

## ***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.***

**J. Guiot et al.**

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Dear editor,

We have tried to take into account the comments of the three reviewers, which have been very useful to improve the paper. Two of them have focalized on the statistics behind the paper and the third on ecological and paleoecological problems raised by it. It is not possible to write a paper with a full description of the statistics in a journal like Climate of the Past. It was not our objective and we think that it is not useful. Our objective is to present a concept, and we have rewritten the paper more clearly in that mind. In other words, we aim to excite the curiosity of paleoclimatologists on the possibilities opened by vegetation modeling in climate reconstruction purpose. For

that we have presented the concept with two new figures, hoping that images are better than a long speech.

REV#1 (C. Buck) Her first general comment is that there is no details about the model structure and different choices done about parametrization and implementation. Indeed there is more detailed papers earlier published. Maybe these papers do not satisfy a statistician, and maybe we should write a paper repositioning the statistical frame of such an approach, but it is not the place of such a paper in Climate of the Past. As told in introduction of this letter, we decided to given access intuitively to the reader, using synthetic schemes. We hope so that the standard reader of CP, if interested, will read other papers to know more. For others, it will give the state of the art of these promising developments.

Another criticism was that we have “ to provide reasonable motivation for use of a fully probabilistic framework and outline how the Bayesian framework can be used to update from prior to posterior ”. We have added a section on the concept being Bayesian theory and we have tried to give in simple words how inversion modelling is connected with that theory.

We have not prepared any technical appendix, as suggested, because they exist in several previous papers. Particularly, in a submitted paper of Garreta et al (Clim. Dyn).

It is also requested to provide the code used to produce the results reported in the paper. We know that we should do that, but, in fact we have used several codes in the different papers previously published, and these codes are not sufficiently “ clean ” to be accessible to potential users. We intend to work on a more general code, maybe in R, and to release it before the end of the year.

Concerning the chronological uncertainty, treatment of chronology needs a paper per se as the reviewer knows. This paper is already enough complicated as it is, without adding the uncertainties due to ages. We have added a sentence in the last paragraph of the conclusions presenting that as a perspective.

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We have also tried to correct the poor English of the text. We hope that the captions are clearer.

It is true that in the previous version, we started without choosing the type of proxies and abruptly we focused on pollen. In this version, we decided to start directly with pollen, as there is not really other proxies for which modeling is so advanced.

### Specific comments

“ Why is the general explanation of Bayesian inference provided in a sub-section headed ‘Europe at the last Glacial Maximum’? ” : The reviewer is right, we have introduced Bayesian inference section in the new ms. “ Lines 1 to 14 on page 105 seem pivotal to our understanding of the methods used, but the English is poor and they are very hard to follow. ” : We hoped to have sufficiently corrected “ On page 105, in line 16, what is ‘bivariate’? ” : Corrected by : “ the relationship between NPP simulations of the model PFT’s and pollen PFT scores “ ” : Section 2.2 implies that the reconstruction in 2.1 uses BIOME3, but this is not clear. Page 107, line 16, implies that 2.1 uses only pollen data ”: We have better explained the various models used “ Figs 1 and 2 show relationships between temperature and precipitation and between precipitation and latitude. What is the importance of the linear fits shown on these plots? ” : there is only relationships between climate variables and latitude. The linear fit is proposed to test the idea that the climate anomalies depend on latitude. - “ On page 108, in Equation 1, limits are shown for rejection sampling without any explanation as to how they were selected. ” : It is just expert guess, other values are possible, the results are not too sensitive. It is now precised (just after the equation with reference to Cheddadi et al 1996) - page 110, line 21: That sentence means that monthly climate time-series are interpolated at the location where the pollen sites are available, from the gridded dataset. This is done independently on the MCMC. We have tried to be clearer in caption of Fig 8.

REV#3

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Concerning the lack of details, we hope that our reply to previous comments will convince the reviewer. Even if the paper cites other papers (several already published and the last one being submitted), we have tried in this version to be self-understandable. Evidently, all cannot be understood in details, but reader can get some flavor of what is a model inversion and what are the potentialities.

Concerning the fact that the paper does not bring anything to the debate of CO<sub>2</sub>, we agree, but what it brings is a solution to biases induced by that problem.

“ They offer no evidence as to whether these different reconstructions are more accurate ” : This paper tries to answer to these questions: what kind of climate is compatible with proxies, given a certain representation (and simplification) of the processes relating proxies and climate, and, if we admit the realism of this representation, can external forcings (such as CO<sub>2</sub>) induce significant biases on the reconstructions? If the answer to the second question is positive, it means that we have to consider that non-process-based reconstructions may exaggerate cooling or drying. This fact cannot be verified statistically on modern data as external factors are precisely different from those of past. But we state that only a mechanistic approach may help.

“ 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&#8211;19). ” : The reviewer is right and we thank him to have pushed us to get better informed on that hypothesis. We have added a paragraph with reference to original authors to clarify it. Nevertheless, we think that hypotheses on analogues are stronger than on processes. We agree that uniformitarian hypothesis is the minimum constraint to reconstruct past climate. We hope that it is now clearer.

1. Concerning the use of precipitation instead of water availability: Indeed, the use of a vegetation model clearly assumes that the constraining variable is water availability. But, by inversion, we may go back to monthly precipitation and temperature, which

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determine this water availability. Nothing hinders one to deduce annual precipitation from the estimated values. It is really a power of that approach. What is pointed out in the last section with LPJ-GUESS is that the model is not enough sensitive to the precipitation seasonality. It is something that must be understood.

2. About the climatic control of herbaceous vegetation: our sentence was too rough. What we mean is : steppic vegetation is less controlled by precipitation in winter than during growing season. In the revised version, this paragraph has been removed as it did not bring anything to the paper.

3. About nitrogen limitation: we are not sure that it was necessary to cover extensively all the limitations of the model used. They are numerous and acknowledging these features will not bring a lot to the paper. Our approach depends on the model used, it is clearly told.

4. About steppe-tundra and biome4: Even if the steppe-tundra biome is explicitly present in BIOME4 and not in pollen-derived biomes, it is an improvement because the relative balance between steppic and tundra biome scores is a way to discriminate between steppes and tundra, an intermediate case being precisely steppe-tundra biome with high scores of both. We have added a sentence to explain that.

5. “ The use of past lake levels as additional information to improve the reconstruction of precipitation is not new ” : We do not pretend that it is new. Instead to use a statistical approach coupled with a bucket model for P-E (as in Guiot et al 1993), we use the more sophisticated built-in model of BIOME4 which takes into account the vegetation of the catchment area of the lakes. We have so a complete coherence between hydro and vegetation parameters. Using another hydrological model as this of Vassiljev is not possible except if we couple completely it with BIOME4. Moreover, we do not assume a linear relationship between lake levels and P-E, but we distinguish three classes for P-E which is much more flexible than a linear relationship. For example, a lake level  $>0.5$  m can be obtained for all P-E  $> -100$

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mm. This keeps a large range of possibilities.

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 ” : If the paper’s focus was paleoclimatological interpretation, I agree. Here we wanted to limit the number of variables to two and to have a climatic frame compatible with pollen but also lake-levels. We decided to use annual temperature and precipitation. In the original papers, other variables are presented.

7. about P reconstruction which follows lake-levels : I agree that it is unsurprising. It is sometimes encouraging to obtain expected results. But it is not the only one result. Uncertainties are also much lower with lake constraints. This proves that results are more accurate. This help also to answer to one general comment above.

8. About the Haslett et al (2006): Reviewer is right. It is an unforgivable omission. It is corrected.

9. seasonality and insolation: It is true and we thanks the reviewer for this remark. Indeed we have assumed that insolation was constant. In the future, we have to introduce the true insolation in the model to be completely coherent with our assertion. We have modified the text accordingly in the conclusion.

10. Final two paragraphs : we agree that there is a place for simple model. This does not hinder the far objective to go also to complex models. This remark is true in all the fields and not only in paleoclimatology. We have modified the text to be more nuanced.

REV#4

Reviewer#4 acknowledges the importance of the paper. He spent some time to prove that, but also to show the weaknesses of the paper. We are grateful for that. We also acknowledge that the paper was not well presented and we have done some effort to present the approach differently (see answers to rev#1). We have simplified the paper

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so that it will be able to give a flavour of inverse modelling applied in paleoclimatology to non specialists. Technical details should be available in other papers (some are already published and others are in press). We would like that he accepts that there much more gain to present something, maybe approximative from the point of view of the statistician, but helpful to convince non statisticians to be open to such an approach.

Concerning the paragraph about semantics of climate reconstruction “ Firstly, it may be useful to rehearse the objectives of that which we call ‘climate reconstruction’ : some terms are extensively used in our discipline. We do not think that it can be discussed here. We have tried to contribute to that problematic, but it is just a contribution.

Concerning the following paragraph starting with “ Secondly, we remind ourselves that the aspects of climate we are concerned have ‘space-time’ dimensions, ”, we can just say that the repositionning of the problem in a time-space context is certainly the direction to go as suggested in application E and much better explained in the corresponding paper of Garreta et al. Other papers are in preparation, but at the present state of the developments, we can not tell more.

Concerning the Bayesian methods, we are aware that they are slow (it is discussed in the Garreta’s paper), and much more with the use of a mechanistic model than without as in the Haslett et al (2006) paper (we mention it now, sorry for the omission).

### Statistical criticisms

Section 5 is maybe the most important section of the paper in the view of the reviewer. For us, the corresponding paper is submitted to Climate Dynamics (and soon accepted, we hope). We just wanted to give a flavour of it. We appreciate the effort of the reviewer to give us tracks to improve this paper from his point of view. We acknowledge that we have too much hesitated between two types of paper. As we have adopted a ‘diet’ statistical point of view, we cannot really follow him in that way. Nevertheless we have used some of his comments to improve the statistical

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background of the paper.

Concerning the idea that pollen data do not record vegetation, we disagree. Pollen data are more or less noisy records of vegetation. It is admitted by paleoecologists. The bridge between pollen and climate is precisely vegetation.

Concerning the time lag between vegetation and climate, it is true that it exists (certainly less than 200 years, a few decades more likely, and the response is immediate when climate become unfavourable and longer when it becomes more favourable). There is some papers published in the past decades showing that the lag remains of the same magnitude of the datation uncertainty. There is no &#8220;a priori&#8221; problem to integrate it in the model.

#### Editorial criticisms

We have restructured the paper as follows: first we explain the methods (analogues, vegetation modeling, inverse modeling in the frame of Bayesian theory) and then we present five examples in an order going from the simplest application to the most complex. This is illustrated with Fig2. We hope that the mind behind the paper will be clearer. “ Some things are over-stated; eg 100: 12-16 The main results are that: ” : we have written differently “ Some things are introduced and never again mentioned; eg 101:4 presence-absence; eg 111:12 particle filter algorithm. ” : for the first point, it has an historical utility, but now it is told differently; for the second, it is clarified in the section on inversion algorithms “ Some things are introduced but the explanation is impossible to read &#8230; ” : we think that it is clear for paleoclimatologists “ Some are confusing; eg 100: 5 In particular, vegetation models provide outputs comparable to pollen data. I really don&#8217;t know what this is intended to mean. I see &#8216;pollen data&#8217; as counts. ” : We have added a sentence to precise &#8220;eg 101: 11 These relationships are called transfer functions. Some do call them that; but is this universal usage? I think not ” : yes it is, at least in paleoclimatology. “ eg 104:7-8 As we are more interested in finding a range (or distribution) of possible climate, it is

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preferable to adopt the Bayesian.. !!! ; as though frequency-based likelihood cannot give ranges!! how about Confidence Intervals? ” : the reviewer is right, but we wanted here to make an opposition with a simple optimisation algorithm. Now, this kind of assertion has disappeared. “ eg 104:11 (when statistical methods are used instead mechanistic models). !!! ; as though they are not using a statistical method themselves, but now one that ;works with; a mechanistic model. ” : it is presented differently now. “ There are many more ” : We hope to have corrected some of them by ourselves

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Interactive comment on Clim. Past Discuss., 5, 99, 2009.

## CPD

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