Authors' Response to Editor:

Dear Dr. Oliveira,

Thank you for your patience during the review process. The two reviews were generally positive in term of the importance of the data and interval of interest. I agree with this positive, so I invite you to submit a revised manuscript for further consideration.

Dear Editor,

Thank you for your constructive comments and the opportunity to revise our manuscript. We greatly appreciate both your comments and those of the reviewers, which have helped to significantly improve the manuscript. Enclosed, we have included the revised version of the manuscript and a "TrackChanges" document that highlights all the modifications we have made. Below, we have provided detailed responses to each comment, with the original comments in black and our responses in blue.

Though there were a few specific recommendations, the overall message from the reviews was that the manuscript was hard to follow and thus the linkages from motivation -> results -> implications weren't clear. As an author I can appreciate that this kind of non-specific feedback is frustrating to incorporate during revision, but I hope you'll see this as an opportunity to hone your message for easier digestion. I'm indicating "major revisions" because I'd like to reserve the ability to have the revised manuscript sent out for one more round of review. As requested, we have carefully revised the text throughout the manuscript to enhance readability and clarity. In particular, the Abstract and Section 4 (Results and Discussion) have undergone major modifications in both writing and structure to address the comments raised by the reviewers and ensure a clearer presentation of the key findings.

In addition to responding to the reviewer comments, please consider the following points when preparing your revised manuscript:

-I'd like to see some more detail on the development of the age-depth model. The addition of the new tie-point is obviously important for pollen-zone chronologies and some of the linkages made to other proxy records in the discussion. A supplemental figure with the superimposed U1446 and LR04 d18O(benthic) records and tie-points would help readers evaluate the suitability of the new control point and, in turn, confidence in the precision assigned to the ages in the manuscript. You mention age-model uncertainty at least once in the discussion, so please also provide an estimate of this uncertainty. We have added a new figure (Fig. S1) showing the age-depth model and mentioned the age uncertainty (lines 107-110 in the revised manuscript), following your suggestion.

-Again, I may have missed it discussion of this, but: is there any potential for biased interpretation of the pollen% data due to appearance/disappearance of prolific pollen producers, or due to changes in sedimentation rate/bulk density? i.e. if the pollen data were presented as fluxes would the interpretations still hold? Admittedly it was hard for me to draw linkages between the pollen zonation table and the Fig 2 pollen diagram, because of so many taxa with presumably only trace concentration. Good to see in your response documents that you plan to address Fig 2 clarity during revision. Pardon me if these questions are on the naive side! Presenting the data as fluxes would not alter the core interpretations because the focus of this study is on changes in vegetation composition rather than absolute abundance. Pollen percentages are well-suited for capturing these changes, as demonstrated by previous palynological studies (lines 135-140), with no evident bias from prolific pollen producers (e.g., *Pinus*) or changes in sedimentation rate (Zorzi et al., 2015; 2022; Clément et al., 2024). As requested, we have revised Figure 2 and moved it to supplementary information (Fig. S2), enabling Section 4 (Results and Discussion) to focus more directly on the main results and their implications.

We hope that you find the revised version suitable for publication in Climate of the Past and are looking forward to hearing from you.

Sincerely,

Dulce Oliveira, on behalf of the co-authors

Authors' Response to Reviewer #1:

This article is a high-quality piece of work with some important new data spanning an interesting climatic transition with material being delivered from central India and providing critical new constraints on the development of environment during interglacial periods within the core monsoon area. I believe that it is worthy of publication but it will need some rewriting before it could be accepted. I actually found the paper really difficult to follow and understand. The writing is very dense and there is so much use of abbreviations that it makes it very difficult to follow. Not everyone is going to be as familiar with all these abbreviations as the authors and I found their use obstructive to my understanding of the manuscript. I'm sure the data is a good quality and if I understood this correctly it suggests that during warm periods there was a greater prevalence of tropical forest in central India compared to other times but that the intensity of the Indian monsoon and the vegetation is regulated by orbital processes but also the extent of ice sheets in high latitudes. One of the other things that stopped me understanding the manuscript more fully was the use of the MIS stages rather than telling me whether it's warmer or a colder period. Again, the number of the stages and sub stages are probably very familiar to the author but not to all the readers and I think it might be helpful if they were to try and better describe what each provide more information about what each stage number means. The authors assume too much prior knowledge. I was a little confused about the comparison of the climate model and the data itself. The conclusion appeared to be that the model wasn't very good unless you accounted also for glacial variations which at least in my reading suggests at the model isn't that good and so I wonder why so much time was spent discussing the results. I provide here below a number of smaller editorial comments and questions to help improve the understanding of the manuscript.

We thank the reviewer for the positive feedback and constructive comments. In light of the aforementioned main comments, we have made substantial revisions throughout the manuscript to enhance understanding. Specifically: (1) The Abstract and Section 4 (Results and Discussion) have been extensively revised to address the reviewer's feedback and present the findings more clearly. Additionally, we have removed discussions beyond the scope of our study, such as those related to the global terrestrial biosphere. (2) We have simplified the language, broken down long, complex sentences, and reduced the use of abbreviations by removing or spelling out non-standard terms. (3) We have also provided clearer explanations for the MIS substages, specifying their ages and indicating whether they correspond to warm or cold periods.

Furthermore, we acknowledge the reviewer's concern regarding the discussion of the climate model results. While we agree that the simulations are not fully comprehensive, they still provide valuable insight into the impact of insolation and CO_2 during the MIS-11 optimum period. Addressing all the hypotheses raised in this study would require more advanced simulations incorporating interactive ice sheets and related freshwater fluxes, which we recognize as direction for future research in the manuscript (lines 512-515 in the revised manuscript). We also recognize that the model results play a limited role in our analysis and do not require extensive discussion. Therefore, in the revised manuscript, we reduced the text related to model interpretations while maintaining their key contributions.

Below, we address each of the reviewer's specific comments.

- Line 20 - core monsoon zone (CMZ) -What is that. The CMZ is the region most reflective of Indian summer monsoon rainfall variations as its variability closely mirrors the overall monsoon variability across India. We improved its description to enhance clarity (lines 110-113).
- Line 36 - ISM weakening could also occur under similarly warm future conditions – Triggered by what? Be specific. Here, we are referring to the monsoon changes triggered by global warming. The abstract has been revised to improve clarity and address the reviewer's comments.
- Line 43 - Core Monsoon Zone (CMZ) – Label this on a map. The CMZ is highlighted in the new Fig. 1 with a striped area, as requested.
- Line 140 - rapid delivery of continental material – Depends what you mean by "rapid". Could be a whole 100 ky glacial cycle for coarse grained sediment. There is likely buffering in the flood plain for fine material including the pollen. In response to this comment, we replaced "rapid delivery of continental material" with a more precise description (lines 135-137). Pollen at Site U1446 is primarily transported by rivers, with the Mahanadi River as the dominant contributor (Zorzi et al., 2022; Clément et al., 2024). Over 90% of its annual sediment discharge occurs during the summer monsoon, efficiently carrying pollen offshore. Due to

its hydrodynamic behavior, similar to fine silts and clays, pollen is transported with suspended sediments and exported beyond the shelf (e.g., Hooghiemstra et al., 2006). This process is favored by the region's narrow continental margin and submarine canyons, allowing pollen to rapidly sink through the water column (Raman and Reddy, 2001). These processes limit long-term buffering in floodplains, ensuring that the pollen record accurately reflects past regional vegetation changes.

- Line 163 - main sum - I'm not quite sure what this means. The term "main sum" refers to the total pollen count used for percentage estimates, excluding Himalayan taxa, aquatic taxa, and spores, as described in the methods section (lines 159-161).
- Line 163 - excluding Himalayan taxa – Why are there Himalayan taxa in the sample. Is the sediment really from the Mahanadi or perhaps the Ganges? Himalayan taxa are present in very low abundances in our samples likely due to minimal long-range transport (Clément et al., 2024, and references therein). However, multiple lines of evidence confirm that the primary source of the sediment at Site U1446 is the Mahanadi Basin rather than the Ganges-Brahmaputra system. The sedimentological characteristics, including elemental composition analyses from nearby IODP Site U1445, closely correspond to the lithology of the Mahanadi Basin. In addition, pollen transport in this region is dominated by fluvial input, with over 90% of the Mahanadi River's annual discharge occurring during the summer monsoon. Cross-shelf transport, favored by a narrow continental shelf and submarine canyons, ensures efficient delivery of sediment to the deep sea. In contrast, the Ganges-Brahmaputra plume is associated with significant flocculation at the river outlet, causing sediments to sink rapidly and limiting long-distance transport. Seasonal surface currents further reduce southward transport from the Ganges-Brahmaputra system. While a minor contribution from remote sources cannot be entirely ruled out, the extensive evidence supports the Mahanadi Basin as the dominant sediment source for Site U1446 (lines 135-140).
- Line 185 - primary indicator of ISM rainfall variability - But the monsoon is seasonal. Couldn't this be found in a tropical, non-seasonal environment? Line 238 - increasing dryness. – and seasonality? We agree with the reviewer that the monsoon is seasonal. However, in the core monsoon zone of India, 80%–90% of annual rainfall occurs during the summer monsoon (June–September) (Gadgil, 2003) (lines 110-113). In this sentence, we refer to the wet evergreen forest ecological group as the primary indicator of summer monsoon rainfall variability. This group includes wet evergreen, semi-evergreen and moist deciduous taxa, which thrive in the wettest forest formations of India due to their reliance on sustained high precipitation levels. Hence, their abundance in the sediments of Site U1446 is interpreted as reflecting summer monsoon rainfall variations (lines 177-182).
- Figure 2 – This chart is very hard to read. We have thoroughly revised Figure 2 by adding a color code for ecological groups and improving the representation of low-abundance pollen taxa. The figure has been relocated to supplementary information (Fig. S2), allowing Section 4 (Results and Discussion) to focus more clearly on the main results and their implications, as suggested.
- Line 250 - The two forest expansion - What does that mean? We acknowledge that the wording of "the two forest expansion" could be clearer. We have rephrased it to specify that it refers to two distinct intervals of tropical forest expansion during MIS 11a (lines 233-235).
- Line 300 - unusually high global terrestrial biosphere – Not sure what this means. Line 305 - contribution of tropical forests to the global terrestrial biosphere - you did not estimate volumes of forest or the volumes of carbon that would be captured by the forest so I don't think you really understand what proportion of the global budget this represents. In line with our efforts to improve readability, we have removed this discussion to maintain focus on the key factors driving vegetation changes and monsoon dynamics during MIS 11. The term "unusually high global terrestrial biosphere" was originally used to describe the concept of "exceptionally high biosphere productivity" introduced by Brandon et al. (2020) to explain how an extensive terrestrial biosphere during MIS 11c may have delayed the CO_2 increase following the MIS 12/11 transition. However, as we did not quantify the global carbon or biomass contribution of forests and this aspect is beyond the scope of our study, we have removed it. This avoids unnecessary complexity and enhances the manuscript's readability, as requested.
- Line 314 - asymmetric "M-shaped" structure – I'm unclear about what you mean. Are you talking about the shape of proxy record? Which one in particular? Yes, we are referring to the shape of the tropical forest percentage record during MIS 11, which exhibits two peaks, with the second being more pronounced,

separated by a dip, forming an "M-shaped" pattern. We clarified this more explicitly in Section 4 (Results and Discussion) (lines 213-216; 293-297).

- Line 325 - EDC ice core – what does EDC mean? And where was this core taken? EDC stands for EPICA Dome C, an ice core drilled in East Antarctica (Jouzel et al., 2007). is now mentioned in the revised manuscript (line 301).
- Line 327 - imprint on the CH4 record - what record are you talking about? What is a proxy for methane? The CH_4 record refers to atmospheric methane concentrations reconstructed from ice cores. Methane (CH_4) levels are measured from trapped air bubbles in the ice, being used as a proxy for past variations in global methane emissions (e.g., Petit et al., 1999; Loulergue et al., 2004).
- Line 339 - tropical wetland extent – how can a speleothem record tell you about the extent of wetlands? We agree that a speleothem record is not a direct proxy for tropical wetland changes (lines 313-321). In this sentence, we are referring to studies that have used the Sanbao speleothem record to infer wetland expansion. The underlying reasoning is that a northward shift of the intertropical convergence zone led to a poleward intensification of Northern Hemisphere westerlies, creating favorable conditions for new tropical wetlands to form. This expansion resulted in increased methane production, which was reflected in methane records.
- Line 343 – speleothem data alone cannot be used as a definitive proxy - or that these data indicate complicated hydrological system. We entirely agree with the reviewer, and this point is extensively acknowledged in the manuscript, particularly in Section 4. Results and Discussion (lines 317-321).
- Figure 5 - I suggest you use different colours when you're plotting two sets of data on the same plot. Using two shades of blue is a little confusing. Why don't you just use something more distinctive? As suggested, we adjusted the color scheme in Figure 5 to ensure clearer distinction between the datasets.
- Line 381 - The asymmetric M-shaped pattern – I don't know what you mean. You're going to need to label that quite clearly on one of your figures. This overlaps with the earlier remark on the 'M-shaped' pattern. As noted in our response to Line 314, we clarified the asymmetric M-shaped pattern in the text based on the reviewer's feedback. However, in line with our efforts to simplify Figures 4 and 5, we did not add additional visual elements to these figures.
- Line 426 - the absence of ice-sheet forcing and associated freshwater flux in the simulations - this seems like a major omission to me. Is it necessary to ignore the ice sheets in the model? Seems like a bad model. We understand the reviewer's concern regarding the absence of ice-sheet forcing and associated freshwater fluxes in the model simulations. As noted in our previous response regarding the model caveats, our study focuses on isolating the impact of insolation and CO_2 during the MIS-11 optimum. However, we recognize that excluding ice sheets and freshwater flux may contribute to the mismatch between model outputs and proxy records during the glacial inception (lines 471-478). This discrepancy further underscores the potential role of these factors in triggering abrupt climate events, reinforcing the need for future simulations that incorporate interactive ice-sheet dynamics, as stated in the manuscript (lines 512-515).
- Line 444 - Yongxing Cave -but that is east Asian monsoon and not entirely clear that it's appropriate. The reviewer is correct that Yongxing Cave, located in China, is influenced by the East Asian monsoon, as shown in Figure 1. In this sentence, we refer to a regional-scale comparison rather than changes strictly within the Core Monsoon Zone of India. The high-resolution speleothem record from Yongxing Cave provides a broader context for assessing monsoon variability and its potential link to the abrupt savannization event in India (lines 343-345).
- Line 472 - involves distinct in-cave processes – also, they may be issues related to regional rainfall pattern. We thank the reviewer for pointing this out. However, we did not address this aspect because the East Asian speleothem records discussed in our study are geographically very close to each other, as shown in Figure 1, and are, in principle, influenced by the same regional rainfall patterns.
- Line 541 - the asymmetric M-shaped pattern in our tropical forest record -I never really understood what you meant by this. This overlaps with a previous comment on the "M-shaped" pattern. As noted in our response to Line 314, we clarified this more explicitly in Section 4 (Results and Discussion) (lines 213-216; 293-297).

- Line 555 - CH_4 overshoots – I don't understand what that means. "Overshoots" is commonly used in paleoclimate research to describe sharp rises in proxy records. In our study, it refers to high-amplitude methane increases as identified by Nehrbass-Ahles et al. (2020) (lines 405-410).

Minor corrections such as typos, abbreviations, references, and small changes were made to improve the readability of the text:

Line 18 - Marine Isotope Stage (MIS) 11 - Should say how old this is in ka.
Line 28 - MIS 11c - Again, we need the ages of these substages.
Line 73 – IODP – Define abbreviation. And SST.
Line 79 - during MIS 5e - provide age in ka.
Line 101 - Integrated Ocean Drilling Program (IODP)- Say this on first use.
Line 106 - LOVECLIM Earth System Model – Need a reference here.
Line 111 - δ18Ob – Superscript "18". Is "b" right? "
Figure 1 - Text in inset North Atlantic map needs to have the same font as the rest of the figure. Text needs to be larger.
Line 154 - A total of 62 levels were sampled for pollen analysis, - A total of 62 samples were taken for pollen analysis.
Line 171 - tropical forest (TF) - Not sure this abbreviation is necessary. Line 174 - tropical forest (TF) – If you are going to abbreviate you only need to define this once.
.Line 189 - Fig. 4 - Call out figures in numerical order.
Line 196 - over the India's CMZ (17°-27.5° N, 67.5°-90° E) - over the Indian CMZ. Also mark this on the map figure.
Line 231 - abundance tropical deciduous forest – abundance of tropical deciduous forest.
Line 246 - PZ MIS 11a-1, - You use too many abbreviations. I already forgot what PZ means.
Line 250 - substantial less humid condition - substantially less humid conditions.
Line 258 - decrease of 22.3% - decrease to 22.3%?.
Line 296 - long records – long duration records.
Line 386 – NHSI - This is a confusing abbreviation. If you told us what it was earlier in the manuscript, then I've already forgotten. I suggest you don't use this.
Line 395 - gradually declines - gradually declined.
Line 483 – soutprashern - Spelling.
Line 498 - still highly discussed - strongly debated? Controversial?
Line 505 - carbon dioxide jumps (CDJ) - this is entirely inappropriate. You have far too many abbreviations. The manuscript is extremely hard to read anyway without you adding this. Nobody understands these abbreviations when there are so many.
Line 503 - DO-like - More confusion.
Line 526 - Ice-Rafted Detritus (IRD) - Please stop.
Line 548 - IN the CH_4 record - IN?

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Authors' Response to Reviewer #2:

Oliviera and co-authors present interesting new data from an important interglacial period that may be analogous to the Holocene. I believe the data is of good quality and the topic is interesting. But I found the writing and figures of the paper to be overly complicated and difficult to follow. I would suggest a major simplification and focusing on key takeaways in both the text and figures so that readers with a wide range of interests can learn something from the paper. As it is currently written, one needs to be an expert in the Indian summer monsoon and MIS 11 to be able to understand the presentation. But in order to reach the goal of demonstrating how this era could be relevant to the Holocene and future warming, the presentation needs to be simplified. I will provide some examples and ideas of how to clarify, mainly focused on figures and nearby text, but please work on the paper as a whole to simplify the message and improve readability.

Overall, I would suggest a focused rewrite and figure updates that highlight key takeaways, but at the same time acknowledges that it is complicated. It's a big win to bring together all of these different data sets, complexity is more exciting to me than a simple answer.

We appreciate the reviewer's constructive comments and positive assessment of our study. We acknowledge that certain parts of the manuscript were overly detailed, which could make it challenging for a broader audience to follow. In response, we have undertaken a focused revision to enhance readability and highlight the key takeaways. We simplified, reorganized and shortened the text, particularly in the Abstract and Section 4: Results and Discussion. To maintain focus on the key aspects of our research, we also removed discussions beyond the scope of the study, such as those related to the global terrestrial biosphere. As the reviewer highlighted, integrating multiple datasets to analyze these interactions is a key strength of this study, and we have ensured that our findings are effectively communicated to the broader readership of Climate of the Past.

We also revised the figures as requested:

Relating to figure 1: The maps as currently presented are confusing. I would recommend breaking this into two separate panels rather than having the Atlantic sites inset. Next, I would add another panel with a figure or table summarizing the preceding text which has a lot of information about rainfall amounts and vegetation type that would be better presented graphically. We modified Figure 1 as suggested, including adjustments to the layout, vegetation information, and the separation of the Atlantic and Indian regions into two panels.

Relating to figure 2: I know that it is valuable to show the whole pollen diagram, but I would love some additional context and explanation for which taxa are important for the green bands. I see some patterns but its tough to get a cohesive understanding. We acknowledged the need for a clearer representation of the pollen diagram. As recommended, we revised it for improved readability and interpretation by incorporating a color code and enhancing the graphical visualization of very low-abundance pollen taxa. This was moved to supplementary information (Fig. S2), allowing Section 4, Results and Discussion, to concentrate more clearly on the most relevant findings.

Figure 3. The combination of the arrows, the dashed lines, and the shaded bars is a lot to take in. I have a tough time knowing what I am supposed to be looking at in this figure. We simplified the visual elements to enhance readability.

Figure 4. It's a bit tough for me to understand why the highlighted sections are important, but other similar large magnitude changes in the pollen are not interpreted. We understand the reviewer's concern and we have clarified the criteria used to identify forest expansion and contraction events, represented by the green and brown bands, respectively, in the figure caption (lines 270-275 in the revised manuscript).

Figure 5. The data should be in front of the bars on the graph. As it is currently, the bars obscure the data. To my eye, it's not totally clear that NHSI is the main driver of the dynamics as it seems like the text is saying. The patterns look like they share features of lots of the potential drivers plotted in this figure. We adjusted the figure layout to ensure that the data is displayed in front of the bars for better visibility and to further clarify the contributions of the main forcings to the observed variability in the text (lines 434-446).