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Discussions

Interactive comment on "Role of land surface processes and diffuse/direct radiation partitioning in simulating the European climate" *by* E. L. Davin and S. I. Seneviratne

Anonymous Referee #1

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General comments:

This paper summarizes the impact of (1) land surface versus atmospheric model improvements and (2) a direct versus diffuse radiation scheme on surface temperatures and the surface radiation balance. The paper addresses relevant scientific questions for the scope of Biogeosciences, however, the current manuscript is lacking sufficient discussion of the results. The authors conclude that the direct/diffuse percentage can account for up to 3% of the temperature variability in the model, and the significance of this number with respect to the model internal variability should be discussed. This paper would be suitable for publication after the inclusion of some additional discussion

and analysis, including a quantification of the role of diffuse light versus the internal variability of the model, a deeper discussion of the model evaluation versus ground-based observations, and an expanded discussion on what happens at the surface with the addition of diffuse light. Detailed suggestions are provided below.

Major comments:

1. The authors need to address the internal variability of the COSMO-CLM. What is the internal variability of these simulations and is the inclusion of the diffuse fraction within the scope of the internal variability of the model? This does not necessarily require additional simulations; the authors could cite other literature for this specific model.

2. The model evaluation of the diffuse light fraction is too brief and requires more detailed explanation. A few points of discussion needed in Section 3.2.1: (a) The sites presented for model evaluation are located at high latitudes, where one might expect the amount of diffuse radiation to be higher and its impact more important – e.g., a total incoming radiation amount of 300 W m-2 as in Figure 7 is exceptionally low. Can any sites in the southern part of the domain be located for evaluation? (b) The Carpentras site is often biased high – is this correlated with cloud cover discrepancies? (c) In contrast, the Peyerne site shows improved agreement, and an explanation for these higher values is needed.

3. Section 3.2.2: The sensitivity of latent heat to diffuse light is presented for spring and summer data. The authors should discuss the seasonal cycle of latent heat and its components (ground, canopy, transpiration) to understand if this effect is merely a function of ground evaporation or if they can attribute it to transpiration. Also, the representativeness of the Hyttiala site for the full model domain should be discussed (e.g., ecosystem type, canopy type, etc.).

4. The model discussion section on the mean climate state (Section 3.2.3) is also too brief and filled with gaps. Suggestions to improve this section: (a) Figure 8 is a confusing mix of absolute values (Figure 8b) and percent changes (Figure 8c), which

make it difficult to tell how much of the latent heat change is due to transpiration; (b) the impact on surface temperature (Figure 9) is very difficult to identify as presented in side-by-side figures; these differences should be clear in the figures and potential mechanistic explanations should be provided in the text; (c) the surface temperature changes in Figure 9 should be clearly identified and discussed with respect to earlier conclusions. For example, Scandinavia (where the model evaluation is occurring in Figure 7) shows a broad increase in transpiration, yet shows a marked difference in surface temperatures with cool biases up to 2K over Norway and warm biases up to 2K over Finland; (d) warm biases in the Mediterranean are very large, yet this represents another region where evapotranspiration increases in the model.

5. Conclusions, lines 13-15: The authors state that the diffuse inclusion can affect the level of partitioning between sensible and latent heat, yet they do not actually show this in the paper. They show that there is a slight increase in the evapotranspiration and do not show or discuss the partitioning in any detail. Either this should be added to the analysis in Section 3.2.2 or be removed from the conclusions.

Minor comments:

1. Model description: The authors mention the convection scheme used (Tiedtke) but I'm assuming that at the resolutions simulated ($\sim$50km) that some type of representation of large-scale precipitation is needed.

2. Page 11610, lines 13-17: At the end of this section, the authors state that the bias corrected by the LSM is the same amount as the change in atmospheric components – can this be quantified in the text to support the figures with domain averages? The authors focus on the improved biases north of 50 degrees, and that biases of the opposite sign often increase southern part of the domain (below 35N).

3. Page 11615, line 9-10: does the use of the word "significant" refer to "statistically significant"? If not, then the authors should change their wording. I would disagree with the use of this term here, because the changes are barely outside of the standard

deviation and are at exceptionally low light levels.

4. Figure 4 seems redundant and unnecessary, or perhaps should be included as supplementary material.

5. Page 11614, line 6: change "evidences" to "evidence"

Interactive comment on Biogeosciences Discuss., 8, 11601, 2011.