

Interactive comment on “The last glacial maximum locations of summer-green tree refugia using simulations with ECHAM3 T42 uncoupled, ECHAM5 T31 coupled and ECHAM5 T106 uncoupled models” by K. Arpe et al.

Anonymous Referee #2

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General comments:

This manuscript presents 3 simulations of the Last Glacial Maximum climate with different version of the ECHAM model (the old PMIP1 run, the new coupled atmosphere-ocean run, a high resolution ECHAM5 run).and compares the simulated climates with paleodata (pollen and charcoal, but also the Dead Sea level) for Europe and the Levant. The title of the manuscript is misleading as the results it presents are not only about refugia. In fact, in the introduction, there is no mention of previous work on refugia, neither from the data or from the model point of view. Although examining glacial

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refugia from model results is relatively recent, there already has been some work on this topic which is not cited at all here, e.g. Cheddadi et al, *Global Ecology and Biogeography*, 2006, Svenning et al, *Journal of Ecology*, 2008. Rather, the manuscript presents the 3 simulated climates, from the point of view and surface temperature, atmospheric circulation and precipitation, and then proceeds with the implied distribution of summer green trees, given those climates. A comparison to data is carried out at this stage of the manuscript, but also, more anecdotically, for precise locations, in the simulated climate description. The question about the impact of a higher resolution is also an interesting one. A few papers deal with these topics for the LGM (e.g., Rind et al, but with rather low resolution models, Dong et al, Jost et al, 2005 for results from regional model). No context is given on this topic either.

My opinion is that this manuscript contains interesting results which deserve being published, but that the presentation of the results is not mature enough to allow publication. The introduction definitely lacks any presentation of the literature on glacial refugia and does not clearly present the scientific questions which will be addressed in the manuscript. This is not fair for the results themselves, which merit being put in better perspective. There is also no mention of previous model data comparisons (e.g. Kageyama et al, *Climate Dynamics* 1999, Kageyama et al, *Quaternary Science Reviews*, 2006, Ramstein et al, *Climate of the Past* 2007). On the data side, the authors refer to Peyron et al, 1998, but not to the companion paper by Tarasov et al (*Climate Dynamics*, 1999), which covers the eastern part of the domain examined in the manuscript. A more recent climate reconstruction from the Peyron et al and Tarasov et al data is given by Wu et al, 2007, who compare different methods of climate reconstruction from the same pollen data. Maybe the authors did not want to cite these works, but then, in my opinion, they should really state why they did not consider this data. This is especially important for variables like temperature of the coldest month or precipitation because for these, Wu et al show large differences between the results from the reconstruction methods.

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There has also been quite a lot done on glacial LGM atmospheric circulation, among which Kageyama et al, Journal of Climate, 1999, Laîné et al, Climate Dynamics, 2009, which both review the results of several models, among which the ECHAM3 simulation studied in the present manuscript for the Kageyama et al 1999 paper. The results of the new simulations presented here are not discussed in the light of these past intercomparisons.

One annoying fact is that the authors do not explain how they obtain the summer green tree distribution from the climate results. They point to another of their publications but I believe a manuscript should be as self-consistent as possible and that even if it is not possible to describe the method in great detail, a summary of the method would be good here.

Figure 2 seems to be incomplete (no panel for JJA), some of the figures with many panels are difficult to read, especially when small features, like for the model-data comparison maps, are to be examined.

The manuscript is quite well organised until the conclusions, which are really quite messy. This paragraph should re-state the main scientific questions that the authors initially wanted to address, how they have addressed them and the limitations and perspectives of their study. Furthermore, some figures are not commented very much and, as a result, some sections are only a few lines long. It seems that the manuscript has been written in haste and has not been carefully checked.

I am willing to review an updated manuscript, with, in particular, a better presentation of the context of the research presented here and a good discussion at the end of the paper. This would help probably clarify them. All these suggestions would give more credit to the results presented here.

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