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Climate
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Discussions

Interactive comment on "Simulated northern hemispheric storm tracks of the Eemian interglacial and the last glacial inception" *by* F. Kaspar et al.

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This is a concise GCM study of Northern Hemisphere winter storm tracks during the Eemian interglacial and last glacial inception (125 kyr and 115 kyr BP, respectively). Using different diagnostics, it is shown that the North Atlantic storm track is shifted northwards and extends further to the east during the Eemian, and that this leads to an increased frequency of storm days in NW Europe. These results are interesting and appropriate for publication in Clim. Past. However, the physical interpretation of the results, the discussion of the concept of storm tracks and of the limitations of the model approach requires some further work and several significant revisions of the text (see comments below).

Major comments:

A) The second part of the introduction (starting on p. 1251 line 21) should be improved. It contains some slightly wrong statements and the argumentation is not clear, for instance:

- p. 1251 line 22: here you discuss the impact of radiation on temperature over land. In the next sentence, the discussion is about baroclinic waves (that mainly occur over the oceans) due to temperature GRADIENTS. How do the sentences go together? Do you want to connect radiative changes (due to the changed orbital configuration) to the mid-latitude north-south temperature gradient which is related to baroclinic wave activity? If yes, how does the changed orbital configuration affect temperature gradients? If not, can you clarify the argumentation?

- p. 1251 line 23: "due to ... baroclinic waves ..., present-day winter climate is characterized by ... cyclones and anticyclones". First, cyclones and anticyclones ARE the two "phases" of baroclinic waves, they are not DUE to baroclinic waves. Second, the fact that meridional temperature gradients lead to baroclinic waves is generally true, not only for the present-day climate and not only for winter.

- p. 1251 line 27: what is "high frequent variability"?

- p. 1252 line 3: it should be mentioned that the band-pass filtered geopotential height variance is an approximate measure of cyclone activity that is easy to calculate but that is not very specific, e.g. it does not contain information about single cyclone tracks. Any wave pattern that leads to geopotential height variance on the synoptic scale (e.g. troughs and ridges) contribute to this "storm track" measure. Alternative measures exist (e.g. cyclone tracking algorithms) that correspond more closely to the evolution and passage of low pressure systems but that require more frequent model output and are computationally more expensive (e.g. Sickmoeller et al. 2000, Hoskins and

Hodges 2002, Wernli and Schwierz 2006).

- p. 1252 line 6: what do you mean by "evaluate the ability of the models to simulate storm tracks under different conditions"? How can you check or verify the ability of the models?

- p. 1252 line 11: "eastward shift of the storm tracks": where? In the Northern Hemisphere or globally?

- p. 1252 line 14: the formulation is confusing: did they find an increase in baroclinicity (i.e. meridional temperature gradient) or in storm-track activity? Or in both?

B) I am not convinced that model resolution is not an issue. On p. 1253 line 15 it is stated that "Stendel and Roeckner (1998) showed that ... T30 is sufficient for the representation of synoptic cyclones and that storm tracks are simulated in a satisfactorily agreement with reanalysis data." I do not know this MPI report, but there is ample evidence that resolution does matter and that cyclones have important structures related to fronts and wind gusts that require MUCH higher resolution than T30. Also, a recent study by Jung et al. 2006 indicates that key features of extratropical cyclones are sensitive to resolution, also in the higher resolution range from T95 to T255. This is not to say that geopotential height variance based storm tracks simulated with a T30 GCM are completely inaccurate, but it should be clearly stated that the coarse resolution imposes serious limitations when simulating cyclones and associated precipitation and wind fields.

C) The discussion of the physical mechanisms on p. 1255 should be improved:

- line 1: in line with one of the comments in part A: how does insolation modify the high latitude temperature gradients? Why mainly in high latitudes? Is this statement (line 1) not in disagreement with line 6 ("winter temperatures are mainly influenced by indirect effects")?

- line 7 "reduced insolation ... leads to reduced temperatures over North America": is this a clear causal relationship? If yes, why only there and not also over e.g. Siberia? Is it maybe a more complicated issue, in that the resulting temperatures depend on the radiative forcing but also on the nonlinear response of the atmosphere (meridional heat transport by eddies)?

- line 11 : "due to stronger westerly winds": how do you know?

- line 16: why is sea ice coverage increased with increased insolation?

- line 19: "stronger meridional temperature gradient": where?? Is this a sound argumentation: temperature in increased due to a stronger gradient? Maybe here it would be useful to look at the Eady growth rate that is often used when analyzing storm track dynamics (e.g. Hoskins and Valdes 1990, Knippertz et al. 2000).

D) Fig. 1: It is not clear, at what level the temperature differences are shown! Also (p. 1257 line 28), it is totally unclear what level has been considered for looking at wind speed! Is this the lowest model level? Or is the 10m wind? In the latter case it should be mentioned how this wind is parameterized in the model. Does it correspond to gusts or to a time mean? Again, it should be (briefly) mentioned that with T30 it is not possible to realistically simulate maximum wind speed occurring at surface fronts. On p. 1258 line 4 it is not clear whether the 50 storm days correspond to the 100 year period (so there is on average one winter storm every second year?).

Minor comments:

1) p. 1256 line 8: it would be good to also indicate the magnitude of the relative differences. They seem to be smaller than 10%.

2) p. 1256 line 19: this is not another "case"! In general, it is not very clear why results for SLP and Z500 are shown and how the two fields go together. Maybe for the purpose of this study it would be enough to show the results for SLP (and mention the qualitative consistency with the Z500 results).

3) p. 1256 lines 20-28: here several formulations are too strong: "The main reasons",

"are responsible for", "because of", "caused by": Again, it is very difficult for the reader to see the physical links. If a more in-depth analysis is difficult, then the formulations could be changed in some places to emphasize the consistency of different changes instead of causal relationships.

4) p. 1257 line 7: relative changes seem to be about 5%.

5) p. 1257 line 7ff: are the changes statistically robust? Are the patterns similar if you considered (instead of your 100 yr) two separate periods of 50 yr, or an extended period of 200 yr?

6) p. 1259 line 21: If the model does realistically simulate the temperature field over Europe, this does not necessarily mean that the gradient is also well represented over the oceans, and that the model captures the eddy activity (storm tracks) realistically.

7) p. 1259 line 27: mention that Bengtsson et al. have used a different storm track measure.

Editorial comments:

- p. 1250 line 7 and several other places in the paper: "on the northern hemisphere" should read "in the northern hemisphere".

- p. 1250 line 7: insert "... change in the northern hemisphere winter storm track".

- p. 1251 line 9 and below : change "at mid-month June" to "in mid June".

- p. 1254 line 21: "a interval" should read "an interval".

- p. 1258 line 25: "ration" should read "ratio"

References

Hoskins and Hodges 2002, JAS, 59, 1041-1061.

Hoskins and Valdes 1990, JAS, 47, 1854-1864.

Jung et al. 2006, QJRMS, 132, 1839-1857.

Knippertz et al. 2000, Climate Research, 15, 109-122.

Sickmoeller et al. 2000, QJRMS, 126, 591-630.

Wernli and Schwierz 2006, JAS, 63, 2486-2507.

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