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Climate of the Past Discussions

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

F. Kaspar et al.

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We would like to thank Heini Wernli for his detailed review. Below we provide a reply to all comments.

Referee's 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:

Referee's major comment A1: - 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 midlatitude 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?

Reply: It was probably confusing to mention the changes over land here. The change in insolation strongly depends on the latitude. We clarified this in the text and included an additional figure that illustrates the insolation anomaly as function of season and latitude.

Referee's major comment A2: - 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.

Reply: We changed the formulation according to the comments.

Referee's major comment A3: - p. 1251 line 27: what is "high frequent variability"?

Reply: We changed the formulation and included a description of this variability (frequent passage of cyclone and anticyclones).

Referee's major comment A4: - 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).

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Reply: We included a short discussion of alternative measures and their advantages and disadvantages. This discussion includes some additional references.

Referee's major comment A5: - 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?

Reply: We agree to the comment of the referee that the ability to simulated storm tracks of past interglacial periods can not be directly verified. However, changes in related parameters, e.g. precipitation, could be compared with reconstructed data. Such data is currently in preparation, but was not yet available for use in the paper. We adapted the formulation.

Referee's major comment A6: - p. 1252 line 11: "eastward shift of the storm tracks": where? In the Northern Hemisphere or globally?

Reply: In the Northern Hemipshere. Formulation was adapted.

Referee's major comment A7: - 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?

Reply: They found an increase in baroclinicity for the storm track regions. Formulation was adapted.

Referee's major comment 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

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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.

Reply: We included a short discussion on resolution and chose a more careful formulation. The comparison of the simulated preindustrial storm tracks with ECMWF reanalyis data shows that the storm tracks are simulated reasonable. An additional statement is also included in the summary.

Referee's major comment C: The discussion of the physical mechanisms on p. 1255 should be improved:

Referee's major comment C1: - 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")?

Reply: Some comments on this and a new figure have already been included into the introduction. We included additional comments here: The changes depend on latitude and season and can be amplified by feedbacks. The change in insolation is stongest in the high northern latitudes during summer. The related changes in sea-ice persist in winter.

Referee's major comment C2: - 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)?

Reply: We included comments concerning the differences between North America and Siberia. Over North America southward advection of polar air enhances the effect of

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the reduced insolation.

Referee's major comment C3: - line 11 : "due to stronger westerly winds": how do you know?

Reply: This topic was analysed in another paper. We included the reference.

Referee's major comment C4: - line 16: why is sea ice coverage increased with increased insolation?

Reply: Again, this is an effect of the change in the seasonal distribution in insolation: Winter insolation is increased, whereas the increase in sea-ice coverage is caused by decreased summer insolation and persists in winter. We clarified the formulation.

Referee's major comment C5: - 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).

Reply: As before, this is a question of seasons. Insolation is reduced in summer, resulting in a stronger meridional temperature gradient. This leads to an enhanced North Atlantic current (also as annual mean), which in turn leads to increased winter temperatures in the North Atlantic. The formulation has been clarified.

Referee's major comment D1: 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.

Reply: The requested information has been included into the text, together with ad-

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ditional explanations how 10 m wind is calculated in the model. According to Erich Roeckner (MPI f. Meteorology; personal communication) the 10m wind is interpolated between the lowest model level and the surface according to the logarithmic wind profile based on a modified roughness length. This modified value is the one that would create in the neutral case the same drag coefficient as obtained with the roughness length and the stability conditions of the atmosphere computed at the respective time step.

Referee's major comment D2: 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?).

Reply: It is the number of storm days per winter season. This has been clarified.

Referee's minor comment 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

Reply: A comment has been added to the text.

Referee's minor comment 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).

Reply: As insolation directly influences surface conditions, we analysed the storm tracks based on SLP. In addition to that we analysed them in 500 hPa to test if the signal persists at higher levels. A comment on this has been included. As referee 1 regarded it as a strength of the manuscript to see the different diagnostics we decided to keep both results.

Referee's minor comment 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

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is difficult, then the formulations could be changed in some places to emphasize the consistency of different changes instead of causal relationships.

Reply: We have chosen our formulations more carefully according to these suggestions.

Referee's minor comment 4: p. 1257 line 7: relative changes seem to be about 5

Reply: A comment has been included.

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

Reply: We performed an additional analysis for two additional intervals and added some comments on this. In case of the GI simulation the results are not robuste in some regions, which is mainly due to the long-term expansion of the sea-ice.

Referee's minor comment 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.

Reply: We have chosen a more careful formulation.

Referee's minor comment 7: p. 1259 line 27: mention that Bengtsson et al. measure.

Reply: Comment was considered. We also mentioned results of an additional recently published paper (Pinto et al, 2007).

Referee's editorial comments:

Reply: All editorial comments were incorporated.

Interactive comment on Clim. Past Discuss., 2, 1249, 2006.