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Hydrology and Earth System Sciences Discussions

Interactive comment on "Inter-comparison of two land-surface schemes applied on different scales and their feedbacks while coupled with a regional climate model" *by* F. Zabel et al.

Anonymous Referee #2

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Title: Inter-comparison of two land surface schemes applied on different scales and their feedbacks while coupled with a regional climate model

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Recommendation: [Major Revision]

GENERAL COMMENTS:

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

This article presents an intercomparison of two land surface models (LSMs) coupled with one regional climate model (RCM). As explained by the authors the LSMs are either unilaterally coupled to the RCM, meaning that the RCM drives the LSM without being affected by its response, or bilaterally coupled, meaning a full interactive integration with information flowing back an forth between the atmospheric component of the RCM and the LSM.

The two LSMs that are compared are totally different. They are operated at different horizontal resolution, they use different vegetation parameters, they use different hydraulic parameters, etc, hence they appear to be different in every aspect. There is a "simple" LSM, the NOAH-LSM, which is an integral part of the MM5-RCM, the whole system being operated at the same horizontal and temporal resolution. Then there is the "complex" LSM, PROMET, which, in this paper, is employed at much finer resolution than the atmospheric component of the RCM, thus an additional tool is required to downscale atmospheric information and upscale the land-atmosphere exchanges.

In my opinion, it is a worthwhile exercise to mutually compare the performance of two land surface models (LSMs) when operated in a similar setting. The problem with this manuscript is that the two LSM are so different that it is very difficult to draw useful and non-trivial conclusions. Moreover, it turns out that the comparison is entirely dominated by huge differences in the arranged set up of the LSMs: 1) PROMET has the option of characterizing a pixel as impervious or sealed, which largely suppresses latent heat flux in urban and mountainous regions. The employed version of NOAH-LSM lacks this option. 2) the formulation of water stress in both LSMs is also radically different, resulting in large differences in evapotranspiration during the summer season in regions that are sensitive to drying out. Thus, the found differences do not come as a surprise, however since the model results are not evaluated with observations we don't which of the two approaches is most skillful. It also seems to me that the differences between the two LSMs are so drastic that they obscure the role of horizontal resolution, which in this paper is used to shed some light on the role of small-scale heterogeneity

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of the land surface characteristics.

I suggest the authors carry out additional sensitivity studies in order to distinguish between the roles of the two parameters I mentioned above and horizontal resolution. For example, I think it would be quite useful to know for the atmospheric community to see whether it is feasible to modify the setup of NOAH-LSM in such way that it is capable of producing similar results as PROMET, at least qualitatively.

Another major point I want to raise is that the authors claim to have carried out, as they call it, unilateral and bilateral integrations. Or, in other terms they refer to 1-way and 2-way. My point is that a pure 1-way coupled integration is not possible since in preparing atmospheric drivers the RCM can not be operated without being coupled LSM, simply because the RCM needs boundary conditions at the surface. In particular when the various LSMs are very different it is hard to interpret the result. What is missing in the paper is the "other" 1-way coupling, i.e. the MM5-atmospheric part 2-way coupled with PROMET forcing the standalone version of NOAH-LSM. Including results from this integration would certainly help the interpretation.

OTHER POINTS

Abstract, line 7: rephrase ". . . which evolved from . . ." as ". . .". . . which developed from . . ."

Abstract, line 10: rephrase "Both use different . . ." as "They use different . . ."

Abstract, line 10/11: rephrase ". . . and different spatial scales . . ." as ". . . and are applied at different spatial scales . . ."

Abstract, line 14: rephrase "Used with same atmospheric drivers . . ." as "Forced with atmospheric information from the same regional climate model . . ."

Abstract, line 15: rephrase " . . ., the model differences . . ." as " . . ., the different treatments of land surface processes . . ."

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Abstract, line 20 (but also in the rest of the paper) I suggest to replace “bilaterally” by “interactively”, and “unilaterally” by “off-line”.

Introduction, p 7093, line 24: “ . . . a more detailed view . . .”. A view of what?

Introduction, p 7093, line 26: spell out the acronym PILPS

Introduction, p 7093/7094: Is there a conceptual difference between land surface schemes (LSSs) and land surface models (LSMs)? If yes, please explain; if not, I suggest to stick to LSMs.

Introduction, p 7095, line 2: Explain what is meant with “over-parameterization”. Level of complexity? Level of detail? Number of parametric assumptions, and thus number of required closures?

Introduction, p 7095, line 6: What type of feedbacks are the authors referring to?

Introduction, p7095, line 26: I suggest to replace “analyzed” by “examined” or “investigated”.

Study area, p 7096, line 15: What the authors refer to as the Atlantic Ocean is in fact the North Sea

Land surface schemes, p 7097 and further: There is no reference in the text to the equations. Also subscripts are sometimes present in the text, but missing in the equations, e.g. σf in Eq. 2,3, and 4, but σ_f in the text. This is confusing. See also Rcmin in Eq. 6, but R_{cmin} in text. Likewise Eq 9.It took a while before I realized that a\psi had to be read as a_\psi. Please check thoroughly.

Land surface schemes, p 7097, line 16: \beta and Wc are both described as contents but they have different units. This is confusing. Denotes \beta a ratio of mass numbers or volumetric numbers?

Land surface schemes, p 7101, Eq. (7): I presume the normalization factor in the denominator should be (dz1+dz2+dz3) instead of (dz1+dz2)

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Interactive Comment

Land surface schemes, p 7102, line 8: What measure of water vapour is meant? Pressure, mass, specific mass?

Coupling approaches, p 7104, line 6: The acronyms REMO, CLM, CRCM referring to other regional climate models (RCMs) should either be spelled out or not be mentioned.

Coupling approaches, p 7104, line 17: The words “bounded” or “confined” are preferable over “restricted”

Coupling approaches, p 7104, line 19/20: I would say the reverse applies as well, within the 2-way coupling mode any biases of the LSM are inherited by the atmospheric component of the RCM. Please, mention so.

Coupling approaches, p 7104, line 23: “longwave” instead of “logwave”

Results and discussion, p7106, line 9: “Figure 8” instead of “Figure 7”

Results and discussion, p7108, lines 15-20: The discussion in this paragraph is hard to follow. The time series plotted in Fig. 11 is not useful, I suggest to plot a frequency distribution of daily evapotranspiration restricted to the spring and summer months. There should also be plotted a result from a MM5-NOAH-LSM integration but I don’t see it. Please clarify this paragraph.

Results and discussion, p7109/7110: I recommend putting sections 5.2 and 5.3 in one section, because the feedbacks to the atmosphere and feedbacks to the land surface can not be separated. E.g. at the end of section 5.2 (Feedbacks to the atmosphere) there is a sentence on effects on precipitation which is also a feedback to the land surface. The reverse applies to section 5.3 when evapotranspiration is discussed.

Conclusions, p 7111, line 9: “ . . . behaviour of each could be adjusted . . .”. A word or words seem missing after “each”.

Conclusions, p 7112, line 4: I don’t think using the word ideologies is appropriate. Better use “lines of approach”.

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FIGURES: the legends in some of the figures are really too small to read, they should be made much larger. This applies in particular to Figs 3, 6, 7, 13, 14, and 15.

Figure 1: I found figure 1 unclear, in fact very confusing. The reference version of MM5 contains NOAH-LSM, hence the upper block should contain “atmospheric component of MM5”. Also –as I pointed out before - the authors have not carried out an integration with NOAH-LSM forced with atmospheric information from RCM{atmospheric part MM5 2-waycoupled to PROMET}, which, by the way, they should have carried out - as I have argued before. But for the current manuscript the NOAH block on the right is obsolete. I also don’t understand the red coloured lines.

Figure 4: The meteorological drivers REMO and CLM are not explained (see my comments (p 7104, line 6) suggesting what to do.

Figs 11 and 12: Plain time series are not informative.

Figs. 14 and 15: According to the text the RCMs (either coupled to NOAH or to PROMET) are always ran at 45 km resolution. Yet, the surface air temperature and precipitation, both being atmospheric parameters, of the RCM simulation 2-way coupled to PROMET is plotted at high resolution (1km according to he caption). I presume the results shown are processed by SCALMET. If so, please mention. If not, explain.

Fig. 16: The quantity plotted is actually not the “water stress”, but the root water uptake reduction function.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7091, 2011.