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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "A few prospective ideas on climate reconstruction: from a statistical single proxy approach towards a multi-proxy and dynamical approach" *by* J. Guiot et al.

Anonymous Referee #3

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General comments:

Although this is in principle an interesting paper, it has a number of important limitations. Foremost amongst these are:

- The paper does not provide sufficient detail in relation to many of the methods used, referring in some cases to other papers that are not yet published and hence not available for consultation.
- The paper appears to offer little if anything that is novel, although the extent to which this is the case is rendered difficult to assess because of its citation of other

papers that are not yet available to read. However, inverting vegetation models as a way to infer palaeoclimate is not new, and nor is the use of additional proxies: The lead author published on the use of lake-level data in this way many years ago and the authors cite an as yet unpublished paper (also currently in open review for the same journal) in relation to the use of δ^{13}C as such an additional proxy.

- The possibility that lowered atmospheric CO_2 concentrations resulted in vegetation implying more arid conditions during the last glacial stage than in reality prevailed is a proposition that has been discussed for at least a decade. The development of this argument has often rested upon the application of vegetation models (see e.g. Cowling & Sykes, 1999). Once again, this paper does not appear to contribute anything substantial or novel to this debate.

Perhaps the principal limitation of the paper, however, is that the authors simply demonstrate, using their approach(es) based upon inversion of a vegetation model(s), that the palaeoclimate reconstructions obtained when taking into account the simulated plant physiological effects of lowered atmospheric CO_2 concentrations, or when applying constraints relating to other proxies apart from pollen data, differ from those obtained when the effects of lowered atmospheric CO_2 concentrations are ignored or when using the pollen data alone. They offer no evidence as to whether these different reconstructions are more accurate. This, however, is the nub of the issue: What we all seek are more accurate reconstructions of palaeoclimatic conditions. Of course, demonstrating the accuracy of reconstructions is not straightforward; such demonstrations require independent evidence of the past state of something that itself can be simulated from the reconstructed palaeoclimate. Nonetheless, without such assessments of the extent to which the accuracy of the reconstructions has been increased by the additional complexity of the approach, or by the constraints derived from additional data sources, the value of these new approaches cannot be established or evaluated.

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As a final general point, I find quite unconvincing the argument that the approach presented, and the progress that it represents, "*relax*(es) *the uniformitarian hypothesis*" (p. 112, lines 18–19). The uniformitarian principle was propounded originally in terms of **processes** and proposed that the past could be explained in terms of causes and processes occurring today. It is thus incorrect to consider that developing and using models to infer past environments on the basis of static modern analogues (e.g. surface pollen samples and modern climate) is any more dependent upon the uniformitarian principle than is the use in palaeoenvironmental inference of dynamic or mechanistic models that simulate processes known to occur today (e.g. vegetation dynamics; the physiological effects of atmospheric CO_2 concentration on plant growth and/or water use efficiency; the fractionation of ^{13}C). Indeed, arguably, the latter approach is more in keeping with the uniformitarian principle as originally proposed than is an assumption that modern analogues can be found for all past climatic conditions – something which in any case most in the field came to accept was not the case some time ago. Without an assumption that processes and causal relationships that we can observe and measure today operated also in the past, the reconstruction of past environments becomes impossible; that necessary assumption is based solidly upon the uniformitarian principle!

Specific issues:

1. A major focus of this paper is upon the limitations of pollen data as a basis for reconstructions of precipitation. This limitation, however, has been well known for *ca*. 20 years, although admittedly this has not discouraged numerous authors from continuing to attempt to make precipitation reconstructions from pollen data. The factor that determines the character of vegetation is not the amount of precipitation, however, but the availability of moisture when it is required by the vegetation for growth. In a Mediterranean climate regime the annual precipitation will often be similar to that in many Boreal areas; however, the seasonal distribution of the precipitation is quite different, as is the evaporative demand, with the re-

sult that under the Mediterranean regime vegetation faces a substantial moisture deficiency during the summer months, whereas in the Boreal regime there is no seasonal moisture deficiency. One solution that the authors do not discuss is the reconstruction from pollen data not of precipitation but of a more appropriate variable reflecting the limitations in moisture availability to the vegetation during the growth period.

2. "*Herbaceous vegetation is not really controlled by the winter conditions...*" (p. 102, line 5): Apart from the inherently temperate zone biased nature of this statement (in many parts of the world the principal seasonality is between wet and dry seasons rather than between summer and winter), it is simply incorrect. In many regions of Mediterranean climate, dominant components of the herbaceous vegetation include winter annuals and geophytes that produce above-ground organs during the winter months; these functional types depend for their successful growth and reproduction quite specifically upon the combination of temperature and moisture availability during the winter months. Furthermore, the importance of winter conditions is not limited to regions of Mediterranean climate; in many Boreal and Arctic regions the character of the non-forest vegetation is strongly determined by the extent to which snow accumulates to a sufficient depth to insulate the vegetation from extreme low temperatures and by the degree to which vegetation is exposed to 'frost drought' conditions of frozen soil combined with transpirational demand.

3. "*... it (*i.e. BIOME3) *assumes that there is no nitrogen limitation*" (p. 103, lines 5-6): Although this may well be a necessary simplifying assumption to make in order to simulate vegetation, it has potentially important implications for the accuracy of the simulations of NPP in many regions where vegetation is today demonstrably nitrogen limited. Given the focus of the paper on issues surrounding the accuracy of reconstructions of precipitation, it is relevant to note that nitrogen fixation is reduced, and nitrogen limitation thus more frequent, in situations where

soils either are waterlogged or moisture deficient (e.g. the Boreal zone or the Mediterranean), and that there also is a feedback between plant productivity and nitrogen fixation. I feel that the authors need at least to acknowledge these issues and to discuss briefly their potential confounding impacts upon their reconstructions.

4. "*...better ability of the model to simulate the LGM vegetation ... steppe–tundra has been introduced in BIOME4...*" (p. 105–6, lines 28/1-2): I had understood from the preceding text that the relationships upon which the reconstructions were based were those between the NPP values simulated by the BIOME3 model for the various PFTs and the PFT scores derived from the pollen data. If this is the case, then the number and/or nature of the biomes simulated by the model is irrelevant, because the biome simulated and/or inferred from the pollen data is not being taken into consideration. Thus, it makes no sense to me that by introducing a steppe–tundra biome in BIOME4 any advantage has been accrued. If there really is an 'improvement' in the reconstructions made using BIOME4, and using pollen-based biome scores as opposed to PFT scores, then it must arise either from some other difference in the formulation of the model or from the switch to using pollen-based biome scores rather than PFT scores. It would also be helpful to have some explanation of the reason behind this latter change of approach because, on the face of it, this seems a backward step: Whilst a uniformitarian approach to the definition of PFTs seems reasonable, the palaeoecological record strongly suggests that such an approach is not appropriate at the level of the biome, with some past biomes, including steppe–tundra, being without any extensive or obvious modern analogue.

5. Section 3: The use of past lake levels as additional information to 'improve' the reconstruction of precipitation is not new, but was the subject of a publication by the lead author some 16 years ago (Guiot *et al.*, 1993). The equating of changes in lake water level with changes in $P - E$ in the present analysis, however, im-

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plies a (simple) linear relationship between the two. Such a relationship is highly improbable. Given their use of a vegetation model to simulate the vegetation for possible combinations of climatic variables, it would be much more appropriate for the authors also to use a catchment model (a type of model with which the lead author is familiar, see e.g. Vassiljev *et al.*, 1998) to simulate the lake water level for the same combinations of climatic variables.

6. Figures 1 and 2: I question the appropriateness of reconstructions of mean annual temperature from pollen data, and thus of basing comparisons between reconstructions upon this variable. Although correlations are often reported between mean annual temperature and major vegetation boundaries (e.g. treelines), vegetation responds mechanistically to temperature seasonality and to accumulated warmth during the growing season rather than to the annual mean. I would much prefer to see the results presented in terms of winter and summer temperatures and/or the growing season thermal sum, variables that are mechanistically more relevant to the vegetation. Mean annual precipitation is in my view an even less appropriate variable to reconstruct from pollen data, and hence to use as the basis for comparisons between reconstructions, for reasons outlined above.

7. "*. . . variations of precipitation follow much better those of the lake levels. . .*" (p. 108, lines 14–15): Given that the climate reconstruction, and specifically the reconstructed precipitation, is constrained much more strongly by the assumed relationship between $P - E$ and lake level than it is by the vegetation (because vegetation is not generally directly sensitive to precipitation, as discussed above), this is an unsurprising result. Whether or not the reconstructed values are more accurate, however, is unproven.

8. "*This constraint gives a time-coherence . . . to reconstructed climate*" (p. 111, lines 18–19): The authors are not the first to develop and apply a method that

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provides time coherency and reconstructs climatic "histories"; they really ought to acknowledge this fact by citing Haslett *et al*. (2006).

9. "*...seasonality changes ... induced by variations of the earth (*sic.*) orbit ...* (are) *implicitly taken into account...*" (p. 113, lines 12–14): Given that the vegetation models used by the authors require 'mean sunshine' as one of their input variables, I would have expected them to have used suitably amended insolation values when simulating past vegetation. It is insufficient to suggest, as they do, that the effects of orbital variations are taken into account through their effects on temperature and precipitation, given that insolation intensity has a direct physiological effect on plants. At the very least I would expect to see this issue properly acknowledged and discussed.

10. Final two paragraphs: I find the authors' conclusion with respect to the need to model "*pollen dispersion* (*sic.*)", and the claim, attributed to André Berger, that real progress will only be achieved when "*an integrated model of the pollen accumulation in the core ...* (including) *all the processes such as vegetation development, pollen dispersion, catchment basin erosion, sediment accumulation*" has been built, unconvincing. There may be a place for such complex integrated models, but as yet we are far from being able to parameterise many components of such complex models. To suggest that real progress will not be made until we are in a position to develop such models seems to me an unnecessarily negative and misleading stance. Just as a great deal has been learned about the climate system using simple models, of which, incidentally, André Berger, made considerable use in his research, it seems to me that there continues to be a place for simple models in the field of palaeoclimate reconstruction from biological evidence. To suggest that we cannot make real progress with such simple models is, I believe, demonstrably incorrect: A review of the progress made in this field over the past 20 years by the senior author of the present paper would, I believe, support my view.

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Editorial issues:

The number of grammatical errors and misuses of the English language are far too many for me to be willing to take the time to enumerate them. In order to be acceptable for publication the entire text requires careful revision and correction by a native English speaker.

References cited:

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