

Interactive comment on “Simulation of land surface temperatures: comparison of two climate models and satellite retrievals” by J. M. Edwards

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I thank the reviewer for reading the paper and suggesting improvements. Specific responses to the individual points follow.

Replies to Specific Comments

A The models do not explicitly distinguish between cloud and fog, so that fog is represented as cloud in the lowest layer. Consequently, grid-points with fog fractions larger than 10 % are counted as cloudy rather than clear. To clarify this point in section 2.2 I propose adding the text (*also rejecting foggy grid-boxes*).

B I agree that the original text was not so clear as it might have been. The satellite
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and model data are processed independently (method 2 in the referee’s terminology). Though the instants at which a particular location is clear in the satellite record and the model simulation do not match, the assumption is that the conditions under which the model and satellite data count as clear are *statistically* similar, so that the statistical comparisons presented here are valid. The analysis of consistency in section 2.3 is partially motivated by this question. Here I would propose adding the following text at the end of section 2.2. *The sampling procedure is applied independently to each model and is completely independent of the satellite data. It is then expected that the resulting climatological composites will be statistically comparable with the satellite data. This assumption is now investigated by considering the various sources of error in the data and the degree of consistency which should be expected between the retrieved and modelled data.*

C I will address the issues of the sampling period and the land surface scheme in turn.

On the subject the sampling periods (1983–1998 for the models and 1996–2000 for the satellite data), observed temperature trends imply differences that are small relative to the differences between models and observations. Section 2.3 of the original version did conclude with a comment about trends in near-surface temperatures, but did not specifically mention trends in the diurnal range. Perhaps the most relevant reference here is to the work of Karl *et al.* (1993) who discuss trends in maximum and minimum temperatures and the diurnal temperature range (DTR) over the period 1950–1990. For the USA, they found the largest trend in the diurnal temperature range during the autumn, amounting to -3 K/century. Since the difference between the mid-points of the periods of the satellite and model data is 7 1/2 years, we might expect the modelled DTR to be larger by 0.23 K. This is quite small compared to the differences between the modelled and retrieved temperatures. Two further comments should be made. Firstly, historical analysis of trends is restricted to air temperatures rather than

skin temperatures and the trends in skin temperature might differ in magnitude. Secondly, Karl *et al.* (1993) suggested that changes in cloud cover were the most likely cause of the decreased DTR, whereas the results presented here are for clear-sky conditions. I would propose adding a reference to Karl *et al.* (1993) and mentioning that they discuss the diurnal range.

Regarding the differences between the models, as the reviewer perhaps suspects, I believe that differences between the two soil schemes are likely to be the most important contributors to the differences. In the original version the first sentence of the last paragraph of section 4.1 read, "*The results are suggestive of differences in the surface schemes of the two models.*" This could be strengthened to read "*These results suggest that differences in the land surface schemes of the two models probably play an important role in explaining the differences in behaviour between them.*" However, the two land surface schemes are quite different and without running them outside their host GCMs with identical forcing it is difficult to be categorical about this, so I do not feel that it would be justifiable to strengthen the remark beyond this. Difference of resolution I expect to play a smaller factor.

- D I agree that it would be useful to have formal error bars on the figure, but I am not sure that the available information on the accuracy of the climatology can be expressed in the form required here. New *et al.* (1999) discuss the accuracy of their procedure by considering the square-root of the generalized cross-validation (RTGCV). As I understand it, this is essentially a measure of the consistency of fitting within the data selected and thus does not fully characterize the possible sources of error. They also present comparisons with other climatologies (notably their Figure 22), which often shows differences in excess of the RTGCV. The issue is complicated, but I do take the point about its importance. Rather than adding error bars to the figure, after referencing New *et al.* (1999), I suggest adding the sentence "*This paper should also be consulted for discussion the accuracy of the climatology and comparisons with the possible alternatives.*" I hope this will alert

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readers to the need to bear errors in mind, while leaving them free to form their own judgement on this difficult question.

- E The reviewer raises the question of the effect of valleys and the surface scheme. Regarding the effect of valleys, surface temperatures are certainly very sensitive to elevation and at the comparatively coarse resolution of the models the detail of the orography cannot be represented perfectly. All other things being equal, one would expect the impact of valleys to be most prominent in mountainous areas to the west, but the systematic differences between the models and the observations are broadly similar between the mountainous west and the flatter eastern areas (regions M and P). Turning to the question of the surface scheme and the physiographic fields, as indicated in response to point C, I strongly suspect that these are very relevant to the differences between the models (and with observations). However, as noted in reply to point C, without isolating individual components of the models it is difficult to make categorical statements about this.
- F Both models show more rapid cooling than the retrievals later in the night, so I would indeed agree that if the summer nights were longer there would be better agreement in the minimum temperatures. This could be interpreted as the nocturnal conditions attempting to correct for the biases during the day. I would regard the most significant errors in the models in July as being in the latent heat fluxes because of summer drying.
- G As in reply to point D I agree that a formal error bar would be useful, but again, I am not sure that the available information on the accuracy of the climatology can be expressed in the form of a precise error bar. (Generally, estimating the accuracy of climatologies is difficult and this is often attempted by comparing different climatologies.) Huffman *et al.* (1997) do indeed discuss error estimates for the GPCP climatology, but their discussion is based on random errors. As an alternative, I have plotted equivalents of figures 9, 11 and 13 using the alternative

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CMAF climatology to confirm that the signal is not an artefact of the climatology. I would suggest adding to comment, "It may also be noted that comparison with the alternative CMAF climatology of precipitation Xie and Arkin (1997) shows the same features and that the two climatologies agree well over the US (Yin et al. 2004)."

1 Replies to Technical Corrections

- a.1 I agree that using letters to refer to panels of figures would be easier for the reader and would propose making this change in a revised version.
- a.2 I agree and would clarify the caption.
- a.3 Tick marks can be inserted at 3-hourly intervals.
- b.1 The form "Fig." can be used throughout.
- b.2 I propose adding a table detailing these characteristics of the data to section 2.
- b.3 With the introduction of letters for panels of the figures, the reference can be made more specific by referring to "Fig. 1(b)".
- b.4 On reflection, I think the term "temperature difference" is more appropriate and suggest that the figure has be relabelled. I think it may also have been confusing that the differences in this figure were shown relative to a cloud-clearing tolerance of 5 %, whereas 10 % is the standard value, so I propose replotting relative to a value of 10 %. Most significantly, I have have expanded the discussion of this figure in the main text. I also propose expanding and clarifying the text at this point.
- b.5 I agree and propose to move the words "model - retrieval".

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- b.6 This is less obvious than it should be since GAMDT in this context is a reference. I propose using the form "the paper of GAMDT (2004)" to clarify.
- c.1 This can easily be corrected.
- c.2 The missing author's name will be inserted.
- c.3 The year will be corrected.

2 References

References

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- Xie, P. P. and Arkin, P. A.: Global precipitation: A 17-year monthly analysis based on gauge observations, satellite estimates and numerical model outputs, *Bull. Amer. Meteor. Soc.*, 78, 2539–2558, 1997.
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