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Climate of the Past Discussions

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 #4

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Review of Guiot et al 2009: A few prospective ideas..........

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Overall remarks

In my opinion, this paper makes a very important and constructive proposal: introduce into (eg) pollen-based palaeoclimate reconstruction ideas that already exist in 'mechanistic' models of vegetation dynamics. A stated central purpose of this paper is ‘to show how it has been possible’ to incorporate such mechanistic modelling into vegetation dynamics. Another purpose is to argue that this is necessary.

My main scientific criticism of the paper is that the details of the statistical under-pinning are not (yet) available for criticism. Thus it is premature to assert that they have shown this to be ‘possible’. The authors know this: they say (line 16, p114) that: ‘This approach is still in development and some improvements are necessary to make the method fully operationnal.’ (sic) (Below I refer to such lines as 114:16.) CPD is not perhaps the place for such details. CPD may nevertheless be the place to publish some provocative if speculative remarks on such things; this is a matter for the Editors. If so, the authors could be encouraged to write such a paper.

I regret however that the quality of this presentation is such that it will need much revision to make it readable. I suspect that other referees, from other parts of science and seeing other contributions, will make similar remarks. Indeed, my positive remarks below are (a) at least partly based on my best guess of what it is that they do regard as the central contribution of their paper, (b) intended to restate it for the Editors of CPD and (c) may form part of the ultimate discussion.

In making these remarks, I write as a statistician interested in the challenges to statistical methodology posed by ‘climate reconstruction’.

I am therefore no expert on a central argument of this paper: that such reconstructions are open to criticism if they are based too heavily on the principle that ‘the present is a model for the past’, the so-called the uniformitarian principle. How vital to the science really is this assumption? It does indeed seem to me to be the ‘Achilles Heel’ of ‘climate reconstruction’; and I suspect that it is important. But this is a question which others will be more qualified to answer. Nevertheless, I welcome the ideas and find them stimulating.

My main concern thus concerns how statistical science can respond, constructively, to such an argument, presuming it to be important. As such the authors raise an

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interesting challenge. More than that, they claim to present a methodology for doing so. On this, I am perhaps qualified to offer some views. The authors of this paper, though not research statisticians, have made a brave job of discussing an interesting idea in the context of some very recent developments within statistics. However, I am not sure that the readers of CPD will be able, from this paper, to learn much about such developments.

My discussion then is in three sections. The first of these deals positively - and constructively, I hope - with the statistical science that lies within the paper. It is intended to persuade CPD that these ideas are to be encouraged. Secondly, I raise some questions on the methodology. Some of these, and their responses, will be for other journals; but the Editors and readers of CPD will need to know that they have been answered. The last section is more critical; it instances several of the very many editorial failings that I see in the presentation, failures that will, I believe, distract readers from the constructive contribution.

The central statistical ideas

Here I state my understanding of the authors' general research objective, and the wider research program within which this paper sits. Some of these very broad-brush statements may help the CPD editors - and ultimately CPD readers - to appreciate the authors' ideas; some will appear in the eventual discussion of this paper. The authors, I think, see the general problem as I do. But I regret that some of the details they do offer may confuse others. If I mis-state their objectives, I apologise; but the authors will have to take some responsibility for this.

Firstly, it may be useful to rehearse the objectives of that which we call 'climate reconstruction'. 'Reconstruction' is a dangerous word; it implies too much. I see the objective as the attempt to use all the information we have - and this involves a very great deal of poor quality proxy data - to make careful statistical inference about the past climate,

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or aspects of it. There are many approaches to such inference; they are all essentially statistical, whether or not they involve ‘backwards statistical relationships’, ‘transfer functions’, Bayesian ideas or mechanistic models. Even with all the information we have we will always be uncertain about the past climate. Such uncertainties include those of making inference from noisy data; they also include issues of bias both in the ‘point predictions’ we might make, but also in the precision we attribute to them. The challenge lies in ‘predicting’ (the past) while articulating the associated uncertainty in a credible way. This paper focuses on one potential source of bias; further it indicates that there may be a constructive way forward.

Secondly, we remind ourselves that the aspects of climate we are concerned have ‘space-time’ dimensions. Climate C should thus be seen as multivariate space-time process $C(s,t)$; it can be envisaged as a very high dimensional ‘stacked vector’ of all dimensions of C at all locations and times (s,t), or at least all on some space-time grid. There is, for example, an increasing interest in abrupt changes in climate, or more generally, with regional climate dynamics at a scale of perhaps one or two decades; the space-time nature of C is central to such discussions. The authors focus here on the evolution in time of climate at a specific location s_0, say; that is, with the process that is $C(s_0,t)$; thus here C is $C(s_0,t)$ for all t in some period. If time is treated discretely, it may comprise say T equal length short periods. They of course make statements about many such locations but at this stage, they do this ‘one-at-a-time’; ie marginally. They aspire to studying (a few dimensions of) $C(s_0,t)$ by sampling ‘histories’ (111:19); they use six (see 110:25). Such histories may be thought of as realisations of multivariate time series (ie vectors of length $6T$). Note that inference is now ‘many-at-a-time’ (ie jointly) and involves dynamic concepts; these include ‘borrowing strength’ from other ‘temporality close’ samples and thus some degree of temporal smoothing. Space-time inference would be ‘very-many-at-a-time’. The authors do not yet offer a practical way to sample such histories, as I understand it; but they can see one, they claim, in a technical algorithm referred to as ‘the particle-filter’.

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Inference for $C(s_0, t)$ for all t in some period cannot properly be deduced from marginal reconstructions $C(s_0, t_i)$ at points t_i in time, nor from the 'probability distribution' summaries (one dimension at a time) presented in papers such as this, nor even from the series that are presented in Figure 5. (I understand these to be the means of a one aspect of a set of marginal reconstructions.) The dynamic concepts cannot be found in marginal reconstructions. Note further that the printed page (a static document) greatly inhibits the presentation of such dynamic 'histories' and their joint uncertainty; an electronic journal such as CPD may offer this possibility. Nevertheless, sampling separately each specific (multivariate or scalar) $C(s_0, t_i)$ is a first challenge to overcome. I think that in this paper, the authors' focus is limited to this. Their aspirations are much larger, of course.

Finally they note that the 'information' certainly includes the modern world, in so far as modern data on both proxies and climate can indeed give us great insight into how climate change can lead to changes in proxies, or more precisely stated, to changes in the systems in which such proxies are embedded. It also includes theory about climate and proxy systems and about their interaction. These theories are in fact *the only basis* for inference about the unobserved past; without them we cannot extrapolate from observed systems to partially observed systems. The uniformitarian principle, and the science on which it stands, represent examples of such a theory. This paper points to a complementary theory and another basis for inference - mechanistic models such as LPJ-GUESS.

They remark that these theories must be consistent with field observations; but they must also be consistent with physics (reflected here in the word 'mechanistic') and with (some) laboratory experiments (eg with different CO_2 atmospheres). The authors are encouraging us to draw on such 'mechanistic' theories; equally importantly to the statistician, they are saying that such theories should become part of the information available to the task of inference. The need for this latter is already well recognised in the climate modelling literature: climates must exhibit dynamics that are consistent with

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physics. (The incorporation into palaeoclimate inference of models of climate dynamics still represents a great challenge.)

Bayesian methods

One very active area of statistical science, as the authors well know, lies with Bayesian approaches to 'partially observed stochastic systems, including 'palaeoclimate systems'. They use this paradigm to discuss their approach. But these methods are still in their infancy. Partly it is that the job - use 'all the information' .. to make 'careful statistical inference' about '... climate dynamics'- is a huge job. But partly it is that the statistical methods are still very new. In palaeoclimate reconstruction, Haslett et al (2006) offered a 'proof-of-concept' of one such approach, relying on Markov Chain Monte Carlo (MCMC) algorithms, and on the Metroplis-Hastings algorithm in particular. That paper in fact showed the need for another approach; for the algorithm was impossibly slow despite making unreasonable compromises on the models employed; and it was rightly criticised in the discussion. Approximations, such as Rue et al, (JRSS(B),2008, which avoid MCMC entirely, seem likely to have a very constructive role.

Bayesian methods of reconstruction involve the making of probabilistic statements about palaeoclimate C given data D, so-called posteriors. But recall that $C(s,t)$ is a very high dimensional object. These can be studied in principle by drawing random samples from the posterior using Monte Carlo methods; many technical details make this *much* more challenging than it might first appear; but the concept is simple. The posterior is built from (a) prior probability statements about the possible 'candidate values' of palaeoclimate, and (b) the likelihood, for each of these candidates, of observing this particular set of 'data'. In this paper the likelihood is defined by a 'forward' model; *given climate* what data can we expect? This is modelled statistically; many such models can be thought of as variations on regression. Formally, likelihood is typically

expressed as a conditional probability statement $\pi(D|C)$, where $\pi(.)$ denotes a prob-

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ability density function, for example; note that $\pi(.)$ typically contains parameters (eg regression coefficients, say θ), which we suppress in this notation for simplicity. But θ is in fact the source of most of the problems. Likelihood, depending on the model, can - for simple cases like regression - be a function a sum-of-squared residuals, as the authors remark (104:1 and 109:1); but the function must be stated. 'Inversion', as referred to in this paper, focuses on $\pi(C|D)$; this is a probability statement of past climate *given data*.

The authors argue that the introduction of an intermediate process V denoting vegetation is (a) necessary for the science and (b) possible in the context of studying $\pi(C|D)$. V is 'like' C - it is a multivariate space time process; it is dynamic. But it is no harder (or easier) in principle to make statements about $\pi(D|V)$ and $\pi(V|D)$ than for C.

Monte Carlo methods make statements about $\pi(C|D)$ by the device of drawing many random values of (the very high-dimensional) C from the posterior or, in a more limited study, of the possibly scalar $C(s_0, t_i)$; these are then summarised in figures such as the authors use. The procedure can be thought of in two stages: (i) draw random samples of C from the prior; (ii) reject those that are not consistent with D. The second step can be more subtle: preferentially accept those for which the likelihood (of D given C) is highest. The prior can be - and often is - 'vague'; we have no preference for **any** values of C at the first stage; as such the posterior is proportional to the likelihood. Informed priors can be used if available. The whole procedure can meaningfully be thought of as 'sampling the palaeoclimate' by randomly generating entire 'histories' $C(s_0, t)$ (vectors of length $6T$) using joint inference. Crucially, *all* such samples are equally likely, given the data; of course some features will be apparent in many samples. An average is a summary, and is easy for the eye to grasp, but is inadequate to the task. Several figures in this paper summarise the frequency distribution and are thus more satisfactory. But they do not adequately summarise histories.

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The contribution

The authors, in this paper, claim that the introduction and modelling of V, using a Bayesian approach, is possible and advantageous for the study of C. It can reduce bias, they argue; but it does extend the inferential chain and as yet (114:22) poses great computing challenges. The conceptual procedure uses models such as LPJ-GUESS to generate random vegetation given climate. Essentially this samples from $\pi(V|C)$, and becomes a second part of the first stage above. Secondly, the likelihood $\pi(D|V)$ is no harder to model statistically than $\pi(D|C)$; it is arguably more appropriate, as they remark, for it fits neatly with the concept of 'plant functional types'. More subtly, they further argue that *dynamic* models like LPJ-GUESS in fact model the impact of climate *change* on vegetation *change*.

This latter possibility is for them, and I think for me, the most exciting thing in this paper. Yet at a level of detail, the authors do little more than sketch the potentialities of the approach. This might be sufficient for CPD if these were laid out in detail somewhere else. But I don't think they are.

Statistical criticism

Section 5 is, I think, the most important section of the paper. That there is one page is regrettable. Several points need to be addressed.

As I see it, there is a need to describe a statistical model of the likelihood $\pi(D|V)$. In fact it is somewhat more subtle than this, for they rightly point out that they are more interested in dynamics and $\pi(D|V)$ must be derived from $\pi(\Delta D|\Delta V)$; here Δ denoted change.

I have no impression from the paper of what this is. Equation (2) is not a likelihood. I guess they have in mind a Gaussian model and exponentiating this function. Indeed

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throughout the paper, I cannot discern *any* statements about underlying probability distributions, other than the use of uniform priors on some dimensions of climate (107:18). There needs to be detail somewhere, if not in CPD.

There is an important practical issue. Modern data sets (in for example the various pollen databases) typically record both pollen data D and climate C at each such site. (In practice some estimate of C is often used, as sites do not usually have nearby synoptic meteorology stations; but we do not pursue this here. But they do not record vegetation. This is not a fatal criticism; LPJ-GUESS can fill in the gap, as far as formal inference is concerned. But the inferential chain is longer; and will be manifest is *greater* uncertainty about the underlying climate as well as a computational challenge. CPD needs to know a bit more.

Off-setting this is of course the potential for reduced bias. 'Reconstructions' achieved in this fashion should be more 'honest' about the uncertainties. But they will not, of themselves, be less uncertain than technologies that do not use such mechanistic models.

As I see it, dynamic modelling will properly reflect vegetation dynamics and will thus yield 'better' models. But such models will properly reflect the dampening of the claimed dynamics by the time-response of the vegetation. If it takes 200 years for an Oak forest to respond, such data cannot show climate dynamics at the scale of a decade. More properly stated: Oak changes can - if properly modelled - permit correct probabilistic statements about the uncertainty in decadal climate changes; but those statements will correctly say that there is great uncertainty. Do the authors agree?

They remark (101:4) that this should involve a presence/absence model, but they seem to offer no proposals at all on this.

They need to articulate for CPD the solutions to the problems posed by the many underlying parameters; elsewhere they need to spell these out in detail.

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If dynamic modelling is still to be a distant goal, that's fine. If they claim to have a practical solution, they need to state it, perhaps covering the details elsewhere. How should dynamic vegetation modelling work with dynamic climate modelling? Haslett et al (2006) proposed a descriptive model - smooth in the sense of a random walk with long tails. Can they complement this?

Editorial criticism

The paper is, I regret, almost impossible to read at any level of detail. Indeed, much of my interpretation above is impressionistic. There are two problems: overall structure and loose language.

Structurally it is difficult to know what this paper is 'about'. The title is clear: a few ideas - not a method. The first half of the abstract is also clear: a grand sweep leading to a break from using the present as an absolute reference. So far so good. There is then a distraction: lake-levels and δ^{13C}. The abstract concludes with the big ideas.

The introduction is big ideas again, as is Section 2, until we get to 2.1; suddenly we are into likelihoods and MCMC and details. They are there to provide credentials for some 'findings'. But they only frustrate for 'the method' (104:27) is in fact completely undefined. The findings are in fact presented with no statistical credentials. I suggest an extensive Appendix for technical material.

Section 3 is a complete distraction from the main message.

I can't judge Section 4.

The most important Section 5 - which is most closely related to the title and the abstract - is also the shortest.

At a level of detail, I suspect that many readers will be completely confused; perhaps it's just me, but my experience of refereeing suggests that it's probably them. I instance

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a *very few* of the *very many* things that irritated.

There are very many instances of the mis-use of English. eg 103:20 *its use for climate reconstruction requires to inverse it.* eg 105:25 *we cannot it a linear relationship* Do they mean FIT? (and by the way, who cares if it is 'linear'? do they mean monotone?)

Some things are over-stated; eg 100: 12-16 *The main results are that: (1) pollen alone is not able to provide exhaustive information on precipitation, (2) assuming past CO2 equivalent to modern one may induce biases in climate reconstruction, (3) vegetation models seem to be too much constrained by temperature relative to precipitation in temperate regions.* I'm not the best one to judge here, but I think these are conclusions they have formed. They offer some evidence, but not nearly enough to call these 'results''.

Some things are introduced and never again mentioned; eg 101:4 presence-absence; eg 111:12 particle filter algorithm

Some things are introduced but the explanation is impossible to read; eg 100:9-10 *We show also how it is possible to take into account several proxies measured on the same core (lake-level status...* . I have no idea what 108:5-14 is all about.

Some are confusing; eg 100: 5 *In particular, vegetation models provide outputs comparable to pollen data.* I really don't know what this is intended to mean. I see 'pollen data' as counts.

eg 101: 11 *These relationships are called transfer functions*. Some do call them that; but is this universal usage? I think not.

eg 104:7-8 *As we are more interested in finding a range (or distribution) of possible climate, it is preferable to adopt the Bayesian..* !!! — as though frequency-based likelihood cannot give ranges!! how about Confidence Intervals?

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.

There are many more.

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