Clim. Past Discuss., 2, S539–S544, 2006
www.clim-past-discuss.net/2/S539/2006/

Climate of the Past Discussions

Interactive comment on "Predicting Pleistocene climate from vegetation" *by* C. Loehle

Anonymous Referee #1

Received and published: 7 November 2006

Review 'Predicting Pleistocene climate variation from vegetation' by C. Loehle.

General comments.

The author adresses a problem that has vexed climate scientists and paleobotanistst alike since climate models were applied to model Pleistocene paleoclimates. Modelled climates of glacials/stadials do not agree with paleoclimate data, showing a climate that is warmer than suggested by paleoclimate data derived from paleobotany. Then, either the climate model is wrong, or the interpretation of the fossil evidence is wrong. The author favors the last hypothesis. He advances mainly taxonomic and biogeographical arguments, to support his view that the paleobotanical data indicate a less severe climate than previously assumed. To explain the data-model mismatch he assumes that lower glacial atmospheric CO2 levels should have affected glacial vegetation.

The idea of the CO2 effect as an explanation for the data-model mismatch is not new, it

has been proposed - and tested - before in several papers cited by the author, and also in data-model comparisons (e.g. Levis et al., 1999, J. Geophys. Res. 104:311191-31198; Kageyama et al., Climate Dynamics 17:23-43; Huntley et al., 2003, Quat. Research 59:195-212). The discussion remains mainly qualitative.

The geologic, biogeographical and taxonomic discussion is interesting. In particular for those who are not well acquinted with Pleistocene plant refugia and migration, this might have been a good introduction. However, the arguments are mainly based on rather old literature, and are partly inadequate (detailed below). In general, the paper adresses mainly data from the American continent, although the titele of the paper suggests a wider scope.

Evaluation.

Relevance - the subject is relevant within the scope of the journal Novelty - the conclusions are not new, the hypothesis that low CO2 concentrations have influenced glacial vegetations has been published in several other papers before, with better arguments. The remarks on biogeography and taxonomy build largely on old literature. Conclusions - not substantial. The scientific methods and assumptions are outlined clearly, insofar this is appropriate for a paper that is mainly based on literature research. The results are not always sufficient to support the conclusions. Sometimes the arguments seem rather selective, underpinning by literature references is sometimes poor. The description of experiments and calculations is not applicable here. Proper credit is given to related work. The paper is more restricted in scope than its title suggests. The abstract could be improved by making it shorter. The paper is clearly written and the ideas are represented in a well structured manner, in a well readable and precise language. Presentation of mathematical formulae etc. is not applicable here. The illustrations are inadequate. The paper will be improved if maps of biogeographical features are added to illustrate the authors' arguments. The references are often not adequate and do not represent the latest literature on this subject.

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Although this paper is quite readable, it does not deliver what the title suggests. From the title I would expect either new ideas on this subject or a more extensive review. However, the paper does not present novel ideas or conclusions. In particular on the mismatch between paleobotanical data and the CO2 effect there is a large body if literature that has adressed the problem in a much more thourough way, including modelling and experimental work. The discussion of biogeographical traces of former refuges is interesting. However, it ignores recently published literature on this subject. Furthermore the discussion is practically limited to North America. Major revision will be required to make the contents of this paper more substantial. I expect that it will become more interesting when it concentrates less on the data-climate model mismatch, and more on the biogeographical arguments for a less cold LGM environment and related features arising from the Pleistocene vegetation history. More recent literature should be included.

Specific comments.

Title. As outlined above the title suggests more than the paper really contains. The paper is quite restricted, not only in subject (a few causes of discrepancies between modelled paleoclimates and paleobotanical data), but also in space and time - North America during the Last Glacial Maximum. Also the ideas presented are not really new as detailed above.

Abstract. The argumentation in line 10-19 lists a number of biogeographical anomalies, but it is not directly evident to the non-specialized reader why these are anomalies. This can be skipped from the abstract, or restricted to the most important ones.

Geologic anomalies. The literature cited in this chapter is rather old, the youngest reference being to the review on fossil periglacial phenomena by Péwé (1983). I can hardly imagine that since then no new data on fossil periglacial phenomena has been published. Page 982, line 10-13: there is no one to one correspondence between vegetation ('tundra') and periglacial features. Several of the periglacial phenomena

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mentioned here may occur under a tree cover, e.g. ice wedges and cryoturbation, as abundant literature on Siberian taiga soils testifies. The limited extend of the periglacial zone beyond the glacial limit (page 983, line 1-3) is not a general rule. In Europe a wider periglacial zone is reported on the base of fossil periglacial phonomena (see e.g. Huijzer and Vandenberghe, 1998, J. Quat. Sci. 13:391-417). The distribution of these periglacial features indicate an important mismatch between climate model results (for Isotope Stage 3) indicating a colder climate (Van Huissteden et al., 2003; J. Quat. Sci. 18:453-464).

Taxonomic anomalies. This part focusses on species that are often lumped into wide taxonomic groups, because their pollen are not or difficult to distinguish from each other. It is quite obvious that this should lead to misleading interpretations, even to non-biologists, and does not need a lengthy discussion. Moreover, it does not do justice to many well-founded paleobotanical studies. Often these are not based on pollen only, but also on macro-remains, that are a good indicators of the actual species that are locally present. Climate interpretation in paleobotany is often based on species with a distinct climate range rather than the lumped species groups mentioned by the author. Last but not least, other biota may confirm paleoclimatic inferences (e.g. Coleoptera, Coope, 2002, Quat. Research 57:401-408).

Biogeographic anomalies. This is the most interesting part of the paper. However, the underpinning with data is rather poor. Most of the cited papers are quite old. A quick search provided several relevant papers that would have been worthwhile to mention (e.g. Davis & Shaw, 2001, Science 292:673-679 - the author does cite older publications of Davis; Lewis & Crawford, 1995, Amer. J. Bot. 82:141-149). Next, I also would have expected some illustration by biogeographical maps, e.g. the distribution of endemic species, to illustrate the arguments of the author. Glacial refuge anomalies. Similar evidence exists in Europe (e.g. Willis and Van Andel 2004; Quat. Sci. Rev. 23:2369-2387 and other publications by Willis). Genetic gradients. Page 985, last sentence: 'Almost no species can be found....etc.'. Is this statement supported by liter-

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ature? Are there species known which falsify this statement? Distribution of endemics. Again, most of the references in this part are rather old. Since this part is one of the core arguments of the paper, it should be based on more recent data and literature. Distribution of races and subspecies. The evidence in this paragraph is weak. As the author states, race and subspecies rarely can be distinguished on pollen. If so, the inferences mentioned under 1, 2 and 3 cannot be falsified on pollen data alone. In particular inference 1 - subspecies should have had separate glacial refuges under the 'standard model' - should be falsified on other grounds than pollen data. In the ensuing examples, it is not clear why multiple, separate refuges for subspecies are impossible. For instance, page 989, line 10-11 any literature reference on the genetic variability of Virginia pine northwest of the Appalachians is lacking; page 989, line 21-23: it is not made clear on which grounds the refugia of Sugar and Florida maple should have overlapped on migration to the south.

Discussion. In this paragraph, the causes of data-model mismatch are discussed. This is strongly restricted to only one possible cause - the CO2 effect. There are several other causes that should be considered also. For instance the effect of fire in conjunction with low CO2 (Bond et al. 2003; Global Change Biology 9:973-982), the effects of interannual climate variability that may have been higher during glacials (e.g. Elias et al., 1999; Arctic, Antarctic and Alpine Res. 31:94-98 for an example on Coleopteran fauna), the effects of large herbivores on vegetation (e.g. Zimov, 2005; Science 308:796-797). The discussion on the CO2 effect is followed by a more interesting discussion on the possible sites of northern refuges in the unglaciated parts of North America. This is the most interesting part of the paper, although again no new ideas or data are presented. In page 992, line 20 the author starts a comparison with the situation in Europe. Here, the conclusion that the European situations compares with that is America is drawn too quickly. There is abundant European literature on vegetation reconstruction during the LGM, which is not mentioned at all. Important review work (e.g. by Huntley) is not mentioned. There is a considerable body of literature on plant migration in Europe that is not mentioned; several of these papers would have

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been worthwhile to include in this paper (e.g. Konnert & Bergmann, 1995; Plant Syst. Evol. 196:19-30; Austerlitz et al., 2000, Genetics 154:1309-1321). Likewise, independent evidence on the considerable discrepancy between modelled and observed paleoclimate (e.g. periglacial features) is ignored. Page 990, line 16: as discussed above, pollen data cannot prove anything on the distribution of subspecies.

Technical corrections.

page 983, line 23: Should this be 'sedges are also common in temperate grasslands'?

Interactive comment on Clim. Past Discuss., 2, 979, 2006.