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Geoscientific Model Development Discussions

Interactive comment on "Modeling agriculture in the Community Land Model" *by* B. Drewniak et al.

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The authors describe the addition of a crop model to the Community Land Model (CLM). They show improvements in CLM relative to observations with the addition of this crop component. In addition, they show the results of two experiments with the new model: one performing a simple simulation of different residue management practices, and the other performing a simple simulation of different planting date triggers.

This is a well-written paper, and the addition of a crop module to CLM is an important improvement to this model. Many of the results in this study are not novel (they echo similar results that have been seen in previous studies), but the model improvements described here are well done, and this work lays good groundwork for future crop studies with CLM.

I would like to see the authors address three major points, and a number of minor

points, before publication. The third major point may require the authors to rerun some of their simulations and analyses; my other points can probably be addressed through revising the text.

First, the authors only cite Levis et al. (2012) briefly, and in passing. Given that that study is quite similar to this one – also involving adding a crop model to CLM, and also using the Agro-IBIS formulation as a starting point for the crop module – the authors need to give more recognition to that study. I understand that the current study differs in some ways from Levis et al. (2012), but the authors need to be more explicit about this. I would like to see some mention of how the two studies are similar and how they differ.

Second, there are a number of places where the authors state that the model agrees well with observations, yet their results – and even their own text – do not seem to support this conclusion. I appreciate the authors' careful assessments of the strengths and weaknesses of the model in this paper, and would like them to rephrase some of these introductory and concluding sentences to more accurately reflect the model's performance. I give specific instances of this in my specific comments below.

Third, I am concerned that the authors may have mis-applied the Sacks et al. (2010) crop calendar dataset in some instances. This is most noticeable in Fig 4b, which indicates soybean planting at the end of April in Bondville, IL. The Sacks et al. dataset shows a soybean planting date of Day of Year 146.5 for Illinois, which is at the end of May (and even later than this in most of the surrounding states, so I don't think this is a problem of spatial averaging). I would like the authors to double-check this, and if necessary, rerun their simulations and analyses with correctly-applied planting dates.

Additional specific comments:

1. P. 4138, L. 9-10: This is an instance where your broad "agrees well with measurements" statement is not entirely supported by your results; more nuances are needed here.

2. Introduction, last paragraph: Although the authors apply their model globally, it is largely based on parameterizations for the United States, and most of their comparisons with observations are for the United States. This limitation should be mentioned somewhere; this would be a good place to do so.

3. P. 4143, L. 13: air temperature, I assume? should be explicit

4. Section 2.1.3: For future work, the authors may want to consider Potter, P., N. Ramankutty, E. Bennett, and S. Donner., Characterizing the Spatial Patterns of Global Fertilizer Application and Manure Production, Earth Interactions, 14, 2010.

5. Section 2.1.4: More details, including equation(s), would be helpful here

6. Section 2.2.1 (or elsewhere): For flux site comparisons, do you just take the model results from the closest grid cell (as opposed to using site-specific data)?

7. Section 2.2.3: The Sacks et al. dataset only covers dominant growth areas of each crop, with very crude extrapolation outside of these areas. Please mention this limitation.

8. P. 4149, L. 8-9: GPP is rarely observed directly at flux sites. How was this derived?

9. Section 3.1.1, second paragraph: Please also give annual sums of GPP and NEE, either in the text or a figure

10. P 4150, L 11-13: The RMSD is roughly equal to the mean; this doesn't seem like good agreement to me

11. P 4150, L 14-15: I agree with this statement for Mead, but not Bondville. You acknowledge the problems at Bondville in the next sentence; this first sentence needs revision to account for this

12. P 4150, L 26-27: Here I disagree for Mead

13. P 4151, L 14-15: This is true for peak LAI, but simulated LAI seems to increase

sooner than observations show.

14. P 4151, L 15-17: For future work, the authors may want to consider the Agro-IBIS improvements documented here: Sacks WJ, Kucharik CJ (2011). Crop management and phenology trends in the U.S. Corn Belt: impacts on yields, evapotranspiration and energy balance. Agricultural and Forest Meteorology, 7: 882-894.

15. P 4152, L 19 and 27-28: Again, your conclusions about good agreement do not seem to be supported by the figures or your own text

16. P 4153, L 3-5: Other than irrigation (which, in the US, should mainly be an issue in the Western US), it seems that your crop parameters should generally reflect present-day U.S. management, at least if they are taken from Agro-IBIS. So I'm not sure that I agree with this explanation

17. P 4153, L 18: Why does this value of 165 bu acre-1 differ from the value in Fig 8?

18. P 4154, L 10: Here and elsewhere: "trends" implies change over time, which isn't what you mean

19. P 4154, L 19-20: "Globally... observed yields": this agreement is not surprising given the huge range in observed yields

20. P 4156, L 6 and 8, and elsewhere: Change "growing season" to "growth period" when referring to the time between planting and harvest (since "growing season length" generally refers to a climatic index, and a longer growing season is often associated with a shorter growth period)

21. Section 3.2.1 and 3.2.2 and associated figures: it would help to show the slope of these relationships in addition to the correlation coefficients

22. Section 4.1: Do you have any thoughts as to whether the problematic N cycle in CLM-CN leads to a too-large or too-small effect of these simulations?

23. P 4159, L 6-8: How can earlier planting lead to later harvest in your model? Or is

this just an artifact of spatially different responses?

24. Section 4.2: It is interesting to me that both of the temperature-based planting rules generally lead to lower yields. I would like to see some expansion of this point, perhaps in the discussion. To me this indicates why the use of a simple rule-based approach to planting decisions generally doesn't work well in a global model.

25. P 4160, L 16: To me this indicates a model deficiency: If farmers could achieve higher yields by planting later, they would do so.

26. P 4161, L 2-6: It isn't really true that this is the first version of CLM that fixes these issues, as I mention in my major points above.

27. P 4162, L 15-21: There is already a transient PFT dataset for CLM. How does your suggestion differ from that? Similarly, why did you choose to do your spinup using a steady-state scenario rather than using an existing transient scenario for spinup? (However, I'm more familiar with recent versions of the model than with the pre-CLM4 version the authors use, so it's possible that the transient scenarios I mention did not exist in that older version.)

28. Table 1: Initial leaf and stem allocation seem to be missing from this table

29. Fig 3: It's very hard to see the improvement here. Difference maps would probably make this easier to see

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