Earth Syst. Dynam. Discuss., 6, C510–C517, 2015
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Earth System Dynamics
Discussions
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ESDD
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Interactive Comment

Interactive comment on "Soil carbon management in large-scale Earth system modelling: implications for crop yields and nitrogen leaching" *by* S. Olin et al.

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Received and published: 21 August 2015

General comments Olin and co-authors nicely lay out developments in the LPJ-GUESS model related to agriculture, management, and coupled C-N cycles in their manuscript. My comments are intended to clarify the revised manuscript and aid reader understanding of this broadly interesting paper. Part of the challenge is documenting important code developments in the model, while still asking relevant and interesting scientific questions. I think the authors largely do a good job of this, although throughout I've tried to flag sections where a broader perspective or background would be helpful. After reading the paper, however, I was struck by several larger questions that should be resolved upon revisions.

1. How well can the model replicate temporal changes in crop yields through increases in cultivated land + changes in management practices? Can it capture anything like the Green revolution? Maybe it's documented elsewhere, but there's never any quantification of the climate change of elevated CO2 effects that underlie all of the results presented here? Step back and acknowledge what you've accomplished (adding crops and management to a global land model that represents terrestrial C-N biogeochemistry)! It seems important to document (and validate?) some of the basics.

I ask because the study seems to be motivated by the need to increase agricultural productivity to meet food, fiber, and fuel needs for a growing population (Introduction), but a hasty read would conclude that productivity has declined over the historical period and remains flat over the next century (Fig. 5c). I realize that's not the focus of this study, but some basic information about projected yields under a business as usual (control) management practice over the historical period and future scenarios would be helpful.

2. Second, greater context for the management practices tested here seems warranted. How may these results inform suggested agricultural management practices at large (i.e. National) scales? I realize that's not the aim of this class of models (section 4.1)- but I know of no other tool that allows us to start asking important questions about the long-term, broad-scale impacts of land management decisions. As in #1, with quick read one could conclude that cover crops, no-till, etc. cost more than they are worth because they decrease yields and don't do much to sequester additional C, besides nobody outside of Europe is really doing anything about N management so who cares? Perhaps this is the intended message? If not, what are the uncertainties or processes that need further development in the model.

Specific comments Abstract: The abstract is very hard hitting and direct. A sentence or two about the larger questions being asked may be helpful to introduce the topic

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for readers. Also the organization of the abstract makes me wonder what the focus of the paper will be (agriculture, N management, or DGVM). The results highlighted on P 1047, L 5-8 regarding effects of land use dynamics make me think it's on the DGVM, but to my reading the agriculture x nitrogen management questions are much more interesting

Introduction: Is a bit long, but I feel like it nicely provides an overview for the questions being asked here. If anything greater focus could be given to the novel aspects of this work (P 1051, L 23) and the full biogeochemical estimates that the study provides for tradeoffs between agricultural productivity, terrestrial C storage, and potentially N effects.

By the bottom of page 1051 through 1053, the organization (any my attention) started to waiver. Yes, agricultural management practices present a host of challenges and uncertainties on biogeochemical fluxes in the field and in models. The text provided is a nice review, but not really focused on the management practices that are the focus of this manuscript. Could revisions note challenges related to land use tillage practices, harvest residue removal, etc- but then focus on the experiments presented in this work (N management and effects on yield & biogeochemical cycles)?

One glaring omission in this work is estimates of N2O emissions. I'm not suggesting they be included in this study, and recognize that such estimates would be highly uncertain. Previous work, however, demonstrates that the climate consequences of N2O emissions may largely offset C benefits of fertilizer application (Butterbach-Ball et al. 2011; Pinder et al. 2012, 2013; Zaehle et al 2011). In my mind this limitation of the study should be directly addressed.

Methods & Results: Lots of minor questions listed below that generally ask for greater clarification or details about the model approach and assumptions related to aspects of the work presented here (e.g. N leaching, agricultural model, management, etc. For example, I feel a simple schematic of the soil biogeochemical model (similar to the one

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in Smith et al. 2014) that highlights where in the model different cropland management practices are used would be helpful. Moreover, aspects of the experiment and results need to be more transparent (e.g. stating depth of SOC observation, total N application rates, etc).

The decision to use a fistful of different climate simulations Cru and four CMIP5 climates under two future scenarios makes this manuscript harder to follow. In my mind this is a paper that documents & evaluates the crop and management featured developed in LPJ-GUESS. Layering on the nuances of multiple climate simulations seems overly ambitious and potentially distracting. Moreover, potential intermodal variation driven by different GCMs is normalized away in the results (Fig 5), making me wonder why different GCMs are really necessary?

Discussion & Conclusion I'd like a more prospective, positive, reflective tone throughout the discussion. Right now certain sections (e.g. 4.1) reads more like an apology for wasting the readers time than a thoughtful discussion of the results presented, their implications, and limitations. Similarly section 4.2 starts off by summarizing data from another paper by the same author group- but not on data presented here (see general comment #1, above)

Technical corrections P 1050, L 3-6. At first a first read the math behind these statistics didn't add up, how can we increase cropland more than 3 fold, when it's already 35% of the land surface? Then I realized that crop AND pasture made up this value. I wonder if it's worth focusing more narrowly on crop representations here, since that's the focus of the model results presented in this paper?

P 1050, L 25-29. Again the statistics aren't used clearly or effectively. Maybe get rid of the 70% value?

P 1054, L 27-27. As NO3 leaching is a major finding of the work presented I find the description of how the model handles N transformations, competition (between soil immobilization and plant uptake) and losses astoundingly brief. I'm not suggesting that

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work presented by Smith et al. 2014 (Appendix C), be repeated. It it's worth briefly summarizing the approach LPJ-GUESS takes for readers not familiar with the model.

P 1055, L1-20. As above, for readers not familiar with the crop model applied in LPJ-GUESS there a lot of uncertainty here. For example, I'm familiar with some models that have a grain fill stage of their life-cycle, that is eventually harvested (C & N pools). The idea of 'heat sum requirements' is not intuitive. Also, What happens to the harvested fraction of crops (i.e., where does the C and N go)? What happens to crop residues?

P 1056, Tillage. Century has a well-tested and described tillage scheme that has been applied in agricultural models (Levis et al. 2014). I'm surprised the authors seem to set out on their own to parameterize this model with what appears to be little validation.

P 1056, N application. Nowhere are total fertilizer amounts stated to illustrate if N application rates are simulated realistically. A reference is eventually provided (P 1059) and even displayed (Fig B1, but N application is never referenced in the text), but approximate fertilizer application rates would be helpful along with this reference. Between sections 2.2, 3.1, and Fig B1, it's not clear what data are being used, Zaehle et al. 2010, Elliott et al. 2014). Please clarify what data were used to force these simulations. See also comments related to Table A2 (below).

P 1060 L 16. What is ≈ 1000, the number of grid cells averaged?

P 1061 L 3, why did management only start in 1990? I guess you have to start somewhere, but

P 1061 L 13 what is Table 2.1.1?

Section 2.2.1, 3.2, & Table 2. References don't match Batjes 2005 (text) vs. 2014 (table). Also text and table should clearly state the depth of sampling for observations and models (which I don't think match. . .)

Section 3.3 The term 'implicit competition for N is somewhat misleading (Line 18, also on P 1071 L 21) because "At the time of sowing of the subsequent main crop, the cover crop biomass is added to the soil litter pool (P 1057 L 19-20). Is it just the cover crops reduced the size of the inorganic N pool (and lock up N in organic forms). These must eventually decompose, but maybe not over the 30 y timescale described here? Couldn't the declines in yield be met with more fertilizer? In reality is this necessary? Is there any cover crop effect on soil moisture and water stress?

P 1064 L 5-10, wait, the model does simulate pastures? How, all of the description in the methods related to crop management? Where are pastures locaed? How are they different from the natural vegetation they replace? There really is a lot going on here that's hard to keep track of...

P 1064 L 11-15, I apologize, but at this point my head is really starting to spin. This analysis (and Fig. 4) suggest how much additional C could be stored in agricultural soils using different management practices over a 30 year period using management practices that optimize soil C storage? If so, what's the number? If we radically alter management practices, what kind of payoffs for C storage does this analysis suggest? More broadly, all the abbreviations of different management practices (e.g. Fcc, Fnt) are convenient for the authors, but impede understanding from readers who are trying to keep track of a complicated story.

P 1069 L 19-24. Do either of the N fertilizer schemes align with actual agricultural practices? I thought the trick w/ N management and precision agriculture was assessing likely N demand early in the growing season so appropriate amounts of N could be applied.

P 1087 A description of the abbreviations used in Table 1 would be helpful in the table heading. Given that Table 3 references the abbreviations described in Table 1, this seems very important.

P 1089. What is the time period for these simulations? Also, are leaching values for organic and inorganic N, if so, what is the relative contributions of these two loss pathways? If this is just for inorganic N, what are the DON exports estimates from the

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model?

P 1092 Table A2. I don't see any information for DS = 0.9 in the table. How does the fraction of N supplied relate to the timing of fertilizer application in the real world? It makes it seem as though the model needs lots of fertilizer at the end of plant growth, which seems unrealistic? As mentioned above, what is the total amount of N supplied to different crops in different regions? How does it compare with actual N application rates? How does N demand change in the future?

P 1095, why is the change in N leaching axis reversed? This does seem intuitive, unless the upper right quarter or each graph is suppose to be a win-win scenario?

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Levis, S., Hartman, M. D., and Bonan, G. B. (2014) The Community Land Model underestimates land- use CO2 emissions by neglecting soil disturbance from cultivation, Geosci. Model Dev., 7, 613–620, doi:10.5194/gmd-7-613-2014, 2014. Pinder, RW, ND Bettez, GB Bonan, TL Greaver, WR Wieder, WH Schlesinger, EA Davidson (2013). Impacts of human alteration of the nitrogen cycle in the US on radiative forcing. Biogeochemistry 114:24-40.

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