comments. As they relate to a similar concern, we address them together below.

Please let us clarify that in terms of the forcing mechanisms controlling CMZ precipitation and vegetation, we divide MIS 11 into two distinct periods: the MIS 11c interglacial optimum, characterized by high and relatively stable CO_2 concentrations and interglacial ice-sheet conditions, and late MIS 11, during which CO_2 declined and ice sheets expanded. For the warm MIS 11c interval, we suggest that precession-driven boreal summer insolation played a dominant role in controlling CMZ precipitation and vegetation. The reconstructed CMZ vegetation closely follows insolation and precession. However, during late MIS 11, glacial boundary conditions, namely declining CO_2 and increasing ice volume, appear to have played a more important role, with reconstructed vegetation following CO_2 and ice-volume changes more closely than precession. We have restructured Section 4.4 in the revised manuscript to make this distinction clearer.

As the LOVECLIM1.3 simulations mainly cover the warm MIS 11c interval (see our reply to the comment above), the rationale for using these simulations is to specifically assess the controlling factors of tropical vegetation and summer monsoon variability in India under interglacial conditions. Consistent with our pollen reconstructions, these simulations show that vegetation and ISM variations in the CMZ during the warm interglacial interval are predominantly driven by precession and insolation. Therefore, one of the added values of the modelling is to support the conclusion drawn from our pollen reconstructions regarding the forcing mechanisms of precipitation and vegetation under interglacial conditions.

To make this rationale clearer, we now specify in the Methods section: "To evaluate the drivers of vegetation and climate variability during interglacial conditions, we compared the pollen-based reconstructions with two LOVECLIM1.3 transient simulations described in Yin et al. (2021) (Fig. 4f). The simulations span the interval from 424 to 382 ka BP, covering two precessional cycles over MIS 11. One simulation is driven by time-varying insolation (Berger and Loutre, 1991) and CO_2 (Lüthi et al., 2008); the other is driven by time-varying insolation only, with CO_2 being fixed at 280 ppmv. In both simulations, the ice-sheet configuration is prescribed at present-day conditions."

Our conclusion regarding the important roles of CO_2 and ice-sheet dynamics in CMZ precipitation and vegetation during late MIS 11 is based on comparison of our pollen reconstructions with insolation, CO_2 and sea-level/ice-volume records. The simulations used in our study do not cover the full late MIS 11 interval and were not designed to disentangle the individual effects of CO_2, ice sheets and insolation under glacial-inception boundary conditions. Separating these forcings using dedicated model simulations would be an interesting objective for future work.

Another added value of the modelling is that it shows that the CMZ region was considerably wetter and more forested at the climate optimum of MIS 11c than during the pre-industrial period. This is because local boreal summer insolation during MIS 11c was much higher than during the pre-industrial period, and because insolation is the dominant controlling factor for CMZ precipitation and vegetation under interglacial conditions. To make this clearer, the related sentence has been revised to: "Notably, according to our simulations, conditions in the CMZ of India were considerably wetter and more forested at the climate optimum of MIS 11c than during the pre-industrial period (Fig. 4f), due to much higher boreal summer insolation (Fig. 4g)."

2) Possible role of ENSO in explaining your results

L. 520-545 are very confusing. You first suggest that the ISM signal could arise from ENSO, but then state that there is much debate on ENSO impact on European climate, so then you discuss whether AMOC changes could have impacted ENSO. Some studies have found a link between AMOC changes and the ISM, why do you need to invoke ENSO, in particular for millennial-scale changes? This part of the text needs to be simplified and follow a logical flow.

We thank the editor for this comment. We only retain ENSO as a tentative possibility for the Fe-1 event at ~406 ka, because this event occurred under full interglacial conditions, with low ice volume and no clear evidence for an AMOC reduction in the Site U1385 benthic $\delta^{13}C$ record. We therefore do not attribute Fe-1 directly to AMOC weakening. We have now revised the text to make this point clearer and more cautious. "However, the influence of ENSO on European climate, as well as the mechanisms involved, remains debated. One possible explanation for the common expression of dry conditions in India and southern Europe may involve the influence of North Atlantic circulation changes on large-scale atmospheric patterns. Although the high-resolution benthic $\delta^{13}C$ record from Site U1385 does not provide clear evidence for an AMOC reduction during the ~406 ka event, intra-interglacial AMOC instability has been documented throughout MIS 11c in the northern North Atlantic (Galaasen et al., 2020). In this context, some model experiments suggest that AMOC weakening can favour a more El Niño-like state by inducing a southward displacement of the ITCZ and modifying tropical Atlantic SST gradients, which may in turn affect the equatorial Pacific thermocline through atmospheric teleconnections (Timmermann et al., 2007; Dong and Sutton, 2007)."

3) L. 575-576: It would be appropriate to also refer to climate modelling studies showing the impact of an AMOC weakening on

As suggested, we have revised the sentence to include climate-modelling evidence supporting the proposed mechanism, as follows: The impact of millennial-scale North Atlantic variability on the ITCZ and, more broadly, on the tropical hydrological cycle has been shown in previous proxy and model studies of the last glacial period (e.g., Marzin et al., 2013; Mohtadi et al., 2014; Dutt et al., 2015; Ota et al., 2022; Zorzi et al., 2022).

Response to referee's comment

We thank both referees for their constructive comments and suggestions, which have helped us to improve the manuscript. We have revised the manuscript accordingly and provide a detailed point-by-point response below. The referees' comments are shown in purple, our responses in black, and the modified text in green.

Referee 3

Referees 3 has three major comments:
1. whether a consistent and synchronised age model has been established to support multi-proxy comparisons;
2. the lack of a clear and robust demonstration of orbital-scale signals in the presented records; and
3. the rationale, necessity, and added value of the modelling results within the overall framework of the study.

1) Age-depth Model
We acknowledge the concern of the referee regarding the chronological framework and his advice to develop a synchronized age model supporting the comparison between records and to make clearer the orbital-scale and millennial-scale components in the pollen data variability.
In the revised version of the manuscript, we propose a new age-depth model derived from AICC2012 as explained in the text. For that, we selected tie-points between the benthic $\delta^{18}O$ records from the IODP Site U1446 and the IODP Site U1385 that is provided with a chronology derived from AICC2012 (age-depth model developed by Nehrbass-Ahles et al. (2020)). Even though tuning benthic $\delta^{18}O$ records from different ocean basins hold uncertainties, it allows to provide a common chronology with the Epica Dome C CH_4 record (Nehrbass-Ahles et al., 2020) and the North Atlantic ODP 983 record and the GLT_syn record (Barker et al., 2019) and improve the comparison. Although the lower watermass of the Bay of Bengal may have a modified isotopic signature compared to the intermediate Indian Ocean, benthic $\delta^{18}O$ records from intermediate Indian Ocean and North Atlantic water masses are assumed to align at the beginning and end of Termination I and II (with a potential 1 ky lag at the

beginning of TII) and at the MIS 5-4 transition (Lisiecki and Stern, 2016). A propagated uncertainty remains however difficult to provide accurately because 1) no uncertainty is provided by Nehrbass-Ahles et al. (2020) for the AICC2012 derived chronology of Site U1385, 2) lead and lags between benthic $\delta^{18}O$ changes from different regions can vary over time and for the studied interval, therefore no information is available.

This synchronization of chronologies results in coeval millennial-scale changes between decrease in the high frequency component of the XRF calcium data from Site U1446 and the abrupt warming in the North Atlantic region (recorded at Sites ODP 983, U1385 and in the GLT_syn, cf. Fig S2) during the interval from MIS 10 to the end of MIS 12, as observed during the last glacial period and last Termination (Zorzi et al., 2022). Most importantly, even though synchronization using benthic $\delta^{18}O$ signals can hold uncertainties of several centuries to millennia, and that we cannot assess the exact timing of millennial-scale changes in the different regions, the sequence of millennial-scale event in North Atlantic record and in the Bay of Bengal records respect the isotopic stratigraphy.

We have modified the description of the model in text as follows:

"The original chronology of Site U1446 is based on the correlation of the benthic oxygen isotope ($\delta^{18}O_b$) record to the LR04 stack (Clemens et al., 2021). However, we constructed a new age-depth model to use a consistent chronostratigraphic framework because this study compares the Bay of Bengal record with the North Atlantic and EPICA Dome C records, all on the AICC2012 timescale. We selected three tie-points between the $\delta^{18}O_b$ records from IODP Sites U1446 and U1385 spanning the period from the MIS 10 glacial maximum to the end of MIS 11c. We used the AICC2012-derived chronology developed by Nehrbass-Ahles et al. (2020) for Site U1385. Since the Iberian margin site has a hiatus at Termination V (TV), we selected two additional tie-points using the ODP 983 $\delta^{18}O_b$ record (Raymo et al., 2004; AICC2012 timescale of Barker et al. (2019)), one at the base of TV and one at the MIS 13/12 transition. We acknowledge that tuning $\delta^{18}O_b$ records from different ocean basins may introduce uncertainties of several millennia, that may vary over time (Govin et al., 2015). However, when Site U1446 chronology is transferred to the AICC2012 timescale, millennial-scale decreases in the high-frequency XRF calcium signal (Ca_{XRF}) from Site U1446, observed from MIS 10 to the end of MIS 11c and at the base of TV, align with abrupt North Atlantic warming following ice-rafted debris (IRD) events recorded at Sites ODP 983, U1385, and in the GLT_syn data (Fig. S2). Such correspondence between an increase in surface water stratification in the Bay of Bengal and an abrupt warming in the North Atlantic following a Heinrich event has been observed for the last glacial period and last Termination (Zorzi et al., 2022). In addition, this new chronology gives an age of 426 ka to the abrupt decrease in Ca_{XRF} at the beginning of TV, which is consistent with the age of the TV abrupt warming recorded in the GLT_syn record (Barker et al., 2019) and at Bàsura Cave in northern Italy at 426 ± 2 ky (Hu et al., 2024) and with the end of the millennial-scale event of weak Asian monsoon recorded at Sanbao Cave at 426 ± 1 ky (Cheng et al., 2016). The age-depth model is based on linear interpolation between control points."

L107-110: The core of this study lies in the new pollen record combined with the improved age model. I recommend providing a dedicated figure illustrating the age model, together with its propagated uncertainties. This would constitute a key foundation for comparing the pollen record with other proxy records. In addition, please specify the age models used for the comparison records, for example, whether they are based on independent chronologies or synchronised to LR04. Without such a foundation, statements—particularly those concerning millennial-scale variability—should be treated with caution.

To show the difference between the original and new chronologies, we have modified Fig S1 in including Site U1446 benthic $\delta^{18}O$ data along both chronologies. We have also noted in the legend captions of the figures which chronology is used for the different records displayed.

2) Orbital-scale signal

Referee 3 pointed out to the lack of a clear and robust demonstration of orbital-scale signals in the presented records and recommended extracting the orbital-scale signal prior to the discussion from line 322-338, for example by applying a 5-kyr running mean to represent orbital-scale variability.

In the revised manuscript examined by the referees, Figure 3 already explicitly displayed the orbital-scale and millennial-scale components of the wet evergreen forest and xerophyte percentage records from Site U1446, extracted using a LOWESS smoothing. To further address the referee's concerns and strengthen the separation of the orbital-scale component, we have applied a gaussian smoothing with a fixed-window of 7 ky to the pollen data. Windows between 7 and 10 ky are commonly used to separate the millennial-scale variability of a time series from the changes attributable to orbital forcing (Barker et al., 2019, 2011; Desprat et al., 2024). The variations in orbital-and millennial-scale components obtained using LOWESS and Gaussian smoothing are consistent and therefore the new results using Gaussian smoothing do not change the interpretation and discussion of the pollen data. Considering the variations in time resolution of the record, using a 5-ky moving average does not appear as the most appropriated method. As shown in previous studies (e.g., Desprat et al., 2024), even though the choice of the filtering method and bandwidth is inevitably somewhat arbitrary, the resulting smoothed data and residuals are comparable. We are therefore confident that the orbital-scale changes derived from Site U1446 pollen data discussed in the manuscript are robust. The method section and figure 2a-e have been modified accordingly.

We acknowledge that the text from line 322 to 328 was including information not solely related to orbital-scale changes. We have modified the paragraph accordingly:

"During the stadial MIS 11b, Site U1446 pollen record indicates an expansion of a semi-arid savannah reflecting reduced summer monsoon rainfall. During the interstadial MIS 11a, the two intervals of forest expansion (388-380 ka and 375-370 ka), with higher values of total forest and wet evergreen elements during the first one, suggest two orbital-scale increases in summer monsoon in India of decreasing intensity. Variations in CH_4 concentrations during MIS 11b follow similar orbital-scale pattern (Fig. 2i). On a regional scale, monsoon activity also decreased in East Asia during MIS 11b and increased during MIS 11a intervals, as in the Indian summer monsoon domain (Cheng et al., 2016; Zhao et al., 2019; Wang et al., 2023) (Fig. 2g). However, the isotopic values exhibited by the Jinfuo and Sanbao cave records during MIS 11a are as negative as during MIS 11c, which would suggest that East Asian monsoon reach similar intensification, which contrast with markedly lower CH_4 concentrations and extent of tropical forest in India during MIS 11a. A recent study, based on a loess microcodium $\delta^{18}O$ record from East Asia and modelling experiments, also showed, in contrast to the Chinese speleothem records, that there was a variable magnitude of summer monsoon precipitation increase during precession minima of the last 400 ky, reflecting the glacial-interglacial variability (Zhang et al., 2025). Moisture source and transport pathway can also affect the $\delta^{18}O$ calcite signal (e.g., Liu et al., 2020). Heterogenous response of the different subsystems of the Asian summer monsoon may also be involved to explain the different signal of the speleothem records compared to Site U1446 record. Palaeoclimate profiles of the northern hemisphere largely mark MIS 11b as cooler, while MIS 11a presents two orbital-scale warmings, with the first one having a larger amplitude (e.g., Fawcett et al., 2011; Candy et al., 2014 and references therein; Grant et al., 2014; Nehrbass-Ahles et al., 2020; Sassoon et al., 2023). We infer that, akin to MIS 11c, the remarkable correspondence between low vegetation and CH_4 changes during MIS 11b and 11a (Fig. 2) mirror variations in the hydrological cycle that are primarily controlled by the ITCZ's mean position in response to the orbital-scale climate changes."

L317-321: On orbital timescales, the two speleothem $\delta^{18}O_{sp}$ records (Sanbao and Yongxing) exhibit similar patterns. Moreover, vegetation variability on orbital timescales is influenced not only by precipitation but also by temperature and other processes. Therefore, it is challenging to conclude that pollen records necessarily provide a better proxy for local hydrological changes than speleothem $\delta^{18}O$ records, although I agree that the new pollen record offers evidence supporting Tzedakis' hypothesis. If this interpretation is not intended, please clarify.

We do not intend to say that pollen provide a better proxy record than speleothem since each record has its advantages and disadvantages. We have rephrased the paragraph as follows:

"The Chinese speleothem isotopic signals, used to reconstruct the East Asian summer monsoon, effectively bear the tripartite division of MIS 11c with minimal isotopic values occurring during the mid-MIS 11c (Cheng et al., 2016; Zhao et al., 2019; Wang et al., 2023), even though age offsets and variability among individual speleothem records are noticeable (Fig. 2g). Site U1446 pollen record thus provides evidence for variations of monsoon intensity within the Indian summer monsoon domain during the MIS 11 interglacial period further supporting Tzedakis et al. (2009)'s hypothesis for MIS 11."

3) Climate simulations

L192-194: Do the model simulations cover the full study interval, i.e. MIS 11? If so, please present the full simulated time span (370–428 ka) in Fig. 4. Notably, I am surprised that no millennial-scale variability or AMOC reduction is evident in the current modelling results. As shown in Fig. 1 of Yin et al. (2021), pronounced abrupt AMOC reductions have been documented. Therefore, if the present manuscript indeed uses the same model outputs as Yin et al. (2021), one would expect to see an associated abrupt precipitation change around ~398 ka. Please clarify this apparent inconsistency.

The model simulations used in this study were performed in Yin et al (2021) and focus on MIS-11c and include only 382-424 ka as presented in Fig. 4f.

Indeed, Yin et al. (2021) simulated an abrupt weakening of the AMOC at 398.7 ka BP, but no abrupt change in precipitation is simulated over the CMZ at the same time. This could be explained by two factors: (1) the abrupt AMOC weakening at the initial stage of glacial inception is relatively weak (~30%) and may therefore be insufficient to overcome the strong precessional control on CMZ precipitation; and (2) the AMOC changes simulated by Yin et al. (2021) occur on multi-decadal to multi-centennial timescales, over which internal variability in CMZ precipitation may be strong enough to obscure the impact of the AMOC. As shown by Yin et al. (2021), abrupt changes in precipitation and vegetation associated with AMOC weakening are simulated in some regions, for example northern Eurasia. Therefore, the expression of abrupt AMOC-driven changes is likely regionally dependent.

L434-456: Nearly all statements in this section are inferences without direct support from the modelling results, which makes the purpose of including the modelling component unclear. In particular, LOVECLIM includes relevant experiments that could help disentangle the respective roles of CO_2, ice sheets, and insolation in orbital-scale ISM variability. As noted earlier, the full MIS 11 simulation output should be shown in Fig. 4. Furthermore, if CO_2 and ice sheets are indeed the primary drivers of orbital-scale variability in the Indian CMZ, one would expect a relatively weak precessional signal in the record. Please explicitly separate the orbital-scale signal and confirm this. Finally, in Fig. 4f, why is the simulated PI level much weaker than during MIS 11, especially given that CO_2 levels are lower than PI values?

L464-478: The rationale for the modelling experiments remains unclear and somewhat confusing. The modelling results do not appear to support the pollen-based interpretation that emphasises the roles of CO_2 and ice-sheet dynamics in driving orbital-scale vegetation changes in the Indian CMZ. Under these circumstances, the inclusion of the modelling component may not strengthen the manuscript's conclusions, but instead risks confusing readers about the main findings. Please clarify the purpose and added value of the modelling experiments.

We thank the reviewer for these two comments. As they relate to a similar concern, we address them together below.

Please let us clarify that in terms of the forcing mechanisms controlling CMZ precipitation and vegetation, we divide MIS 11 into two distinct periods: the MIS 11c interglacial optimum, characterized by high and relatively stable CO_2 concentrations and interglacial ice-sheet conditions, and late MIS 11, during which CO_2 declined and ice sheets expanded. For the warm MIS 11c interval, we suggest that precession-driven boreal summer insolation played a dominant role in controlling CMZ precipitation and vegetation. The reconstructed CMZ vegetation closely follows insolation and precession. However, during late MIS 11, glacial boundary conditions, namely declining CO_2 and increasing ice volume, appear to have played a more important role, with reconstructed vegetation following CO_2 and ice-volume changes more closely than precession. We have restructured Section 4.4 in the revised manuscript to make this distinction clearer.

As the LOVECLIM1.3 simulations mainly cover the warm MIS 11c interval (see our reply to the comment above), the rationale for using these simulations is to specifically assess the controlling factors of tropical vegetation and summer monsoon variability in India under interglacial conditions. Consistent with our pollen reconstructions, these simulations show that vegetation and ISM variations in the CMZ during the warm interglacial interval are predominantly driven by precession and insolation. Therefore, one of the added values of the modelling is to support the conclusion drawn from our pollen reconstructions regarding the forcing mechanisms of precipitation and vegetation under interglacial conditions.

To make this rationale clearer, we now specify in the Methods section: "To evaluate the drivers of vegetation and climate variability during interglacial conditions, we compared the pollen-based reconstructions with two LOVECLIM1.3 transient simulations described in Yin et al. (2021) (Fig. 4f). The simulations span the interval from 424 to 382 ka BP, covering two precessional cycles over MIS 11. One simulation is driven by time-varying insolation (Berger and Loutre, 1991) and CO_2 (Lüthi et al., 2008); the other is driven by time-varying insolation only, with CO_2 being fixed at 280 ppmv. In both simulations, the ice-sheet configuration is prescribed at present-day conditions."

Our conclusion regarding the important roles of CO_2 and ice-sheet dynamics in CMZ precipitation and vegetation during late MIS 11 is based on comparison of our pollen reconstructions with insolation, CO_2 and sea-level/ice-volume records. The simulations used in our study do not cover the full late MIS 11 interval and were not designed to disentangle the individual effects of CO_2, ice sheets and insolation under glacial-inception boundary conditions. Separating these forcings using dedicated model simulations would be an interesting objective for future work.

Another added value of the modelling is that it shows that the CMZ region was considerably wetter and more forested at the climate optimum of MIS 11c than during the pre-industrial period. This is because local boreal summer insolation during MIS 11c was much higher than during the pre-industrial period, and because insolation is the dominant controlling factor for CMZ precipitation and vegetation under interglacial conditions. To make this clearer, the related sentence has been revised to: "Notably, according to our simulations, conditions in the CMZ of India were considerably wetter and more forested at the climate optimum of MIS 11c than during the pre-industrial period (Fig. 4f), due to much higher boreal summer insolation (Fig. 4g)."

4) Other comments

The referee made two comments on the temperature influence on vegetation:

-L201-205: The interpretation in Clement et al. (2024), that shifts in major ecological groups primarily reflect changes in ISO rainfall, is based on two warm interglacial periods characterised by relatively stable CO_2 concentrations and thus limited changes in the background temperature state. This context should be carefully considered when discussing orbital-scale vegetation variability in the present study, which also spans glacial inception periods. For example, in lines 206–209, the dominance of semi-arid steppe vegetation may also reflect colder climatic conditions. I do not suggest that the current interpretation is necessarily incorrect, but I encourage the authors to provide a more explicit and unequivocal statement regarding what the records specifically indicate.

-L458: As noted earlier, is orbital-scale ISM variability unequivocally represented by pollen-based vegetation changes? What is the role of temperature in driving vegetation variability, and does ISM variability itself also involve temperature changes? Please clarify.

During the more glaciated intervals (mainly MIS 10 and 12), we characterized the vegetation as a semi-arid steppe for intervals with high values of grasses and xerophytes. This vegetation type is likely to be more closely related to a warm semi-arid steppe which mainly depends on the precipitation regime and thrives on dry conditions than a cold semi-arid steppe which is favored by dry and cold conditions as found in the mid-latitudes. At Site U1446, SST derived from Mg/Ca and TEX86 measurements during MIS 12, but also during other glacial stages such as the LGM or MIS 6, do not fall below 24.5 °C and 26.5°C, respectively (Clemens et al., 2021). In contrast, the semi-arid steppe that developed during the cold intervals of MIS 6 in Iberia (for instance HS11) is associated to Mg/Ca SST between 11 and 14°C in nearby ocean sea surface (Sánchez Goñi et al., 1999; Tzedakis et al., 2018). Even though glacial cooling can be greater on land than in the ocean (e.g. LGM-PI mean annual temperature anomaly in the tropics of -1.5 to -2.5 °C in the ocean versus -3 to -5 °C on land; (Kageyama et al., 2021)), temperatures probably did not cool sufficiently to favor a cold, semi-arid steppe such as seen in the mid-latitudes. LGM climate simulations estimate a mean annual surface temperature above 20°C and a winter temperature above 10°C in most of India (Liu et al., 2020). This temperature is still within the range of the hot steppe and desert bioclimatic zone (Kottek et al., 2006) suggesting that the expansion of a semi-arid steppe in India is more likely to be the result of a change in precipitation than a change in temperature.

If we cannot rule out the possibility that temperature played a role on vegetation during MIS 11, its impact on the major ecological group shifts was probably minor compared to that of precipitation. For example, during most intervals of MIS 11a and 11b, when tropical forests shrank, deciduous tropical trees persisted in significant proportions alongside the expansion of Poaceae, indicating the expansion of tropical savanna landscapes at the expense of tropical monsoonal forests. In present-day vegetation, such a change is only due to differences in precipitation when temperatures stay above 18°C. Otherwise, Site U1446 does not record an expansion of high-altitude wet evergreen or temperate deciduous elements as observed in the Western Ghats and the Himalayas (Bonnefille et al., 1999) during the colder periods, suggesting that temperature alone cannot explain the recorded MIS 11 vegetation changes. In addition, vegetation simulations from the LOVECLIM transient model experiments for MIS 11 and MIS 13 show a strong correlation between the tree fraction and annual precipitation in South Asia, with a very weak relationship with the growing degree days above 0 (Su et al., 2023).

To consider the referee's comment we have added some information at the beginning of section 4.1:

"Site U1446 pollen record shows two intervals characterized by an open vegetation, mainly composed of grasslands and semi-arid taxa (mostly Poaceae and Amaranthaceae) that correspond to the glacial stages MIS 12, 433-426 ka, and MIS 10, 363-370 ka (Figs. 2 and S3, Table S2). The dominance of semi-arid steppe during these intervals indicates prevailing dry conditions and very low ISM rainfall. Cooler conditions during these glacial stages may also have favored the opening of the vegetation. However, in the tropical regions such as India, cooling is likely to have remained small compared to precipitation change. SST records from Site U1446 show temperatures that stayed above 25 °C even during the glacial maxima of the last million years (Clemens et al., 2021). Land temperatures were probably lower than ocean temperatures (Kageyama et al., 2022), although LGM climate simulations predict an annual surface temperature above 20°C and a winter temperature above 10°C in most of India (Liu et al., 2020). Such temperatures still lie within the envelope of the hot steppe and desert bioclimatic zone (Kottek et al., 2006) suggesting that the expansion of a semi-arid steppe during glacial periods in India is more likely to be the result of changes in precipitation than temperature. In combination with climate change, the low CO_2 concentrations of the glacial periods may also have favored the expansion of open vegetation since C4 grasses are physiologically favored by reduced atmospheric CO_2 conditions (Ehleringer et al., 1997; Harisson and Prentice, 2003) but the high abundance of xerophytic plants which are C4 dicots attests to drier conditions during MIS 12 and 10.

The MIS 11 in central India, from ~426 to 370 ka, comprises three major forest phases that are interspersed with periods of more open vegetation, ranging from grasslands to semi-arid savannas (Fig. 2a-e). Based on the current relationship between vegetation distribution and precipitation in India, changes in the main ecological groups are interpreted as reflecting shifts in ISM rainfall (Clément et al., 2024, and references therein). Vegetation simulations from transient model experiments of MIS 11 show that the tree fraction is strongly correlated with annual precipitation in South Asia but has a very weak relationship with the growing degree days above 0 (Su et al., 2023). This suggests that temperature variations during MIS 11 may also have influenced vegetation in India (outside mountain ranges), but likely played a minor role."

L216: I also recommend specifying the exact figure panels being referred to. For example, "Fig. 2" should be revised to "Fig. 2e, d" here. There are many similar instances throughout the manuscript that should be corrected.

Done. The figure citations have been revised throughout the manuscript to specify the relevant panels where applicable.

L219: Based on Fig. 2e and d, the maximum abundance of tropical forest and wet evergreen forest taxa appears to peak at ~410 ka. Could the authors clarify why a broader interval of "419–408 ka" is given here? Is there a formal definition of maximum/minimum intensity that has not been described in the text? If not, I recommend specifying ~410 ka rather than using such a wide time window.

The broader interval is based on pollen results, i.e. on the zonation of the pollen diagram performed using constrained hierarchical clustering analysis as shown in Fig. S3. which allow defining intervals during which the pollen assemblages are significantly different from the adjacent zones. This a method commonly and classically used in palynology.

L223: The term "climatic thresholds" is introduced without prior definition. Please clarify what is meant here.

We have replaced “although not below the climatic thresholds required to sustain the wet evergreen forest” by “although humidity remained sufficient to sustain the wet evergreen forest”.

L233-236: How are the expansion periods defined here? Based on visual inspection, I do not find that the two proposed expansion phases coincide with decreases in δD. Please clarify.
Again, the forest expansion phases from 389 to 380 ka and from 375 to 370 ka correspond to pollen zones that are determined not only on visual inspection but using constrained hierarchical clustering analysis of the pollen data (Fig. S2). The two minima in dD values centered at 387 ka and 372 ka are therefore occurring during these expansion phases and the dD maximum at ~378 ka occurring along with the opening of the forest.
We have changed the text as follows:
“Subsequently, the warm stage MIS 11a (388 - 370 ka), features two major forest expansions of the tropical forest from 389 to 380 ka and 375 to 370 ka, as defined by constrained hierarchical analysis of the pollen data (maxima of tropical forest: 42%, wet evergreen forest: 21%), separated by a more open vegetation phase that suggests an ISM decrease (Fig. 2 and Fig. S2). The two forest expansions during MIS 11a coincide with decreases in δD values (Fig. 2).”

L241: “Forest event” should be capitalised as “Forest Event”.
Since forest events are labelled “Fe-X”, we don’t think necessary to capitalized as proposed.

L295-297: it would be helpful to explicitly spell out the proxy records referred to here (e.g. $\delta^{18}O$, δD, or Antarctic ice-core records). Ideally, a figure comparing these multiple records should be provided, which would likely serve as a key reference figure for understanding the broader climate evolution during MIS 11c.
The tripartite division mentioned in this sentence is a widely identified pattern of MIS11c, largely demonstrated in other works (e.g., Tzedakis et al., 2022). We do not think necessary to add a figure dedicated to show this point.

L309: What is meant by “two comparable peaks” here? Is this referring to Fig. 2h?
We have replaced the sentence “the northern records show a plateau during MIS 11c or two comparable peaks” by “Unlike proxy records from low and mid-latitudes, the northern latitude records, such as Lake El'gygytgyn in Siberia (Melles et al., 2012), show strong warming from the beginning of MIS 11c”

L316-317: What are the “several distinct patterns” referred to here?
We have rephrased the sentence as follows:
“Yet, the speleothem record and the CH_4 record exhibit differences in terms of structure and phasing throughout MIS 11, particularly after 408 ka.”

L334-336: Are humidity/aridity here assumed to be equivalent to strong/weak monsoon rainfall or circulation? In addition, the authors highlight an important feature of Asian cave records: higher summer insolation does not necessarily lead to more depleted isotopic values (e.g. MIS 5e vs. 5c and MIS 11c vs. 11a). Could the pollen record provide further insight into this issue?
See response in section 2) Orbital-scale changes.

L355-357: What mechanism could account for the prolonged or more frequent El Niño conditions around ~408ka? Several studies have documented AMOC disruption events during this interval (e.g. Galaasen et al., 2020,*Science*, 367, 1485–1489). Could this provide an alternative explanation?
To comply with this comment, we have added the following paragraph:
"This common link may be related to the influence of AMOC changes on large-scale atmospheric circulation. Even though the high-resolution benthic $\delta^{13}C$ record from Site U1385 does not provide evidence for an AMOC reduction during the event at 406 ka, intra-interglacial AMOC instability has been documented throughout MIS 11c in the northern North Atlantic (Galaasen et al., 2020). While AMOC variability is known to strongly affect the European climate, some model experiments suggest that AMOC weakening may intensify ENSO, as a result of the southward shift of the ITCZ and modification of the SST gradient in the tropical Atlantic, which impact the equatorial Pacific thermocline via atmospheric teleconnections (Timmermann et al., 2007; Dong and Sutton, 2007). However, the ENSO response to an AMOC weakening varies between models, likely due to the difficulty of correctly simulating the tropical Pacific atmosphere–ocean system (Pausata et al., 2015)."

L406-408: Were the sharp rises in CO_2 driven by abrupt AMOC intensification?
The study of Nehrbass-Ahles et al. (2020) states that the Carbon Dioxide jumps synchronous with rapid and large rises in CH4 concentrations (CDJ+) indicate DO-like warming event directly linked to AMOC invigorations. Such CDJ+ events, named 11a.2, 11a.3, 11a.4 are synchronous with the strong increases in CH4 marking MIS 11a and abrupt AMOC invigoration as recorded in the $\delta^{13}C$ benthic record from Site U1385 (Nehrbass-Ahles et al., 2020)

L435-436: I do not observe a stable sea level during the early to mid MIS 11c, but rather a pronounced rise toward the highest sea-level stand during mid MIS 11c. Please clarify and revise the related statements accordingly.
The written text does not refer to a "steady sea-level" but a "steady sea-level rise"

L438: Is the age model synchronised with other proxy records? If not, this statement appears too strong and should be toned down. In addition, based on visual inspection of Fig. 4, I do not identify a clear lead of CO_2 maxima relative to ice-volume minima. Please clarify.
We have modified the text as follows, considering the new AICC2012 derived chronology for Site U1446:
"During the early and mid-MIS 11c, both the pattern and amplitude of the wet evergreen forest record track the precession-driven summer insolation at 20°N (Fig. 4e, g). The ISM strengthens at the two insolation maxima (precession minima at 409 and 428 ka), reaching its optimum at ~407 ka, nearly in phase with the second, strongest MIS 11 insolation peak, and weakens during at the intervening insolation minima (precession maximum at 419 ka). In contrast, the changes in forest and summer monsoon do not parallel the CO2 concentration and ice volume variations of MIS 11c which do not present the same tripartite division but a rather a regular sea-level rise until the mid-MIS 11c and high values of atmospheric CO2 concentrations from the early MIS 11c (Fig. 4e, h, i). In addition, high levels of CO2 concentrations (> 270 ppm) persist almost until the end of MIS 11c, well beyond the mid-MIS 11c ISM optimum while the wet evergreen forest gradually declines in tandem with decreasing summer insolation (Fig. 4e, g, h)."

Referee 2

108: I recommend including detailed information on the age–depth model in the Methods section, as it differs from previously published work.
109: Additional detail is needed regarding the addition of this control point, including how was it determined and what it represents In Figure S1, it would be helpful to clearly show the control points from Clemens et al.,2021 and which one is from this study, and to explain this explicitly in the figure caption.
As recommended, we have revised the chronological framework and now use a new age–depth model transferred to the AICC2012 timescale, allowing a more consistent comparison between the Bay of Bengal, North Atlantic, and EPICA Dome C records. The Methods section has been expanded to explain the selection of tie-points, the rationale for using Sites U1385 and ODP 983, and the main chronological uncertainties associated with tuning benthic $\delta^{18}O$ records from different ocean basins. We have also added a new supplementary figure showing the revised age–depth model, the control points used, and its comparison with the original chronology of Clemens et al. (2021).

127-135: References required here
Done.

137-40: This sentence is difficult to follow and would benefit from rephrasing.
Done.

151: Change levels to samples
Done.

151: I recommend removing the temporal resolution from this sentence and adding it to the information about the age model when it is moved to the methods section.
Done, it is now in the methods section.

206: Change to: The site U1446 pollen record
Done.

208: Change to: dominance of semi-arid steppe taxa
Done.

208: I would recommend using ‘indicates’ rather than ‘suggests’
Done.

210: Change to: Fig. 2.
Done.

208, 210, 212, 224: I suggest briefly outlining the dominant taxa in your record that support the characterization of “semi-arid steppe,” “open vegetation,” “wet evergreen forest,” and “tropical deciduous forest and grasses.” This will help readers understand the basis for these ecological classifications.
Done.

225: For those unfamiliar with this proxy and this study, briefly explain the biomarker measured (n-alkanes? Fatty acids?) and how and where it represents hydrological changes.

We have changed the text as follows:

"The δD_{precip} record from Site U1446, estimated from C28 n-acid in leaf waxes and reflecting ISM circulation (McGrath et al., 2021), does not display a decline during the first forest peak. However, the isotopic minima during the second forest peak indicate intensified ISM circulation during the mid-MIS 11c, followed by a reduction through late MIS 11c, in agreement with our reconstructions."

240: The current explanation of a forest event is quite confusing, as is the highlighting of these events in Figure 2. It gives the impression that a "forest event" refers to either a contraction or an expansion of forest, but in the figure the definition seems inconsistent: some forest events highlight mostly the contraction, while others highlight mostly the expansion. I recommend providing a clearer definition of a forest event and standardizing how these events are represented in the figure to avoid confusion. Furthermore, I find the use of the term forest event confusing, as I would interpret this term as a forest expansion- here it looks like it is describing periods of forest contraction which I don't think will be intuitive for the reader.

We have modified the text to primarily define the events as forest contractions.

"Based on the criteria outlined in Section 3 (Fig. 3), we identified four major tropical forest shifts (labelled Forest event Fe-1 to Fe-4) that correspond to events of significant contraction of the wet evergreen forest. These forest events reflect abrupt changes to drier conditions likely driven by ISM weakening. The first and most dramatic forest contraction, recorded at ~406 ka, is marked by a high-amplitude and abrupt decrease in the tropical forest (22.3% in ~1.6 kyr) mainly due to a decline of the wet evergreen forest at the expense of grasses (Poaceae) (Figs. 2b-e and 3). After Fe-1, the progressive decrease in tropical forest during late MIS 11 is interrupted by three significant forest contraction events, events Fe-2, -3, and -4, centered at ~387.7 ka, 383.3 ka and 374.6 ka, respectively (Figs. 2 and 3). Each event is also marked by large increases in xerophyte plants (maxima >21%) and concurrent declines in wet evergreen elements (minima <4%), indicating the expansion of semi-arid savannas in the studied region, which reflects the most severe drought conditions and reduced ISM during MIS 11. These high-intensity events are rapidly followed by a sharp increase in tropical forest taxa with wet evergreen elements (13% for Fe-2/Fe-3 and 23% for Fe-4, within 0.6-1 kyr), indicating abrupt transitions to wetter conditions driven by an intensified ISM."

516: change to: than during the preindustrial period

Done.

Figure 2:

• The arrows are ambiguous and it is not clear which features they are pointing to.

We have clarified this in both the main text and the figure caption. The caption now specifies that the arrows in panels (g) and (h) denote potential concurrent millennial-scale events.

• Reconsider the term "forest event" or define it clearly.

Done.

• The use of the brown highlights and dashed lines in this figure is confusing, as they do not appear to align consistently with the inferred forest contractions and expansions. For example, the dashed line marking FE-1 coincides with a decline in forest cover and an expansion of Poaceae, whereas the caption suggests this interval represents a forest expansion. In addition, the brown highlighted interval should be extended to encompass the full period of forest decline, which appears to continue until ~403 ka.

Finally, representing forest expansion solely with a dashed line is potentially misleading, as the expansion phases extend over longer intervals. These phases would be clearer if shown as shaded bands, analogous to the contraction intervals, to better capture the full duration of both forest expansion and contraction events.

Done. We have revised the figure to avoid confusion by removing the dashed lines previously used to indicate forest expansions.

• It would be helpful to include an arrow on the dDprecip graph to show which direction infers weber/drier.

Done. We have clarified this in the figure caption rather than adding an arrow to the graph, to avoid making the figure too visually crowded.

Figure 3:

• This is a matter of personal preference, but the use of solid colour shading would be clearer and more effective for highlighting forest expansion and contraction phases. It would also be helpful for the reader if the style of shading was consistent in figures 2, 3 and 4.

Done.