

Response to interactive comments by three anonymous referees and the editor on “The MIS 11 – MIS 1 analogy, southern European vegetation, atmospheric methane and the ‘early anthropogenic hypothesis’” by P.C. Tzedakis

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I am grateful to the three referees and the editor for their comments and the opportunity to clarify some important issues. Below, I provide a point-by-point response.

Anonymous Referee #1

The manuscript by Tzedakis compares data from MIS 1 to data from MIS11 and MIS 19. MIS 11 is often referred to as an analogue for the present Holocene. The manuscript elaborates on two questions: 1) How do MIS 1 and MIS 11 data have to be aligned so that the records show the reaction to the same forcing. The major question here is how long the present interglacial would last without human influence. Alignment by orbital frequencies offers two very different answers to this question. With precessional alignment the Holocene would be about to end now while it continues for another few millennia with orbital alignment. 2) Is the Holocene methane record already significantly influenced by human activities 5 kyr before now?

The analysis by Tzedakis is based on the assumption that there is a strong correlation between the global methane concentration and the abundance of trees in Europe. This seems indeed to be the case for the last 800 kyr, the period covered by ice core records. However, this is not really a surprise since glacial interglacial changes leave their imprint on pretty much any paleo record. Also that there is a millennial scale change in the tree pollen records is not really surprising. It has been shown that so called Dansgaard-Oeschger events, millennial temperature changes in Greenland, represent large scale changes in the ocean and atmosphere from the northern polar region to the tropics and beyond. In that respect I would expect, and that has been demonstrated in Tzedakis et al., 2009, a correlation between the abundance of vegetation (here tree pollen) in the northern hemisphere and the northern hemispheric (Greenland) temperature record (represented by the methane record in Tzedakis et al., 2009). However, the correlation that is seen on glacial-interglacial levels and on the big, at least hemispheric, changes over Dansgaard-Oeschger events do not prove that such a correlation is also given for the relatively small changes observed over an interglacial period. Looking at Figure 3 I really have difficulties seeing the correlation between methane and temperate tree pollen, and methane and Ericaceae. There are some trends which can be found between some records. However, I would like to see a correlation plot. Even excluding the Holocene period I am convinced that there is no significant correlation between Ericaceae and methane and not a very good one for tree pollen and methane.

While the global/hemispheric nature of changes on orbital and millennial timescales means that ultimately “almost everything is correlated to everything else”, there is a need to elucidate specific mechanisms and processes linking different parts of the Earth System. Tzedakis et al. (2009) showed a strong coherence between changes in southern European tree populations and atmospheric methane concentrations over the last 800 thousand years. They argued that variations in the continental hydrological balance via shifts in the mean latitudinal position of the ITCZ provide a link for the observed correspondence, leading to concomitant changes in southern European vegetation on one hand, and low-latitude wetland extent and methane/VOC emissions, on the other. Wavelet analysis of correlation between methane and tree pollen records from the Portuguese margin and Greece showed strong coherence values in the short eccentricity, obliquity and climatic precession bands, but also at shorter, millennial-scale periodicities, at the 95% significance level.

Referee 1 suggests that changes occurring over an interglacial period would be too small to expect a correlation of methane and southern European vegetation and asks for a correlation plot. In reply, I refer the reviewer to Fig. 4 in Tzedakis et al. (2009), which shows strong coherence values characterizing the interglacial periods (Tzedakis et al., 2009, Fig. 4). Moreover, it is important to point out that while the amplitude of glacial-interglacial variability exceeds that of intra-interglacial variability, there are clear and well-documented changes in the continental hydrological balance of low- and mid-latitudes occurring during the course of an interglacial, which affect both methane concentrations and southern European vegetation.

More specifically, during boreal summer insolation maxima at the onset of interglacials, the maximum northward displacement of the ITCZ leads to an amplification of the hydrological cycle in northern low latitudes and an increase in wetland extent and CH₄/VOC emissions. During the course of an interglacial, the northernmost position of the ITCZ gradually shifts south in response to decreasing summer insolation and Northern Hemisphere cooling. This leads to weakened Indian, East Asian and African summer monsoons and a reduction in northern low-latitude wetland extent and methane concentrations.

With respect to vegetation, it is important to appreciate that the apparent subdued nature of changes in summary tree pollen curves during the course of an interglacial, conceals important shifts in vegetation composition. Palaeoecologists use the term ‘interglacial vegetation succession’ to describe the sequential expansion of different vegetation communities, with certain species tending to appear early and others later during the course of an interglacial. In southern Portugal, pollen diagrams show a pre-temperate (late glacial) phase of open woodland (with juniper, pine, birch, deciduous oak); the onset of the interglacial is characterized by early expansion of mediterranean sclerophylls and deciduous oaks; this is followed by a decrease of mediterranean sclerophylls and an expansion of deciduous trees; the final part of the interglacial is characterized by late successional trees (conifers) and heathland (Ericaceae), and an increase in herbs.

These vegetation changes can be viewed within the context of shifts in the mean latitudinal position of the ITCZ. In the early part of an interglacial, the maximum northward displacement of the ITCZ in summer brings southern Europe well under the influence of the zone of subtropical descent, leading to more extreme summer aridity and accentuated seasonality of precipitation compared to present, and to the expansion of mediterranean and sub-mediterranean vegetation communities. As the northernmost position of the ITCZ gradually shifts south during the course of an interglacial, the seasonal impact of subtropical subsidence in southern Europe is reduced at the expense of mid-latitude influences. This leads to increased annual moisture availability and reduced temperatures and, in turn, to the expansion of late-successional trees and heathlands.

With respect to Ericaceae, no correlation with methane was proposed anywhere in the manuscript. The purpose of showing the Ericaceae (heathland) pollen percentages in Fig. 3 was to emphasize that their expansion is a consistent feature of the later part of the vegetation succession in Portugal, characterizing all interglacials, rather than a clear indicator of Holocene anthropogenic impact argued by van der Knaap and van Leeuwen (1995).

I have a serious problem with the precessional alignment of the records. MIS 11 covers 2 precessional cycles. Is it clear which one to synchronize to once you persuaded yourself that precession is the orbital parameter to tune the records to? I think not. So just for the fun of it I propose an alternative precessional alignment (see figure 1). The consequence is that the Holocene still has a long time to go. Principally I think it is problematic to align the time periods according to orbital parameters. We do not really have a good understanding how glacial interglacial changes are triggered. Therefore the better alternative is probably to align cores according to the local temperature record. Taking into account the uncertainty of time scales we can then discuss what the orbital parameters are doing at times of glacial-interglacial change.

Referee 1 suggests that a precessional alignment of MIS 1 and MIS 11 is undesirable compared to an alignment of local (i.e. Antarctic) temperatures, because the succession of different precession cycles presents too many alternative solutions. To illustrate the futility of such schemes, Referee 1 proposes an alternative precessional synchronization where the MIS 1 and MIS 11 alignment is shifted by one precession cycle so that today corresponds to 418 ka (rather than 398 ka, of the Loutre-Berger (2000, 2003) and Ruddiman (2005a, 2007) schemes, hereafter the 'LBR' scheme). Following the logic of this scheme, the Holocene would then have another ~23 kyr to go. However, Fig. 1 shows that this scheme leads to a significant divergence in the alignment of the eccentricity variations between the two periods. Given that the modulating effect of the 400-kyr eccentricity cycle underpins the search for orbital analogues, such an offset in the eccentricity signals renders the proposed synchronization scheme untenable. Thus precessional synchronization schemes are not unconstrained as implied by Referee 1, but instead are governed by the timing of minima in the amplitude of eccentricity variations,

in multiples of 400 kyr, and require that an alignment of the eccentricity signal should not be significantly violated.

Referee 1 suggests that since we do not have a full understanding of the interaction between astronomical forcing and other climate processes and feedbacks in determining the timing of glacial-interglacial changes, then a better alternative is to align the local temperature records, or in other words, the two terminations. There is certainly merit in this proposal, but the problem is that if we accepted *a priori* that the only viable synchronization of the two intervals is to align Terminations I and V, then there would no longer be any need to test the 'LBR' precessional scheme. However, this paper aims to examine the MIS 1 – MIS 11 analogy, which in turn leads to a review and an assessment of the different alignment schemes that have been proposed in the literature. It therefore adopts an agnostic view between the different schemes and treats them as equally valid propositions that should be tested by independent means, if possible.

Detailed comments:

p. 1346, line 1-9: The Holocene records are from a different source than the rest of the pollen records. Is the land sequence quantitatively representative for the same region as the marine record? Whether or not Charco da Candieira reflects natural changes is a different issue. First the question must be if this site supposedly undergoes similar natural changes as the area which is represented in the ocean cores.

The Holocene pollen record from Charco da Candieira was selected because it represented the most detailed and best-dated sequence from Portugal from that interval. However, Referee 1 is correct to point out the different catchment areas between the land and marine pollen sequences. To address this we can use the Lateglacial/Holocene pollen record from marine core SU81-18 in the Portuguese margin, from the vicinity of marine cores MD01-2443 and MD95-2042. Comparison of the SU81-18 and Charco da Candieira pollen records reveals similar patterns.

p. 1342: EDC time scales as any time scale comes with an uncertainty. EDC3 at termination V has an uncertainty of 4kyr. This needs to be taken into account in the discussion.

This is indeed correct and should be incorporated in the text.

p. 1343, line11-14: I would agree if we would have a full understanding of the orbital forcing mechanisms leading to terminations. Since we don't I suggest synchronizing the terminations and taking into account the time scale uncertainties relative to the absolute time scale and see if we can find similarities in the orbital parameters.

Referee 1 suggests that instead of using an obliquity alignment it would be better to align the two terminations, using the Antarctic temperature record. However, this suffers from the large uncertainty in the EDC chronology for MIS 11 and its divergence from the Dome Fuji timescale as discussed in the text. While it might be argued that such an alignment bypasses the chronological uncertainty, it would not contribute to a proper determination of the forcing factors since the phasing would remain largely uncertain. Thus, instead of aligning Termination I to Termination V (whose age may be revised in future), I argue that an alignment of the obliquity signal appears more appropriate, not only because ice core timescales may evolve compared to astronomical timescales, but also because the choice of proxy may also influence the synchronization. Finally, since the designation of potential analogues for the Holocene has an astronomical basis, I still maintain that the alignment of intervals should rely on astronomical parameters.

p. 1348, line 25- p. 1350, line 7: I would like to see the termination I tree pollen before 14kyr and the termination V data before 425 kyr BP on figures 4 and 5. It looks like the tree pollen increase a couple of thousand years after methane increases. To me that is a clear sign that the tree pollen can not be taken as a proxy for local methane sources. We know that high latitude methane sources are responsible for a significant part of the glacial interglacial increase of the methane concentration. Clearly this is not represented in tree pollen records.

This is not correct, the Lateglacial interstadial tree population expansion starts at the same time. For MIS 11, unfortunately the base of MD01-2443 core stops at ~424 ka.

p 1342: As far as we understand from climate records going back to roughly 100 kyr the methane record can be taken as a good proxy for the northern hemispheric temperature. In fact pretty much everywhere except for Antarctica. In that respect the precessional alignment as of figure 2 shows a global delay of termination I vs. termination V by more than 10 kyr. How does that fit into the idea that MIS11 being an analogue for MIS 1?

Referee 1 is correct to point out that a precessional alignment suggests that MIS 1 is only analogous to the second part of MIS 11c and that the two terminations are incommensurate. Ruddiman has suggested that this may be a function of the protracted deglaciation in MIS 11. According to this argument, the MIS 12/11 deglaciation lasted almost 20 kyr, while the MIS 2/1 deglaciation lasted only about 10 kyr.

Anonymous Referee #2

In his manuscript Tzedakis compares astronomical parameters, ice core and tree pollen data over the last 800,000 years as a basis for discussing the analogy of MIS 19 and MIS 11 to the current interglacial period (MIS 1). Identifying a past time period of similar astronomical parameters suggests climate responding similarly to the radiative forcing without additional anthropogenic forcing. How early the human influenced global climate in the course of the Holocene can not be

finally answered. In the introduction Tzedakis summarizes very accurately the ongoing discussion of the "early anthropogenic hypothesis" over the recent years. The manuscript is well written and easy to understand. Tzedakis adds pollen records representing southern European vegetation changes to test the quality of the analogy of MIS 11 resp. MIS 19 to MIS 1. Unfortunately, the presented pollen records do not deliver new insights on this analogy. Comparisons between different time periods are not very robust. They confirm what is known already from ice core and marine records.

Referee 2 is correct to point out that a comparison of the different alignment scheme based on the tree pollen records is not particularly robust, in the sense that what is being assessed is general trends. But this is equally true of comparisons using Antarctic temperatures or other proxies. However, I do not agree with the assessment that they do not add anything new on what is already known from ice core and marine records. While Antarctic D/H, marine isotopic and southern European pollen records are all sufficiently independent of methane records in order to undertake such comparisons, changes in southern European tree populations have the advantage of being closely coupled to low latitude methane emissions via shifts in the mean latitudinal position of the ITCZ. This makes the pollen record well-suited to provide an assessment of the natural vs. anthropogenic nature of methane trends during the Holocene.

The study uses the fact that the presented European tree pollen records are well correlated to the global atmospheric methane concentration reconstructed from the EPICA Dome C ice core. This is based on the assumption, that tree population is sensitive to the hydrological cycle in a similar way as microbial activity for methane production, methane oxidation, methane transport in the soil and extent of potential methane source regions. The correlation is certainly true for millennial scale variations and there are mechanisms that can explain it, as presented in Tzedakis et al., 2009. However, it has been shown that temperature variations during rapid Dansgaard-Oeschger events lead to variations in Northern Hemispheric sources that alone could explain the observed methane concentration variability at millennial time scales.

Tzedakis et al. (2009) focused on orbital changes, but also included a discussion on millennial-scale variability in southern European tree populations. They argued that millennial-scale changes could also be placed within the framework of north-south shifts in the mean position of the ITCZ, which are in turn associated with changes in the Atlantic Meridional Overturning Circulation. However, they also noted that the coupling between low-latitude and southern European changes does not exclude the possibility of additional contributions to the methane budget from extra-tropical sources.

But glacial-interglacial changes in atmospheric methane in addition certainly involves tropical source changes on a global scale. Looking at Fig. 3 I'm not convinced that, despite the proposed mechanisms, the presented regional pollen records (located north of 35 N) do well reflect the tropical climate signal and thus variations in tropical

methane sources. I doubt that there is a strong correlation for interglacials between pollen records and atmospheric methane before or after removing common millennial scale variations. But this has to be shown.

Tzedakis et al. (2009) have shown that there is a strong correlation for interglacials and discussed the mechanism for this (see reply to same point by Referee 1). In addition, it is important to clarify that the Portuguese temperate tree pollen records presented in Fig. 3 *do not* “reflect a tropical climate signal” as suggested by Referee 2, but rather that both low latitude and southern European hydrological changes are linked via shifts in the ITCZ. *This is a subtle but key difference.* It also important to underline that changes in the hydrological cycle are not always of the same seasonal sign in the tropics and southern Europe (for example enhanced summer precipitation in low latitudes and summer aridity in the Mediterranean during intervals of northern insolation maxima), but the observed changes are consistent with ITCZ displacement. All this is discussed in depth in Tzedakis et al. (2009).

Given the fact pollen records do co-vary with atmospheric methane except for the Holocene does not help very much in terms of aligning different time periods, this can be done easier using the methane record alone. Is there a reason why atmospheric carbon dioxide concentration has not been presented in the plots? It certainly has a bigger effect on the amplification of net radiative forcing than methane. Including carbon dioxide, a well integrated biochemical tracer of the climate system, in this analysis, would help to improve the characterization of interglacials. In addition, how does the changing carbon dioxide fertilizing effect on plant growth affect the vegetation distribution of trees and grasses? Is this of importance for glacial-interglacial carbon dioxide changes in the order of 100 ppm?

Indeed CO₂ has an overwhelmingly larger impact on terrestrial vegetation than methane, both through its effects on climate and also via direct fertilization. On glacial-interglacial timescales, lower atmospheric CO₂ content leads to lower photosynthetic rates and reduced water-use efficiency of plants. Within the course of interglacials, however, the direct CO₂ effect on vegetation is limited, as the magnitude of changes in CO₂ concentration is relatively small. Thus, while variations in CO₂ concentrations and southern European tree populations over the last 800 kyr reveal similarities on glacial-interglacial timescales, in detail the two records do not show the exact same patterns during interglacials (Fig. 2). By comparison, tree population changes and atmospheric CH₄ concentrations show a much greater coherence (note for example the “M” structure in MIS 11c). It is important to underline, however, that this coherence is not a direct result of atmospheric methane effects on vegetation. Instead, the close coupling between the records reflect shifts in the mean latitudinal position of the ITCZ and its impact on low- and mid-latitude hydrological changes, which respectively affect atmospheric methane emissions and southern European vegetation. *It is precisely the fact that methane is not directly involved in forcing vegetation changes (in contrast to CO₂), that the comparison between the two records is sufficiently independent to evaluate the natural vs. anthropogenic nature of the Holocene methane record.*

The highlighted decrease in the abundance of trees in southern Europe and the atmospheric methane increase in the late Holocene are difficult to compare with their counterpart in MIS 11 and MIS 19. While in MIS 11 any statement depends on the alignment of the records, the conclusion for MIS 19 is rather weak due to the very low time resolution of the single temperate tree pollen record.

Indeed, the absence of high-resolution well-dated MIS 19 pollen records does not allow an evaluation of the hypothesis for a Southern Hemisphere source for the late interglacial methane peaks. The limitations of the MIS 1 – MIS 19 pollen-based comparison are already discussed in the text. However, it is not true that the MIS 19 discussion offers limited insights. While the issue of the methane trends could not be assessed conclusively, the alignment of MIS 1 and MIS 19 suggests that the Holocene has another 9 kyr to run its course, assuming that the orbital analogy is correct.

This leaves the reader with some criticism concerning the terrestrial pollen record from Charco da Candieira and its agreement with the records from the deep sea cores. Is there an overlapping period for these records that could be shown in a graph? Do the pollen data sets from the different cores represent the same vegetation area?

This is a valid point, also raised by Referee 1 and addressed in the previous section.

Concerning the periods MIS 11 and MIS 1 it would be very interesting to plot all data in one figure using an alignment based on greenhouse gases and Antarctic temperature. Then leads and lags of pollen records and astronomical parameters could be discussed in a mechanistic way. Comparisons like in Fig. 4, where glacial-interglacial temperature increases are separated by more than 10 kyr are not really insightful. This is crucial, especially if one considers that ice core dating uncertainty is in the order of +/-1% or better. Generally, absolute and relative dating uncertainties of the presented data sets should be provided and included in the discussion.

Similar points have been raised by Referee 1 and addressed in the previous section.

It is well noted that any alignment for MIS 11 and MIS 1, using either precessional or orbital periods, does not lead to a convincing agreement in the paleo records and thus MIS 19 is the time period to consider as an analogue. As mentioned above the current analysis of the presented pollen records do not give strong evidence for or against the "early anthropogenic hypothesis" and needs to be improved qualitatively and quantitatively.

This is not entirely correct. The pollen-based comparison of the vegetation trends in MIS 1 and MIS 11 favoured the precessional alignment of the 'LBR' scheme of the two interglacials. This would support the notion that in the absence of anthropogenic interference, the Holocene should be nearing its natural completion. On the other hand, alignment of MIS 19 with MIS 1 (which is straightforward and does not require pollen records) suggests that the Holocene has another ~9 kyr to run its natural course. I agree that the paper does not provide a solution to this debate, but that is precisely the 'take home message': if answers vary with the choice of analogue, resolution of these issues using past interglacials remains inconclusive.

Anonymous Referee #3

In general this is a very well written paper addressing an interesting topic and notably bringing the view from terrestrial pollen record to arguments about the right analogs for the Holocene with respect to the "early anthropogenic hypothesis." The argument that MIS 19 may be a better analog is clearly presented and the strengths and weaknesses of this position are discussed in an even-handed way. Although there are some weaknesses in the data, in the sense that it would be desirable to have a higher resolution pollen record, I think they are fairly dealt with and that this paper can stimulate more thinking on the subject.

Ultimately the point of this paper hinges on Figures 7 and 8, and whether the methane trends in MIS19 and MIS1 as plotted in that figure 7 look similar, and whether the pollen and methane trends as plotted in Figure 8 look similar. Given the lower resolution of the MIS 19 data, and the fact that every interglacial must be a little bit different, the answer is a bit subjective. The EDC methane peak at 780 ka may be a millennial scale feature, in which case it would not necessarily be related to the characteristics of interglacials. On the other hand, perhaps the late Holocene rise in methane is a result of similar millennial scale processes.

Both Referee 3 and Bill Ruddiman (in his interactive comment) are correct to point out that the MIS 19 CH₄ peak 778 ka may be a millennial-scale feature, not related to orbital changes. This possibility was not considered in the original manuscript and should be included in the revised MS (see also my response to Ruddiman's comment).

The comparisons in Figures 7 and 8 are probably the best we have for now, and the manuscript recognizes the limitations of the data quite honestly. Some specific comments on the manuscript: A primary comment is simply a request to make it clearer what is meant by "precessional alignment," "termination alignment", and "obliquity alignment." As I understand it the original time scales are used for most of the data sets. For example in Figure 2 ice core methane and dD data are plotted on the EDC3 time scale for -50 to +50 ka and the MIS 11 interval. An orbital parameter, either precession or obliquity, is also plotted, presumably on the orbital time scale (the x-axis label is "EDC3 time scale", making this confusing). Then, the MIS11 data and orbital parameters are simply shifted en masse with respect to the data and

orbital parameters for -50 to +50 ka, until either the precession parameter variations match, or the terminations match (or later in the paper, until the obliquity curves match). It may be important to point out that the methane data are not used to align data sets, as has been done by other authors. Errors in the ice core time scale are not discussed here, as noted by other reviewers, but they should be. Also, it would be useful to describe what the precession parameter plotted in Figure 1 and elsewhere is. For readers used to seeing insolation curves the appearance of a negative value for precession at 10 ka may at first glance be puzzling - a brief explanation (text book stuff) could be helpful.

These are valid points and can be incorporated in a revised version.

Page 1344, Line 6. The role of VOC in the methane budget is not that certain. See Leleiveld et al. 2008 in Nature for an alternate view.

Referee 3 is correct to point out that the effect of VOCs on the concentration of OH radicals is subject to large uncertainties. This has been highlighted by unexpectedly high OH concentrations measured over tropical rainforest, despite the release of vast quantities of VOCs by tropical vegetation (Lelieveld et al., 2008). Efficient OH recycling may sustain the atmospheric oxidation capacity (Lelieveld et al., 2008), which suggests that there may be processes mitigating the VOC effect on long timescales.

EW Wolff (Editor)

This paper has received three reviews and one discussion comment. All the reviewers, either explicitly or implicitly, find the paper to be clearly-written, and to be discussing an important topic. They all make some minor comments which the author should address in his author response and revised version.

Two of the reviewers are however rather negative about the overall premise of the paper. In trying to assess how to treat their criticisms, I feel they separate into two main issues. The first one concerns whether any orbital alignment is valuable or viable: in a way this is more a criticism of the whole Ruddiman hypothesis than of this paper, which starts from the premise that there might be an orbital alignment and tries to assess which makes more sense. To deal with this first more general point, I would urge the author just to make it clear where he is following an idea rather than approving it. For example, for me the point is very well made that while the precessional alignment for MIS11 has some merit in terms of the closest orbital fit, it performs disastrously at predicting the time of termination. I think the author can deal with this point by reminding the reader is that there is more to methane than just orbital control (clearly there are also millennial and deglacial signals), and being clear then what the hypothesis is in expecting to be able to make such orbital alignments.

Indeed these ideas can be made more explicit in a revised version, as I have discussed in my replies to referees' comments in the previous sections.

The second point made by both negative reviewers is to question whether southern European vegetation is indeed closely linked to methane at the timescales of interest in this paper. This indeed needs more attention: the author's previous paper showed a link over the longer timescale, but it is still a fair question to ask whether this applies during interglacials. The author therefore needs to do more work in explaining Figure 3: both in explaining what relationship he expects between the methane and the two pollen records, and in demonstrating (statistically if possible) that the link is firm during interglacials. For example, while the temperate pollen-methane link looks good in MIS11 and 9e, it is much less obvious in 7e, and only holds in 5e if we allow a large phase shift. And reviewer 1 was clearly unable to tell what relationship we are supposed to see between Ericaceae and methane at all. This should all be discussed in order to establish just how certain we are that the pollen records can help us with this problem.

I have attempted to address the referees' points in the previous sections and some of that discussion will be incorporated in the revised version. However, there is a danger of simply repeating long tracts from Tzedakis et al. (2009), where the arguments have been already rehearsed. With respect to Ericaceae, no correlation with methane was proposed anywhere in the manuscript and this can be made clearer in the figure caption. Regarding the pollen-methane correlation during MIS 5e, the editor is correct that it requires a phase shift of ~2 kyr. However, the chronology of that particular section in core MD95-2042 is based on inferred sea-level still-stands (Shackleton et al., 2002), and is not as robust as that of MD01-2443. Recent work (e.g. Drysdale et al., 2009) suggests an earlier timing for the MIS 6/5e deglaciation and brings the pollen and methane records in phase, but I think the matter is not fully settled yet.

Of course the end result is rather uncertain, both because of uncertainties about the integrity (i.e. whether natural or not) of the MIS1 pollen record, and because of the low resolution in Tenaghi Philippon MIS 19. However, I think this idea has sufficiently interesting potential that it should be published after revision, and I invite the author to prepare a new version for CP. In doing that, he should answer the detailed points made by each reviewer, clarify the suppositions behind the orbital alignment idea, and especially do more work on discussing how well the pollen-methane relationship holds in interglacials (Fig 3).

I thank the editor for the clarity of his comments and guidance provided in this matter.

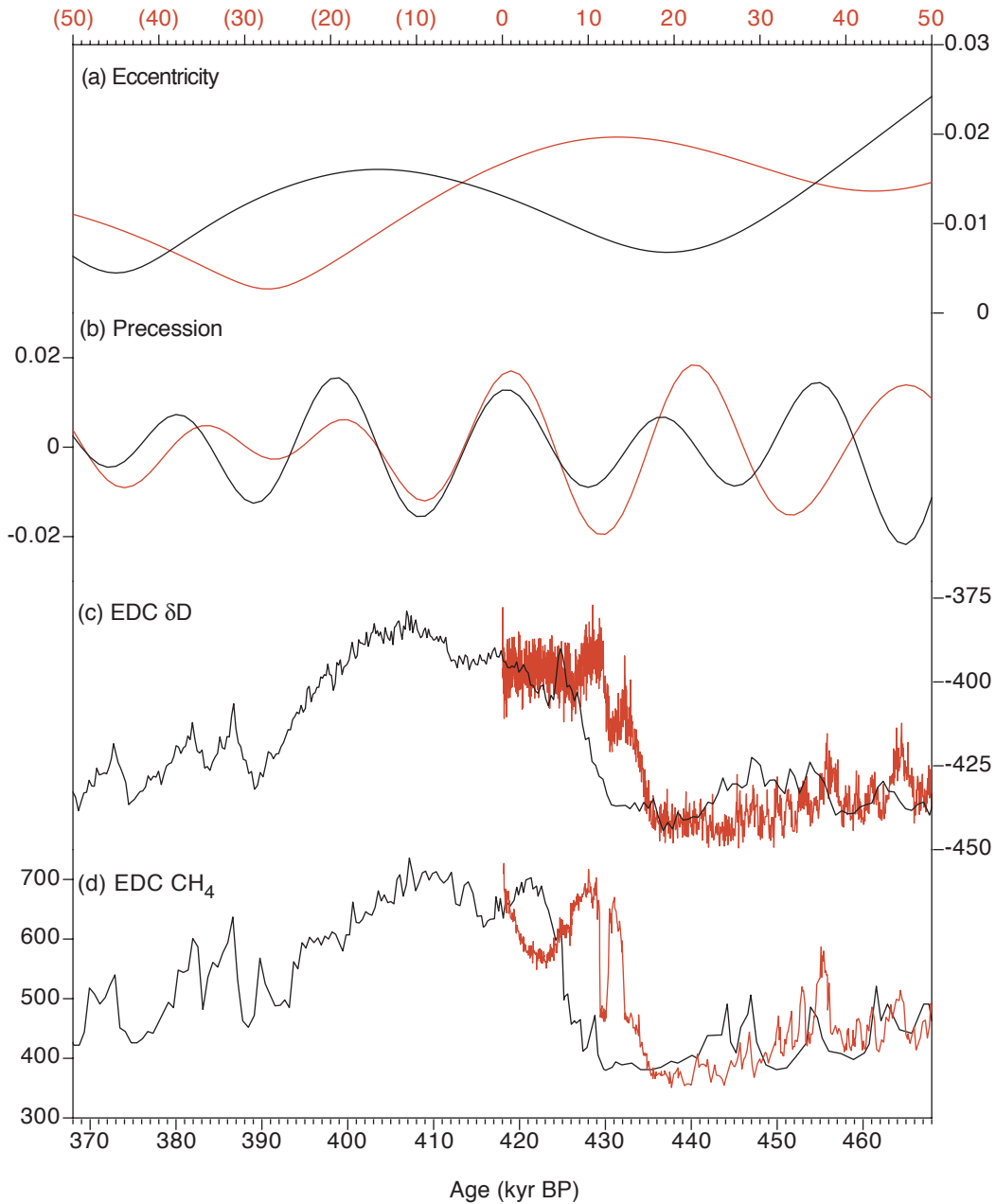


Fig. 1 Alternative MIS 1 - MIS 11 precessional alignment suggested by Referee 1, so that today corresponds to 418 ka (i.e. one precession cycle offset compared to the Loutre-Berger/Ruddiman alignment). This leads to a significant divergence in the phasing of the eccentricity variations between the two periods. (a) eccentricity and (b) precession index, plotted on the astronomical timescale (Berger, 1978); (c) δD composition of ice in the EDC ice core, Antarctica (Jouzel et al., 2007), plotted on the EDC3 timescale; (d) atmospheric CH_4 concentration from Antarctic EDC ice core (Louergue et al., 2008), plotted on the EDC3 timescale.

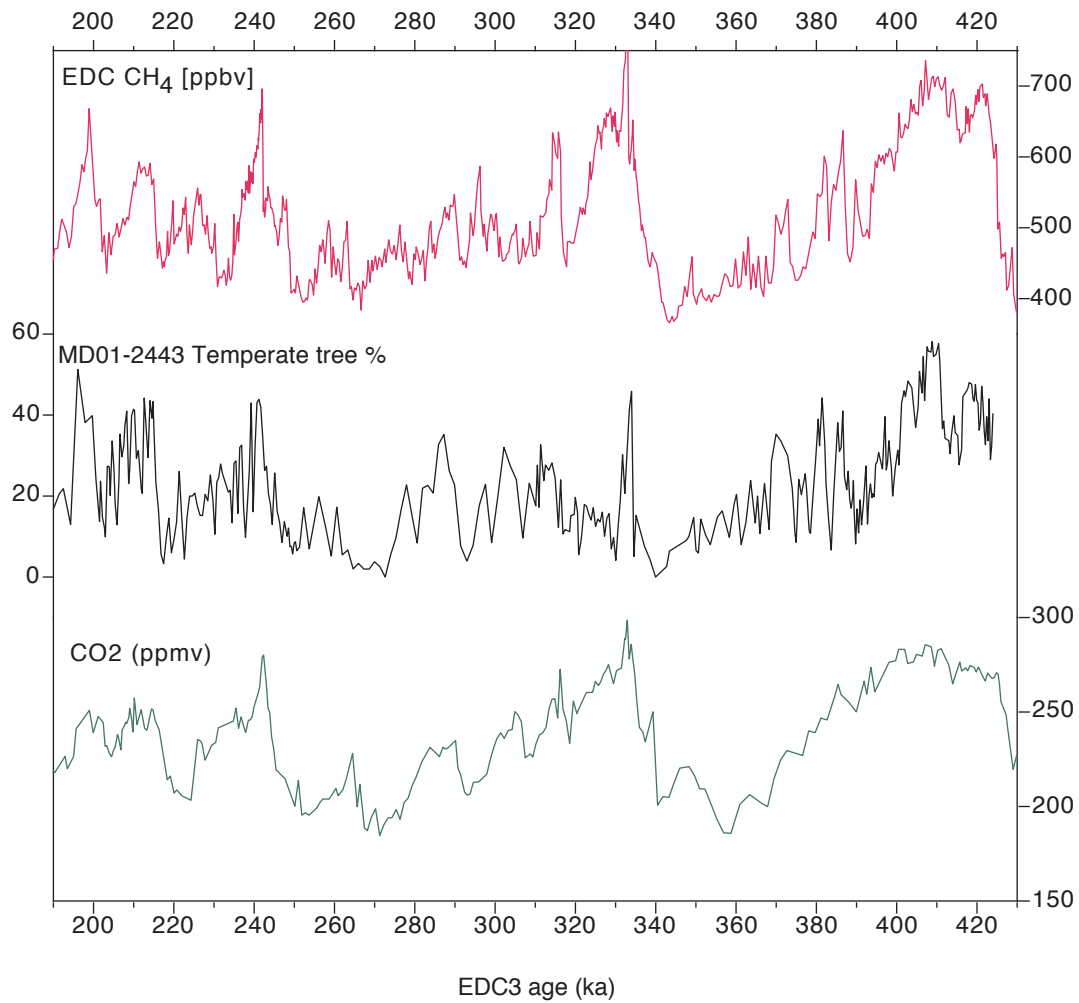


Fig. 2 Comparison of atmospheric methane (Loulergue et al., 2008) and carbon dioxide (Lüthi et al., 2008) concentrations from Antarctic ice cores and temperate tree pollen percentages from marine core MD01-2443 (Tzedakis et al., 2009) in the Portuguese margin.