

Interactive comment on “Crop physiology calibration in CLM” by I. Billionis et al.

Anonymous Referee #1

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Billionis et al. present a detailed description of a new calibration procedure for parameters within the CLM 4.5 crop model. They apply the procedure to estimate values for 10 poorly constrained parameters for soybean at a single site in the U.S.A. In a test with artificial data, their calibration method is able to reproduce the parameters used to generate that artificial data, and the calibration improves the performance of the crop model at the site for which it was calibrated.

Unfortunately, I find myself insufficiently qualified to assess the quality or the novelty of the mathematics underlying section 4 of the manuscript. I have therefore restricted my review (with the exception of a few typos) to other aspects of the manuscript. As section 4 represents the core of the method, my review should therefore only be considered in addition to others in which this section can be properly assessed. Nonetheless, I find some concerns over the manuscript in its current form, which I detail below, and which I believe should be addressed before publication. Overall I find the standard of

C2585

language is satisfactory, and the paper fits within the scope of GMD.

Major comments

As a demonstration of a method to calibrate a site-scale model I suspect that the manuscript has a great deal of merit. But as a method to calibrate a large-scale model it is lacking in details of how the method should be applied to calculate a generally applicable parameterisation. It is mentioned in the discussion that there is insufficient data to determine globally relevant parameters. I accept that this is probably the case, but one must question the logic behind developing a calibration procedure for a large scale model that relies on non-existent data in order for it to be applied at the scale the model is intended to operate at. How then can the calibration procedure be used to improve CLM performance? At the very least the authors should make an evaluation of whether the parameters derived for this single site improve the simulation of soybean at other sites in North America. At minimum, data on yield should be available.

Regarding the choice of calibration parameters. It seems strange to calibrate to NEE, when NEE also strongly depends on heterotrophic (i.e. soil) respiration, which is largely out of the remit of the parameters being calibrated. By including NEE in the calibration there is a risk of parameterising the crops to account for deficiencies in the soil scheme.

Presumably the level of re-translocation of carbon from leaves/stems to grain is dependent on nitrogen availability (if not then references should be provided). If so, then the fitting of `fleafcn` and `fstemcn` means that the fidelity of the calibration is conditional on the nitrogen availability in the soil. Such effects are referred to in the results and discussion sections, but to me it seems strange to calibrate these parameters at all when observations covering a range of nitrogen availabilities are not available – it once again appears to go against the principle of developing a general parameterisation for a large-scale model.

Minor comments

C2586

The abstract should provide a description of the results also.

In the abstract and on pg. 6735, l. 27, it is claimed that the calibration will improve “projections” of plant development and carbon fluxes. This terminology is a bit loose, as it seems to imply long-term (e.g. 100 year) projections under changing climate, atmospheric CO₂ mixing ratios and nutrient availability. It is clear that for environmental conditions similar to the calibration conditions, the calibrated model would be expected to perform better. However, it is far from clear why a calibration to current conditions should actually improve projections for conditions that lie outside the range of the calibration conditions. The terminology should either be altered, or better still, this point should be discussed.

p. 6735, l. 19: It is not defined what is coupled or uncoupled.

p. 6737, l. 9: Presumably “fits within the framework of natural vegetation” means that all the processes apart from the allocation scheme are treated exactly the same for crops as for natural vegetation?

p. 6740, l. 19: “Level 4” will be meaningless to most readers. Suggest to remove or explain.

Eq. 3 appears to imply the calculation of a slope for GRAINC (z3), however this is not mentioned in the accompanying text.

p. 6752, l. 15: Over precision of reported numbers.

p. 6752, l. 20: Timings would be more useful if accompanied by a number of cores and a CPU clock speed.

The text appears to imply that stemcn is calibrated, but it is not listed in Table 1. Is livewdcn the same as stemcn? If so, nomenclature should be consistent throughout.

Figure 4, grprec panel: The distribution here appears to move out of the range of allowable parameters. This would appear to suggest that the range should be widened.

C2587

Alternatively, if such a widening would lead to unphysical values, then this points to a deficiency in the model formulation, which would presumably merit some discussion.

Typographical/grammatical errors

p. 6734, l. 2: “terrestrial ground” sounds strange. I would suggest “of the land surface”.

p. 6734, l. 3: “applied to”, rather than “used for”.

p. 6734, l. 24: I would suggest to change to “...version 3.5 to switch to a cold bias in CLIM4.0.”

p. 6735 l. 25: The logic of the sentence seems backwards. Suggest “This is the case for the CLM-Crop model, as well as for ESMs.”

p. 6742, l. 8: Suggest “prior to this point” instead of “ahead of it”.

p. 6742, l. 15: Degrees should be defined. Presumably either “C” or “K”.

p. 6747, l. 7: “moment” instead of “momment”.

p. 6749, l.9: “effective”.

p. 6750, l. 16: “degeneracy”.

p. 6753, l. 24. “ultimately”.

Interactive comment on Geosci. Model Dev. Discuss., 7, 6733, 2014.

C2588