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Interactive comment on "Implementation of dynamic crop growth processes into a land surface model: evaluation of energy, water and carbon fluxes under corn and soybean rotation" *by* Y. Song et al.

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The paper describes a detailed crop model for corn and maize, implemented in a land surface model ISAM, calibrated using field data on a given location and additionally validated with data from another site. It explores the impact of allowing a dynamic root allocation system.

The paper is organised well (the set-up of using a narrative thread in the main text and the equations and parameters listed in tables is useful), but brings across a mixed set

of messages, who are not all equally valuable. The evaluation of the (detailed) model does not give a lot of information about what is gained by the complex parameterizations, as a clear benchmark is lacking. A lot of the results shown in figs 1 and 2 can probably also be found using a statistical regression model (see Abramowitz et al, 2008, for a nice demonstration of the added value that is actually added by a model to the information that is already contained in the forcings). A second point that needs additional attention is the seemingly large bias in the sensible heat flux. Details on how the energy balance is solved in this model are not given, but I assume that the model preserves energy and that the mismatch in sensible heat is compensated by a large mismatch in soil heat flux, and thus that there may be a problem in partitioning heat between soil and atmosphere. That is not clear from the discussion and the treatment of the observations/model outputs. The part that is definitely useful is the dynamic root allocation, that shows a large potential for drought mitigation, which indeed is not included in many state-of-the-art LSMs.

I would suggest to reorganise the manuscript by focusing on this root allocation procedure, where you can define a clear benchmark experiment by comparing the two strategies and calculate the statistical significance of the difference between the two simulations. Material on the model & experimental design can still be included in the manuscript but is not a goal per se but a tool to make the case of the dynamic root allocation. This has consequences for the structure of the paper, its title and the emphasis of the analyses and discussion.

Specific remarks

Although the English is ok, it has a lot of Asian influence (missing "the", "a", . . .). Please have it corrected by a native speaker

9902-22: the Smith et al (1976) is an old reference. Is there newer literature that supports their findings?

9905-22: to a non-agronomist "silk emergence" is not a clear term. Please explain

Eq 1: the storage term is a bit strange here: it depends on the time scale how large this term is: at the seasonal time scale S should be zero: what is stored has to come out eventually. But in your equation S seems to be a systematically positive term (S $\sim$ Rn and the mean of Rn > 0) which is physically not consistent. Please discuss the time scale issue of S

Eq 2: please discuss the implication of Wilmott's metric before presenting the equation (move 9910-10 to 14 upward). Does this metric subtract the climatological cycles (seasonal, diurnal) before evaluating the skill? Otherwise high skill can already be obtained if the first order cycles are represented, which is not difficult to achieve

The reason why S1 and S2 are placed in a Supplement is not clear to me. Why not included in the main text? Please reconsider when the structure of the paper is revised

9913-13 (and more places): replace "daily pattern" by "seasonal cycle"

Fig 1: please add the notion that the left two columns are calibration data, and the right two columns are validation experiments. And explain how the "growing period" is defined.

Abramowitz G., R. Leuning, M. Clark, and A. J. Pitman (2008), Evaluating the performance of land surface models, Journal of Climate, 21, 5468-5481.

Interactive comment on Biogeosciences Discuss., 10, 9897, 2013.