

Interactive comment 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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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,

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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.

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.

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

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especially do more work on discussing how well the pollen-methane relationship holds in interglacials (Fig 3).

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