Geosci. Model Dev. Discuss., 4, C615–C618, 2011
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Geoscientific Model Development Discussions

GMDD

4, C615–C618, 2011

Interactive Comment

Interactive comment on "Use of agricultural statistics to verify the internannual variability in land surface models: a case study over France with ISBA-A-gs" *by* J.-C. Calvet et al.

Anonymous Referee #1

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This paper presents a novel method of land surface model benchmarking whereby agricultural statistics are used to verify the interannual variability of simulated vegetation biomass. This is an interesting study making use of, potentially, relevant annual observations of vegetation biomass. I recommend that the paper is accepted following attention to the comments below.

General comments:

1) Clarification of objectives.

Because the model used in the study, ISBA-A-gs, does not include any crop-specific parameterisation, the authors are assuming that the processes that govern variabil-

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ity in the yield of crops are the same as those that govern variability in unmanaged vegetation biomass accumulation. This assumption should be stated more clearly in the paper. Crops and grasslands are photosynthesising vegetation biomes so this assumption is clearly not completely invalid. However, crops have been chosen, bred and are managed, to produce high yields of grain, at optimum quality, with optimum use of resources. Therefore, there are several reasons to expect that variability in yield may not be perfectly correlated with the annual maximum biomass of "natural" vegetation. Given this discrepancy between the nature of the simulated and observed variables, the stated objective of the paper is "to assess to what extent this information [agricultural statistics] can be used to validate a generic LSM". This is a worthy aim but I do not think it is answered in this paper. At the beginning of the Conclusions section the authors state that "agricultural statistics were used to assess to what extent the ISBA-A-gs land surface model is able to reproduce the interannual variability of the dry matter yield". I think that the difference between these two statements is important and needs clarification to improve the clarity of the paper. The second statement is much closer to what was done in the paper. I suggest that the initial objective is rewritten as it is not possible to answer without a second, independent, observational data set of vegetation productivity which is more directly comparable to the modelled variables.

2) Grasslands v. crops

The authors show that significant correlations are found between simulated vegetation biomass and crop/grassland yield in some regions. A major conclusion is that the variability of grasslands are better captured then croplands. Is this surprising given the lack of a specific crop representation in ISBA-A-gs, or is it because the grassland observations are in fact simulated values themselves and that the STICS formulation could be similar to that in ISBA-A-gs? A more thorough discussion of these points is needed. Also, I was astonished to see in the Conclusions that the authors think that "the model could probably be improved by representing managed grasslands", despite finding that "a striking result is the excellent scores obtained for managed grasslands".

Surely, this would argue for concentrating on improving the representation of crops, not grasslands?

3) Justification for the optimisation of MaxAWC.

In my understanding, MaxAWC is a property of the plants and soil and so I expected a value to be determined a priori from datasets such as ECOCLIMAP. In fact, the final paragraph of page 1481 seems to suggest this is possible. Instead, the authors use this parameter to tune the model. The same simulations are then used to evaluate the model performance. Ideally, any tuning should be done independent of validation.

4) Clarity of benchmarking protocol

The paper fails to completely deliver on the GMDD evaluation criteria that in benchmarking papers it should be possible for the protocol to be precisely reproduced for an independent model. The description was mostly good but it was not clear to me how the aggregation of 8km simulations within each département unit was performed. Perhaps adding to my confusion is that the circles in Fig 1 (left) are referred to as SAFRAN grid cells in the figure caption, but as départementes in the text (page 1483, line 21).

Specific comments:

page 1480, line 10: "verification of the hypothesis made in SURFEX on the value of ÂĚ" You do not mean hypothesis. You are evaluating the default choice of parameters in the model.

page 1480, line 14: why did you not also consider maize?

page 1481: it would be nice to be able to see the drought parameterisation.

page 1486, line 17: why was the preliminary parameter sensitivity study only performed for one site?

page 1487, line 1: the lack of sensitivity to ?c could be because you kept MaxAWC at 120. I'd expect that the optimised values of these parameters are correlated.

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page 1486, line 10: "the main factor", can you present any justification for this statement.

Technical comments:

page 1478 line 9-13: this very long sentence needs re-writing

page 1479, line 17: Smith et al (2010a,b) is not in reference list.

page 1489, line 11: "optimized two" should be "two optimized".

Table 2: add some vertical lines to aid reader (e.g. between Pea and Grasslands)

Interactive comment on Geosci. Model Dev. Discuss., 4, 1477, 2011.