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Climate of the Past Discussions

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

Anonymous Referee #2

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Overview: The author addresses a debate in paleoclimatology as to the nature of the last ice age. On one hand, paleobotanical evidence, especially pollen as noted in this case, suggests that the late glacial maximum (LGM) was extremely cold along the ice margin, and that plant communities migrated southward from the ice sheets into southern refugia to survive these extreme conditions. The opposing argument is from modeling efforts that suggest the LGM climate to be more temperate and equable, and that over large areas modern vegetation types were common and not displaced significantly. Throughout the paper, the author argues for the later and not the former by using biogeographic and taxonomic data, then suggests that the lowering of atmospheric CO_2 may have played a significant role in past (LGM) plant distributions leading to erroneous climate reconstructions. This paper addresses this very relevant scientific question, but not in any way that is particularly new. Unfortunately the author does not necessarily bring new ideas or data to the discussion table. In addition, the cited work appears to be more hand picked then comprehensive with a predominance of older literature (pre-late 90's).

Evaluation: As the other reviewer has noted, the geologic, biogeographic, and taxonomic sections were found to be the most interesting. Again, the citation of significant ideas and the most up-to-date citations seemed to be lacking throughout. For example, it is hard to believe that Péwé (1983) was the last definitive work on potential periglacial features across the region. Also lacking were graphics to define the area of interest and help would help to explain the discussion using genetics, the distribution of endemics, and distribution of races and subspecies. In addition, it is somewhat difficult to pinpoint exactly the area of interest. The title suggests a world-wide explanation, yet the geologic discussion ranges from European mountains, to the western Great Plains, to Pennsylvania and back to Illinois, Indiana, and Ohio. A firm geologic description of the area of interest would go a long way towards defining the focus of the paper. The paper then needs to focus on the reasons for choosing the area. In addition, this focusing should include a shorter and more compact abstract. Below are specific comments directed towards a number of specific areas.

Geologic anomalies: A better discussion of exactly the type of periglacial features expected and those that are reported would help to define the spatial area of interest and set the stage for the coming discussion of the mismatch between data and modeling efforts. In addition, and as noted above, a more recent review of studies would certainly help. It also seems that somewhere an attempt has been made to define the extent of the boreal zone, based on the soils and sedimentary evidence alone. This certainly would provide additional ammunition for arguing that the dichotomies between modeled and paleobotanical reconstructions are in fact significantly different.

Taxonomic anomalies: This section focuses on just two genera, spruce and sedge, but implies they are the primary culprit for resulting cold interpretations of the LGM. The discussion of Picea critchfieldii is appropriate in that the misidentification of a more temperate species in a genera that is primarily boreal in distribution, presents significant

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interpretation problems. In the case of Cyperaceae similar problems occur, however, it is a problem of scale as members of the family Cyperaceae grow over a large and diverse area. Therefore, the presence of the pollen emanating from temperate species is interpreted as reflecting boreal conditions, especially if it is associated with a misidentified boreal conifer such as Picea critchfieldii. In other words, a single pollen type represents an entire family, thus we would expect it to be less diagnostic in terms of climate interpretations. In addition, one of the underlying assumptions with pollen analysis is that the abundance of pollen produced by specific vegetation is static, therefore recovered percentages can be adequately assigned to vegetation types. In addition to other confounding effects of CO2 (to be addressed below), it is well known in the horticultural field that changing atmospheric CO2 concentrations affects the absolute numbers of flowers (Kimball, B.A., 1983, Agronomy Journal 75: 779-788) implies the absolute amount of pollen produced is variable. This effect appears to be species specific, therefore a static vegetation source supply is questionable. It is this type of analysis, where many of the assumptions behind both the paleobiology and modeling results are laid bare that would greatly help the overall paper.

Biogeographic anomalies: This section of the paper provides some of the newest information, with the inclusion of both the use of traditional paleobotanical (pollen) data with the addition of the burgeoning field of genetics. Two primary problems exist, 1) the serious lack of citations to bolster statements about past distributions, especially with regard to some of the newest papers available, and 2) some kind of graphics to illustrate the points being made. In terms of the graphics, there are good digital maps available for most of the historic distributions of species in conjunction with Thompson et. al (U.S. Geological Survey Professional Paper 1650-A, -B, -C). Using these in some form could certainly graphically portray the major points of this section. Similarly, the job would be much easier if the area of interest was better defined and more compact (as suggested above). Throughout the discussion of genetics, great care must be taken not to overstep interpretations of the data. The burgeoning field of microbiology and its application to biogeographical questions uses language that is often confusing

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and hard to decipher. The discussion of races and subspecies is interesting, but again the use of graphics could significantly increase our understanding as the area with and without races and subspecies, a visual comparison, seems to bolster the overall argument.

Discussion: Criticism of the discussion section will be focused primarily on the effects of low CO_2 on vegetation patterns. However, it must be said that many of the previous faults also apply such as the lack of appropriate citations, up to date citations and the lack of graphics. The author points to the effects of lowered CO_2 as a potential driver of vegetation response during the LGM. He points out that C4 grasses have a strong advantage over C3 plants at low CO_2 but this advantage is reduced at low temperatures. He then launches into a discussion of different C3 plant types and the effect of low CO_2 in terms of water-use efficiency (stomata need to be open longer because of low CO_2 therefore greater plant-water loss). He talks about competitiveness in terms of current classifications (e.g. xeric conifers) and how stomatal responsiveness would determine plant distributions. Throughout this discussion, he seems to be lumping broad-based plant groups together as much as he criticized paleoclimatologists for doing the same with pollen data. In doing so, it seems that a significant point is lost. The reduction of CO_2 and its effect on plant physiology is tremendously complex because carbon is the primary building block and atmospheric CO_2 is the primary source for plant life. As plant physiology increasingly nears the carbon dioxide compensation point, where carbon fixed equals carbon respired, any process that reduces carbon acquisition or increases respiration plays an increasingly important role in determining life histories and competitive strategies. Viewed through this lens, lumping plants into broad-based groups, usually based upon current distribution patterns (boreal, xeric, mesic broadleaf, etc.) rather than individual species seems inappropriate.

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