Clim. Past Discuss., 2, S703–S705, 2006
www.clim-past-discuss.net/2/S703/2006/

Climate of the Past Discussions

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

C. Loehle

Received and published: 19 December 2006

First, some general comments are in order about the approach that I took. Currently, there are models of Pleistocene vegetation and climate based on similarity of pollen records to present data, and then there are simulations of vegetation response to CO2. Both of these are models. In my paper I bring independent data to bear on the question of reconstructing Pleistocene vegetation as indicators for climate. These multiple sources of data support the CO2 effect models and call into question the response surface pollen reconstruction approach. In the paper, I have clarified my goals to show more clearly what I am attempting to do. Thus it is "not new" to state that CO2 could have affected vegetation at the Pleistocene, but I used independent data to test and elaborate the details of this general idea. Also, while the idea may be "not new" it has not been fully explored and has not been incorporated into very many climate tests or climate reconstructions. I expand my literature cited to show this. Thus the comment that my article title is misleading is not correct. While the case study is North America,

I am testing which approach to reconstructing climate from vegetation is valid using independent data.

A question is raised about why I focus mainly on eastern North America. Only a single instance is necessary for testing between the two models mentioned above. This region is particularly well-studied, which makes it ideal for illustrating the points I wish to make.

Specific comments are dealt with next.

It is stated that Levis et al 1999, Kageyama, and Huntley et al 2003 proposed "and tested" the CO2 influence idea. Levis does simulate CO2 effects, but such simulation is not a "test" but is rather merely support for this idea. Kageyama mentions a CO2 effect, but does not elaborate on it. Huntley et al. mention reduced water use efficiency (only) but again this is not a "test" but is merely support for the hypothesis of CO2 effects. A test would use independent data to show that the CO2 effect model gives reasonable predictions. This is what I try to do.

It is stated that periglacial features can occur under tree cover. While periglacial features are not inconsistent with forest, I am interested in the opposite case. If there are no periglacial geologic features, it is not reasonable to argue for the presence of tundra. It is stated that a narrow periglacial zone is not a general rule, and a wide zone existed in Europe. I am not arguing that narrow zones were universal, but rather that the reconstruction of a tundra zone based on pollen in North America is erroneous.

It is stated that macro-remains are also used in paleobotanical studies. This is true but does not affect my conclusions. If a plant species is too rare to show up in pollen it is also often too rare to show up as a macrofossil either. Response surface models are usually based on the dominant species in any case, with rare species not giving the model much weight. It is mentioned that other biota may confirm climatic inferences, and I do cite literature on some animal species in the revised ms.

CPD

2, S703–S705, 2006

It is stated that races and subspecies can not be distinguished on the basis of pollen, and thus do not contribute to my hypothesis tests. The races and subspecies being discussed are not paleo but are the present distributions of the races and subspecies. These have been identified by morphology and genetics. The point is that these races and subspecies could not have remained distinct if they had shared a geographic range during the Pleistocene. The reference about Virginia pine is Parker, which I already cited.

It is asked why there could not have been multiple distinct refuges. The southern refugial area in question consists of the geographically unbroken coastal plain of Georgia, South Carolina, Florida and Alabama. Except for fish, there are no real barriers keeping species from moving across them, and they indeed today share a common flora. There is no mechanism to define distinct refuges, as there was in Europe. I clarified this in the discussion.

Regarding the situation in Europe, I have edited my ms to focus mainly on the existence of refugia and how this would be affected by the CO2 effect.

Other effects such as fire and herbivores are mentioned, and I added a comment on this in my revised ms.

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