Responses to referees on CP submitted article:

Impact of terrestrial biosphere on the atmospheric CO_2 concentration across Termination V

G. Hes et al (2021)

We are very grateful to all three reviewers for their constructive remarks and comments allowing both a better analysis of our results and a clearer communication of them. We address (in blue) the suggestions of change made by the three referees on a one to one basis.

RC1: Comment on cp-2021-143, Suzanne Leroy, 09 Dec 2021

Thank you for both the general comments and your detailed review of the manuscript.

General comments

By focusing on interglacial forests during TV, the authors are looking for explaining reasons for low CO_2 before the MBE. The combination of three steps (new data, data compilation and modelling) makes for an interesting manuscript, deserving publication.

The grain size of pollen is nor clay but silt and fine sand (line 94).

Yes, this has been corrected in the manuscript.

Lines 94-96: explain briefly why it reflects more the Guadalquivir? Is it a question of current? The paper you mention is not available yet at the time of this review.

Thank you for raising this point. Based on more recent interglacial studies (Moal-Darrigade et al., 2022b) we know that both the Guadiana and the Guadalquivir rivers contribute to fine particle - among which are pollen - transport to Site U1386. Another published paper (Moal-Darrigade et al., 2022a) focuses on TV but does not specify the relative importance of Guadalquivir and Guadiana rivers on the origin of the sediments. The full analysis for TV is not yet published so we prefer to modify the manuscript, stating that the pollen originates from both watersheds. Yet, whether the pollen comes from the Guadalquivir, the Guadiana or both does not make a real difference because both host the same meso- and thermo-mediterranean bioclimatic stages and both reflect the southwestern Iberian vegetation.

“The core section of interest for TV (Unit IA) is embedded in a unique contourite structure shaped by the historical variations of the Mediterranean Outflow Water (MOW) originating from the Strait of Gibraltar. The local lithology is composed of nannofossil muds, calcareous silty muds and silty bioclastic sands with a dominance of silty muds (Stow et al., 2013; Moal-

Darrigade et al., 2022a) and is overall homogeneous across TV. The size of pollen grains (10-100 μm) belongs to the silt fraction and a recent study has shown that fine sediment fraction, including pollen, found on site U1386 essentially originates from the Guadalquivir and the Guadiana rivers (Alonso et al., 2016; Moal-Darrigade et al., 2022b). Moreover, modern pollen samples from the Southern Iberian margin deep-sea floor have proven to accurately represent the vegetation of the adjacent continent (Naughton et al., 2007; Morales-Molino et al., 2020). Therefore, the pollen record from site U1386 reflects the regional vegetation evolution of southwestern Iberia. This vegetation presently belongs to the thermo-mediterranean and meso-mediterranean bioclimatic stages across the Guadalquivir and the Guadiana catchments (RIVAS-MARTINEZ, 1987)."

Despite the justification proposed, the sum of 100 terrestrial pollen grains remains very low, and I cannot see how your % are stable with such a low sum. I would recommend that you increase your counts and reach a total terrestrial sum of 300, and a sum without *Pinus* of 150-200. Please provide clearly the sums used for terrestrial pollen for each sample, and also the sums without *Pinus*. So the reader can estimate himself/herself the quality of your data.

The text has been modified lines 133-141 in order to prove, with sedimentary evidence, that 100 pollen grain count is enough to provide a reliable insight on the regional terrestrial vegetation.

"In order to obtain a reliable representation of the sample composition, at least 100 terrestrial pollen among which 20 different morphotypes excluding *Pinus*, aquatics and spores were counted. Rull (1987) shows that a pollen sum of 200 grains, including *Pinus*, is sufficient to produce reliable estimates. Indeed, for higher values there are no significant variations in the confidence interval width. In the specific case of Site U1386, we observe both strong pollen concentration and MOW (reflected by a high Zr/Al ratio and high sortable silt values) at the end of TV but low pollen concentration and high MOW during MIS11, suggesting that pollen concentration is relatively independent from MOW strength (Fig. S2). Additionally, the pollen percentages derived from a 100 grain count yield a reliable picture of the considered sample, within the 0.95 confidence limits (Maher Jr, 1981)."

The data is in the process of being published on Pangea server. The sums for each sample will therefore be accessible very soon.

You should anyway delete the sentence "the more pollen counted, the better the concentration estimates are" as you do not seem to follow this recommendation.

The sentence has been deleted.

Is the "200 grains" of Rull without *Pinus*?

It includes *Pinus.*

Although this manuscript is on TV, adding a few more samples at the top of the diagram until the start of the following glacial (thus covering all types of forests), would provide you with the end of the interglacial and give you a more complete and satisfying view of MIS11 forests. This is only a suggestion.

It definitely would be interesting to analyze more samples towards the end of MIS11. However, the experimental part of the current research project focuses on the TV time interval, lasting from 433 kyr to 404 kyr BP, and is now well defined in the revised version of the manuscript.

Table 1: “fern spores” are unexpected proxy for forest. Please explain the rationale. ODP 658 is in front of the Sahara that was already developed in TV.

For site GIK16415-2, Dupont and Agwu (1992) explain that the ferns develop in forested environments and therefore constitute a reliable proxy for forest expansion. This explanation was added to the manuscript: “As each pollen record featured different pollen assemblages according to their location, we extracted (or defined and computed when it was lacking) the arboreal pollen percentage from each pollen assemblage (“Forest proxy” column, Table 2) in order to build the most representative metric for forest evolution globally. In the specific case of site GIK16415-2 record, we use fern spores as forest proxy for their percentage accurately reflects the forest development (Dupont and Agwu, 1992).”

Section 3.1: is there a change in lithology at the transition between pollen zones 1 and 2? Could this be the reason for the high amount of oxidation-resistant *Taraxacum*?

Yes, there is a slight change in the lithology (see sedimentary stratigraphy on Figure 2). However, there is no evidence for a correlation between the change in lithology and the high amount of oxidation-resistant *Taraxacum*. We observe a high amount of concentration of *Taraxacum* irrespective of their corrosion state at site U1386 but also at site U1385.

The concomitant *Taraxacum* increase may witness an increase in moisture availability compared to the previous zone U1385-1, dominated by semi-desert plants, but still zone U1385-2 evidences dry environments. In modern pollen assemblages, the highest *Taraxacum* percentages are found in dry environments of Greece and Morocco, where mean annual temperatures span from 12 and 17°C and precipitation from 0 to 250mm (Leroy, 1997).

Figure 2: add a lithological log to the left of the diagram. Line 93 suggests that it has changed over time over the whole core. What is the case for the portion of the core you studied?

A lithological log was added to Figure 2. The portion of core we studied is overall made of homogeneous clay. There is very little influence of the MOW because we don’t have any hiatus and only one segment with heterogeneous elements. The latter features a transition from clay to clayey silt synchronous with the change in pollen concentration.

Line 351: “Forests are reliable proxies for terrestrial biosphere”. Please develop here. How much is this group better than swamps or other groups? provide figures/numbers if possible.

We have not found any study comparing swamps to other forests. At any rate, our pollen data doesn’t exclude arboreal pollen from swamp forests in the tropical region.

The statement was complemented by other references:

“Altogether, forest ecosystems represent the largest biomass reservoir during the preindustrial holocene period (Prentice et al., 2011) and the Last Glacial Maximum (Crowley, 1995; Prentice et al., 2011). Additionally, the carbon uptake by modern forests accounts for the large majority of the terrestrial carbon sink (Pan et al., 2011). In this respect, forests are reliable proxies for the terrestrial biosphere.”

The text needs to be improved as it feels at times like a literal translation of French. It also contains with quite a few typos. Some of them were corrected in the annotated manuscript.

The typos and improper language formulations were corrected as much as possible.

Throughout the manuscript I would recommend being more restrictive with the use of the word “event”, and keeping it only for very brief changes. In many instances, it could be replaced by, for example, “period”.

The manuscript was modified in consequence.

Points of details

Pollen/cm^3: add superscript (eg lines 230, 271).

In section 2 besides fig.1, in most cases, it is not necessary to call to figures. This will facilitate figures calling in a logical order, later on.

Fig. 5a and 5b should be reversed. First the map, then the records.

Line 301: explain briefly here what SINES is (i.e. a forest phase …).

In the text: the family names of plants should not be in italics, e.g. Cupressaceae, Poaceae.

Moreover check their spelling.

All the abovementioned points of detail have been addressed in the new manuscript.

Add the position of TV on figures 2, 3, and S2.

Yes, the TV period has been added on figures 3 and S2 and the uncertainties were also added on each pollen percentage Figure 3. However, we prefer not to refer to any temporal framework when discussing Figure 2 as it is a pure description of the pollen record with no assumption on the age model.

The reference format for the main text and the SI needs to be homogenised and adapted to the journal requirements.

The references have been reformated.

See the annotated manuscript for further comments.

Response to the comments made directly on the manuscript.

Line 160: For U1386, the mean resolution is 700 kyr but the finest resolution is 500 kyr.

RC2: Comment on cp-2021-143, Anonymous Referee #2

In this manuscript Hes et al. studied the pollen as a proxy for forest development and paleoclimate from the 433-405 kyr time span from a deep-sea marine sediment core from the Gulf of Cadiz (IODP Site U1386). This study then deals with a very interesting period in Earth history during the mid-Bruhnes transition: Termination V - the transition from Marine Iso-tope Stage MIS12 to MIS11, which was characterized by the largest de-glaciation of the late Pleistocene, and by one of the warmest and longest interglacials of the Pleistocene, MIS11.

Another particular feature of MIS 11 is that an early CO_2 peak, usually associated to the deglaciation in response to increasing temperatures, fails to be detected. Therefore, all these questions make this study of great interest to researchers working on paleoclimate and modelers forecasting future climate scenarios.

From all this, I think this is an interesting study but I have major concerns that should be dealt before publication:

1. My main concern is the overinterpretation of the data. This study is too ambitious and the conclusions obtained about the CO_2 mitigation by high-latitude forests are barely justified by the scarce data available for this time-span. In particular, only two records including the one studied here are available for the Mediterranean forest group and the resolution (and thus accuracy) of the pollen analyses are not that great either (700-yr in this study and even lower in site U1385). The statement about that Mediterranean forest development wasn't thus important in the CO_2 mitigation is in my opinion very risky. More discussion about this should be added and/or the language should be toned down.

In the revised manuscript, more discussion was added concerning the Mediterranean forest and the temperate and boreal forest, especially concerning the timing of the forest peak and the forest area represented by the respective composites (see Section 4.2. The potential role of boreal and temperate forests in atmospheric CO_2 mitigation during TV). Additionally, the text was toned down in the Discussion and Conclusion sections, and now leaves room for the contribution of Northern Hemisphere warm temperate forests to atmospheric CO2 pumping, although with a shifted timing. Below are some of the modified paragraphs:

Line 27: "Finally, the direct comparison of global simulated forests (iLOVECLIM model) to our pollen database reveals overall consistent temperate and boreal forest evolutions despite model biases, thereby supporting the hypothesis of a significant CO_2 sequestration by middle and high latitude forests of the northern hemisphere shortly after the onset of TV."

Line 550: "Therefore, we propose that the temperate and boreal forests of the Northern Hemisphere are the major drivers of the high terrestrial biosphere productivity signal and the moderate atmospheric CO_2 values at the beginning of MIS11, while the contribution of the warm temperate forests of the Northern Hemisphere is weaker and arrives later (from 420 kyr BP)."

Line 579: "While the total carbon stock increases by ~900 GtC until 410 kyr BP (Fig. 9c), Fig. 9b shows a redistribution of the carbon sinks mainly from the Tropical forests (~54% of total carbon stock at 436 kyr BP to ~45% at 404 kyr BP) to the high-latitude boreal and temperate forest ([40:90] °N) over the 436-420 kyr BP period (~20% of total carbon stock at 436 kyr BP to ~28% at 420 kyr BP) and to the larger area above 15°N over the 415-405 kyr BP period, indicating a northward increase in carbon fluxes. Therefore, we suggest that the high-latitude boreal and temperate forests account for the overall biosphere productivity increase leading to atmospheric CO_2 sequestration between 425 and 415 kyr BP. The warm temperate forests and sclerophyll woodlands, which include the Mediterranean forest, more significantly contribute to the biosphere productivity over the 405-415 kyr period. The specific role of tropical forests remains unclear: the decrease in tropical forest percentages - mainly driven by GIK16415-2, Lake Malawi and Lake Magadi records - near 430 kyr BP leading to a local minimum around 428 kyr BP (Fig. 5) suggests that shrinking tropical forests could have temporarily contributed to the increase in CO_2 at 430 kyr BP but we do not have any confirmation in the carbon stock analysis (Fig. 9)."

Line 618: "Based on the presented global pollen-based vegetation records and simulation analysis we suggest the following scenario:

1) The strong warming at the onset of MIS11 results both in a strong increase of high latitude terrestrial biosphere productivity and in a rise in atmospheric CO_2 concentration probably driven by ocean physical and chemical degassing processes.
2) Because of the exceptionally high ice sheet melt characterizing MIS11 (Dutton et al., 2015), forests can develop northwards. This strong expansion, mainly observed in the temperate and boreal forests, would allow carbon removal by terrestrial biosphere to partially compensate the oceanic carbon losses, resulting in a long and unique CO_2 plateau from ~425 to ~415 kyr BP. Although the warm temperate forests of the northern

hemisphere subtropical regions also respond to this warming, they might only play a significant role on the carbon cycle change around 410 kyr BP.

While there are yet too few data to depict the full impact of vegetation on the carbon cycle, we expect the aforementioned conclusions to be regarded as a first benchmark case for further assessment of global terrestrial biosphere during TV."

2. Regarding the site groupings done for the different pollen sites covering the selected time span, it looks to me that it was done with a very loose criterium. For example, in the "Euroasian" group, records from all over this area were chosen including records characterized by very distinct climate requirements such as Chinese records very conditioned by monsoon or other climate patterns.

Also, the taxa used to represent the pollen record are very random; sometimes using single species and other times using group of taxa.

In this respect, why the two Mediterranean sites were separated from the Eurasian group? Aren't they also located in Euroasia?

In the revised manuscript (which includes 4 new records, thanks to the comments by referee #3's), we explain more clearly in the text and thanks to the biome map (Figure 5a) how we classify the records in the different bioclimatic groups. The four composites were therefore renamed in accordance with the represented groups. The Eurosiberian boreoamerican group (ES-BA) is based on Braun-Blanquet (1930)' mega biome distribution. Therefore, it includes records at various latitudes and altitudes as long as the observed forests are dominated by temperate or boreal taxa. Southern to the ES-BA group, the Northern hemisphere subtropical group (Subtropical-N) now includes records mainly representing warm temperate forests (such as the Mediterranean forest, but not only).

Section 2.2.2, Line 206: "Pollen records are classified in four forest groups defined according to their main bioclimatic specificity. The Eurosiberian Boreoamerican group (ES-BA), as defined by Braun-Blanquet (1930), covers boreal and temperate forests. Therefore, both Tibetan Plateau pollen records (Heqing and ZB13-C2) are counted in the ES-BA group, the high altitude compensating for their relatively low latitudes. Although Tenaghi Philippon and Lake Ohrid records are on the geographical boundary with the Mediterranean region, they are also classified in the ES-BA group for they rather reflect a temperate forest cover. The Northern Hemisphere Subtropical group (Subtropical-N) encompasses warm temperate forests of the Northern Hemisphere and therefore extends beyond the geographical boundaries of the Mediterranean region (Mediterranean *sensu lato*). The Tropical group (T) refers to tropical and afromontane forests. Finally, the Southern Hemisphere Subtropical group (Subtropical-S) only includes the MD96-2048 record which is at the limit between tropical and subtropical forests (Table 2)."

Moreover, we added an explanation on the choice of forest proxy lines 180-184. "As each pollen record featured different pollen assemblages according to their location, we extracted (or defined and computed when it was lacking) the arboreal pollen percentage from each pollen assemblage ("Forest proxy" column, Table 2) in order to build the most representative metric for forest evolution globally. In the specific case of site GIK16415-2 record, we use fern spores as forest proxy for their percentage accurately reflects the forest development (Dupont and Agwu, 1992)."

Tropical and South African records show very ambiguous signals during the deglaciation with maxima around 430 kyr (see Fig. 5) – could they also be involved in the CO_2 mitigation during TV?

Some discussion on this point was added to the manuscript line 581:

"The specific role of tropical forests remains unclear: the decrease in tropical forest percentages - mainly driven by GIK16415-2, Lake Malawi and Lake Magadi records - near 430 kyr BP leading to a local minimum around 428 kyr BP (Fig. 5) suggests that shrinking tropical forests could have temporarily contributed to the increase in CO_2 at 430 kyr BP but we do not have any confirmation in the carbon stock analysis (Fig. 9)."

Not much can be said regarding the Southern Hemisphere Subtropical composite (Subtropicla-S) because it is composed of only one record (MD96-2048, South Africa) with very low resolution. More data would be required to confirm or reject our hypotheses.

3. The authors focus on Termination V (TV, see for example manuscript title), however in reality the authors not only focus on that deglaciation that should have occurred relatively fast - I read in other papers between 427-424 kyr; see for example Hodell et al., 2003 - but also on MIS11 (see Fig. 3 also picturing MIS11e, d and c) and fail to give a clear picture of the time period that they are really focusing on in this study. Therefore, a clear definition of the TV period should be given and should also be shown in the different figures.

The revised manuscript now gives a clear definition of the benthic foraminifera oxygen isotope-based termination V, line 44:

"Termination V (TV, ~[404-433] kyr BP), the fifth last deglaciation, is defined as the period between the highest and the lowest values of the benthic foraminifera oxygen isotope ratio ($\delta^{18}O_b$) following seminal work by Broecker and van Donk (1970) and Broecker (1984). TV begins during glacial Marine Isotopic Stage (MIS) 12 and extends to MIS11, thus covering the longest deglaciation period (~29 kyr) in the last 650 kyr (Sarnthein and Tiedemann, 1990) while orbital precession is at the lowest."

Please refer to the answer made to referee #3 on the Study interval as a complement.

4. Sometimes it is confusing where the authors are placing the beginning of MIS11 interglacial in their pollen record and should be clarified. In Fig. 3 it is placed when the "ubiquist" group enhanced but in Fig. 2 it is placed when the Mediterranean taxa increased (pollen zone 2b). Please be consistent.

By the way, what is SINES? The definition should be added to the text and figures.

Indeed, there was a mistake in the text line 242 and figure 2. We have modified the text and figures to better underline the difference between the regional terrestrial interglacial (SINES forest phase) defined by pollen-based evidence from the MIS11 (the global marine interglacial) defined by $\delta^{18}O_b$. The onset of MIS11 is synchronous with the increase in Ubiquist and the onset of SINES (the regional southern Iberian interglacial) is defined by the 20% threshold in the Mediterranean forest.

5. It is interesting to see that the "warm foraminifera" are also delayed with respect to the isotopic data. Does this also mean that the marine environment in this area didn't react to this climate change that fast either?

Yes, we do observe a delay in the "warm foraminifera" response, for which the increase arrives after the increase in Mediterranean forest and much later than the beginning of ice volume decrease. It reflects a decoupling between the global ice volume change and the regional climate change indicators (similarly to the Mediterranean forest decoupling on land: SINES terrestrial interglacial begins 4 kyrs after the globally-defined MIS11). A decoupling between warm foraminifera and terrestrial vegetation is also observed at the beginning of glacial periods (Sánchez Goñi et al., 2013).

6. Where is the dashed black line in fig. 8?

The dashed line was added to Figure 8.

7. The resolution is too low to resolve submillennial-scale changes (see conclusions).

The conclusions were changed.

8. The pollen records studied here suggest a moderate warming during MIS11, disagreeing with the statement in the conclusions "The strong warming at the onset of MIS11…" Please add more discussion about this.

The significant development of temperate and boreal forests suggests a strong warming across the termination. We add some discussion on this point line 554:
"The forest growth is particularly strong for the North American (N) and the Siberian regions (+40 to +50 % over TV), supporting the argument for a massive boreal and temperate forest development. Such a significant forest development suggests a strong warming across TV and during MIS11 consistent with the reconstructed high temperatures, reason for which Melles et al. (2012) refer to this period as a "superinterglacial"."

RC3: Comment on cp-2021-143, Anonymous Referee #3

Hes et al. present new palynological data from the Iberian Margin (IODP Site U1386) spanning Termination V (TV; i.e., MIS 12/11 transition) and evaluate the effect of terrestrial biosphere on the atmospheric CO2 concentrations using model simulations in which they include other available, globally distributed pollen records from this interval. This is an important dataset that fills in a critical gap in the palynological records off Iberia, and hence it merits publication. On the other hand, however, there are important methodological caveats related to set up of the study that preclude publication of the manuscript in its current form. As it currently stands, I find the interpretation of the results premature, and hence, I refrain from commenting on it before the points below are addressed by the authors in a revised version.

Study interval

The focus of the manuscript is on TV, which according to the authors it spans the period between 433 and 404 kyr BP (line 39). This is wrong (e.g., see the duration of glacial terminations in the Chinese speleothem record by Cheng et al., 2016). What the authors consider as TV actually spans almost the entire MIS 11c interglacial. If the authors decide to consider the entire period 433-404 kyrs, then they should also include several other palynological records that span MIS 11c, and re-run the model simulations.

We have clarified the definition of Termination V that we use. We follow the definition of Terminations given by Broecker and Van Donk (1970) and Broecker (1985) considering that the Termination is the time for deglaciation, which is determined by the period separating highest benthic foraminifera $\delta^{18}O$ from lowest values (see lines 42-47). Indeed, with this definition, TV covers the 404-433 kyrs period and therefore extends over part of MIS11 interglacial.

Cheng et al. (2016) do find a much shorter duration for TV in the Chinese speleothem record. However, the $\delta^{18}O$ form speleothems is not a direct measure of ice sheet mass but a proxy based on the isotope fractionation process, which can be modified by the Asian Monsoon. Therefore, it is more a regional signal than the benthic foraminifera $\delta^{18}O$ signal that we use to define TV at a global scale. Candy et al. (2014) reminds the following: "Recent studies have attempted to define the onset, duration, termination and the prediction of the duration of interglacials in marine and ice core (Cheng et al., 2018) sequences (Tzedakis et al., 2012a,b) but currently, no universally applicable definition is available."
This clearly stipulates that there is no strict definition for the Termination V interval. Therefore, given that our definition of Termination V is clearly established and that the time frame of the study is consistent with this definition, we argue that the study interval we refer to as Termination V is valid.

Candy et al. (2013) further notes about Termination V: "The long period of deglaciation, represented by Termination V, means that sea level rise during MIS 11 was slow and that the high sea level in MIS 11 does not occur until the middle of this warm stage (Rohling et al., 2010). As highlighted earlier, this means that the interglacial peak of MIS 11, and therefore the peak in sea level, does not occur until 30,000 yr after the end of MIS 12, in comparison with the approximate 12,000yr that elapsed between the end glacial and interglacial peaks of MIS 9, 7 and 5." Multiple other studies find very long periods for Termination V: 29 kyr for Sarnthein and Tiedemann (1990) and 21,6 kyrs (from 427,2 to 405,6 kyr BP) for Parrenin and Paillard (2012).

We believe that there is no need to re-run the simulation since the study period is now clarified and proven consistent with previous studies.

Please also note that there is at least one more model study that focuses on terrestrial biome simulations for MIS 11–12 (Kleinen et al. 2011), which the authors should consider and elaborate on in a revised manuscript.
Thank you for your helpful suggestion. We haven't found any study corresponding to Kleinen et al. (2011) focusing on MI11-12. We believe that the comment was referring to Kleinen et al. (2014) which is relevant in the context of our study. Our model results have been discussed in reference to this paper line 613:

"Finally, the Tropical forest, the mid-latitude temperate forest and the high-latitude boreal and temperate forest are marked by a decreasing carbon stock from ~408 kyr BP resulting in a total carbon loss of 200 GtC in 404 kyr BP (Fig. 9b). This carbon stock decline is both consistent with the shrinking simulated forest in the North American (N) and Siberian regions (Fig. 7) and with the forest simulations by Kleinen et al. (2014)."

Additionally, we have included and discussed other modeling studies:

Line 77: "Several modeling studies already address the question of climate-carbon-vegetation feedbacks over MIS11 using either time slice (Kleinen et al., 2014; Rachmayani et al., 2016) or transient experiments (Kleinen et al., 2016; Ganopolski and Brovkin, 2017). However, none of them fully cover TV leaving room for a global model assessment of climate-vegetation interactions during the whole deglaciation period."

Line 586: "To further assess the carbon sink potential of temperate and boreal forests we analyze the variation of the simulated total carbon stock (i.e carbon fluxes between the atmosphere and the {vegetation+soil} system), both a proxy for terrestrial biosphere productivity and a direct measure of atmospheric CO_2 removal (Fig. 9). Overall, the simulated total carbon stock (Fig. 9b) increases across TV, peaking around 410 kyr consistently with the average pollen-based forest maximum (grey line, Fig. 8) and in line with the simulated vegetation carbon maximum by Kleinen et al. (2016)."

Pollen records database

In contrast to the authors' argument (line 174), there exist many more pollen records that span TV and aren't included in this work. For instance, Ioannina (Tzedakis 1994), Heqing

(Xiao et al. 2010, An et al. 2011), Lake Baikal (Prokopenko et al. 2010), Lake Biwa (Tarasov et al. 2011), Praclaux (de Beaulieu et al. 2001) and Valles Caldera (Fawcett et al. 2011) among others. In addition, higher temporal resolution data exist for some of the pollen records included in this manuscript (e.g. Lake Ohrid – Kousis et al. 2018, Koutsodendris et al. 2019, and Tenaghi Philippon – Ardenghi et al. 2019); these should be also included in the analysis.

Thank you for providing these interesting references for other pollen records. Despite being very relevant for our study, some of these records are not made available for the scientific community on online platforms and some authors refused to share their data or didn't respond to our demands (Cheng et al. (2018):no answer, Praclaux (de Beaulieu and Reille, 1992): negative answer, Lake Baikal (Prokopenko et al., 2002): no answer, Lake Kokotel (Shichi et al., 2009): no answer, and Lake Biwa (Tarasov et al., 2011): no answer, Lake Van (Litt et al., 2014): no answer).

Nonetheless, we have modified the list of works included in the database by adding 4 pollen records that were accessible namely Ioannina (Tzedakis, 1993), Heqing (Xiao et al., 2010), Lake Malawi (Ivory et al., 2016) and Valles Caldera (Fawcett et al., 2011).

We have modified Section 2.2.1 to clarify the way we choose the pollen records:
"We conducted an extensive literature review to compile the available marine and lacustrine pollen records covering TV. Only records spanning the 423-405 kyr period at least were selected. At a global scale, a total of 18 pollen records (including the present U1386 record) with variable resolution were made available by the scientific community (Pangea database, European pollen database, Neotoma database). However, we note the existence of a few more pollen records covering TV that are not included in this work due to a lack of data accessibility (de Beaulieu and Reille, 1992; Prokopenko et al., 2002; Tarasov et al., 2011; Cheng et al., 2018; Litt et al., 2014) or the use of different metric, pollen concentration instead of pollen percentages (deVernal and Hillaire-Marcel, 2008). As each pollen record featured different pollen assemblages according to their location, we extracted (or defined and computed when it was lacking) the arboreal pollen percentage from each pollen assemblage ("Forest proxy" column, Table 2) in order to build the most representative metric for forest evolution globally. In the specific case of site GIK16415-2 record, we use fern spores as forest proxy for their percentage accurately reflects the forest development (Dupont and Agwu, 1992)."

Moreover, the study by Dupont et al. (2011) included inTable 1 doesn't span MIS11/12; do the authors refer to Dupont et al. (2019)?
Yes, we refer to Dupont et al. (2019), it has been changed in the revised manuscript.

Lithology and pollen concentration
The authors explain that the study samples are taken from a core interval that comprises a 'unique contourite' affected by the Mediterranean Outflow Water (MOW) (lines 91-92). The question that arises is to what extent the changes in the MOW strength influence the transport and deposition of pollen grains at the study location. To convincingly show that the palynological results aren't affected by ocean dynamics, the pollen sums and concentrations should be directly compared with the grain size and XRF data from Site U1386 that record the MOW variability. Only then it will be possible to conclude that even pollen sums of 100 grains can provide reliable insights on terrestrial vegetation at the study site.

To respond to this interrogation, the lithology was added to Figure 2 and the Zr/Al ratio (a proxy for sedimentation rate) and sea level were added to supplementary figure S2. The text lines 133-141 were modified to support the 100 pollen count methodology.
"In order to obtain a reliable representation of the sample composition, at least 100 terrestrial pollen among which 20 different morphotypes excluding *Pinus*, aquatics and spores were counted. Rull (1987) shows that a pollen sum of 200 grains, including *Pinus*, is sufficient to produce reliable estimates. Indeed, for higher values there are no significant variations in the confidence interval width. In the specific case of Site U1386, we observe both strong pollen concentration and MOW (reflected by a high Zr/Al ratio and high sortable silt values) at the end of TV but low pollen concentration and high MOW during MIS11, suggesting that pollen concentration is relatively independent from MOW strength (Fig. S2).".

Pollen groups
Please explain in a more concise way which pollen taxa are included in each group (lines 140-143 are very confusing). Is it correct that *Betula* is included in two groups, i.e., Mediterranean and Pioneer forests? Please explain.
It could be argued that *Betula* fall in the Mediterranean forest group but it best characterizes the Pioneer group. *Betula* has been removed from the MF sums (cf Turner and Hannon (1988)).

Also, shouldn't *Populus* and *Salix* be included in the Pioneer Forest group?
No, , *Populus* and *Salix* are not considered as Pioneer trees/shrubs (Turner and Hannon,1988). They were included in the Mediterranean forest group.

Which taxa are included in the Ubiquist group?
In order to clarify this whole section (lines 139-149), the text was simplified to only describe the climate corresponding to the different pollen groups while the specific taxa for each group are displayed in a new Table 1.

Other comments:
Please carefully check and correct the spelling of the pollen taxa names (several taxa e.g., *Helianthemum, Hippophaë,* and Cupressaceae are misspelled many times in the text).

Thank you, this has been modified.

Consider the latest, higher resolution CO2 data from the EDC ice cores for the interval 330-450 kyr BP (Nehrbass-Ahles et al. 2020).
Thank you for this suggestion. Figure 7 and 8 and the corresponding text have been updated with the new CO2 record by Nehrbass-Ahles et al. (2020).

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