[Paper #gmd-2015-68]

Response to Anonymous Referee #2's comments on "Integration of nitrogen dynamics into the Noah-MP land model v1.1 for climate and environmental predictions"

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Thank you to the reviewer for his/her insightful comments. We have incorporated revisions to address the suggestions as best as possible and hope that our changes will address all concerns.

The comments from the reviewers are pasted below (in **black font**) with our responses inline (in **blue font**).

The main task of this paper is to improve the N cycling processes represented in large-scale land surface model Noah-MP by integrating necessary processes in FUN for plant N uptake and fixation and processes in SWAT for soil N cycling. Such effort can lead to new contribution to improvement of the land surface modeling, especially for N cycle application and is of great importance to model community of climate, environment as well as biosphere.

My main concern is as following:

1. There may be systematic error in soil moisture modelling in your updated Noah-MP. As I noticed that the model generally overestimated and underestimated the soil moisture at low- and high- soil moisture cases, respectively, despite the observed outliers. Another issue in soil moisture modelling is that the tillage did not change the water dynamics in soil. However, you stated that you considered the redistribution of N in the submodel (SWAT). The question is how is the redistribution of N represented in SWAT? Does not it couple to soil water dynamics in SWAT or to other processes? You should explain this point a little bit more!

Re: We appreciate the good comment. There is a nice example in Neitsch et al. (2011) showing how tillage is represented in SWAT as below.

> "The mixing efficiency of the tillage implement defines the fraction of a residue/nutrient/pesticide/bacteria pool in each soil layer that is redistributed through the depth of soil that is mixed by the implement. To illustrate the redistribution of constituents in the soil, assume a soil profile has the following distribution of nitrate.

Layer #	Depth of Layer	NO_3 Content
surface layer	0-10 mm	50 kg/ha
1	10-100 mm	25 kg/ha
2	100-400 mm	20 kg/ha
3	400-1050 mm	10 kg/ha
4	1050-2000 mm	10 kg/ha

> If this soil is tilled with a field cultivator, the soil will be mixed to a depth of 100 mm with 30% efficiency. The change in the distribution of nitrate in the soil is:

Layer #	Depth of Layer	Initial NO_3	Unmixed NO_3 (70%)	Mixed NO_3 (30%)	Redistribution of Mixed NO_3	Final NO_3
surface layer	0-10 mm	50 kg/ha	35 kg/ha	15 kg/ha	22.5×10mm/100mm = 2.25 kg/ha	37.25 kg/ha
1	10-100 mm	25 kg/ha	17.5 kg/ha	7.5 kg/ha	22.5×90mm/100mm = 20.25 kg/ha	37.75 kg/ha
2	100-400 mm	20 kg/ha	20 kg/ha			20 kg/ha
3	400-1050 mm	10 kg/ha	10 kg/ha			10 kg/ha
4	1050-2000 mm	10 kg/ha	10 kg/ha			10 kg/ha
			Total mixed:	22.5 kg/ha		

Because the soil is mixed to a depth of 100 mm by the implement, only the nitrate in the surface layer and layer 1 is available for redistribution."

We have added a little bit description of the implementation of tillage in the manuscript.

To our best knowledge, the SWAT model does not consider the redistribution of soil water due to tillage either. Since tillage can only reach 100 mm which is just the depth of the first soil layer in Noah-MP, there is no redistribution of N within this surface layer. In other words, one purpose of tillage is to bring the surface residue down into lower soil layers; while this is not necessary for water. We understand that tillage can also alter the soil structure, and hence the soil hydraulic properties (e.g. porosity, hydraulic conductivity), which would affect soil water redistribution. However, this feature is not considered in this study because of the lack of data on soil hydraulic properties.

2. The effectiveness of this mode for a large-scale application. Since this model is only evaluated on one site in LTER of USA, I worry about the large-scale performance of this model. Yes, I know that the observations for N cycle components are generally limited. But I still wonder if you can get a more realistic result of NPP or NEE at other sites spanning a great climate gradient, for example, comparing to the default Noah-MP. One valuable point for your model is that the new Noah-MP model seemingly produce a more realistic interannual variation of NPP comparing to observation, whereas the default Noah-MP failed. This could be due mainly to the effects of dynamic N cycling together with the soil dynamics. I suggest to perform your model to some other sites spanning a great climate gradient to see whether you can get an improved estimation of NPP/NEE (as well as the IAV) comparing to the default one. This can partly verify that your model can be applied on a larger scale.

Re: We agree with the reviewer that more extensive tests across a gradient of sites would provide better insights into the generalization capability of the new Noah-MP. We are currently creating a 2D driver and then the model will be run and evaluated on regional or global scales.

Furthermore, we are participating in model intercomparision projects that are evaluating the new Noah-MP and other models across sites across the U.S. As noted by the reviewer, these additional sites may partially verify the generalization capability of the new Noah-MP. However, due to the lack of comprehensive observations of N dynamics, it is unclear whether N processes are well reproduced and responsible for the improved/decreased model performance in explaining NPP/NEE. Given the focus of this paper on N, we think it is more appropriate to design a rigorous plan to evaluate new Noah-MP for other sites without observations of N dynamics, and report the results in a separate paper.

In a word, I suggest a moderate revision before accept for publication on GMD.

Some specific comments:

1. Please check: "…land model…" or "…land surface model…"

Re: To be consistent, we changed all to "…land surface model…".

2. Page 4116, line 10: please give the full presentation for LSMs (i.e. Land Surface Models), because not all of the reader are familiar land surface modelling.

Re: "LSMs" was defined before the acronym was used repeatedly.

3. Page 4119, line 20: what is the BNF?

Re: It is symbiotic Biological Nitrogen Fixation. We changed it to the full form.

4. Page 4119, line 3-4, I did not find the mentioned equation (Eq.) 1-4 in your paper! Do you mean the equations in Fisher et al. (2010)? If so, please state it clearly. If not, please provide them.

Re: We meant the Eq. (3-6). We changed the sentence to "plant N uptake and fixation follows the framework of Fisher et al. (2010), which determines N acquired by plants through Eq. (13) advection (passive uptake), Eq. (4) symbiotic biological N fixation, Eq. (5) active uptake, and Eq. (26) retranslocation (resorption), (3) active uptake, and (4) symbiotic biological N fixation."

5. Page 4120, line 5, how did you determine KN and KC, are they parameters? Where did you get the Nleaf (I mean which submodel is in charge for Nleaf, please clarify it)?

Re: k_N and k_C are parameters directly from Fisher et al. (2010).

N_{leaf} is managed by the FUN sub-model

6. Page 4120, Eq. 7: there may be some error in the last component of this equation, please make sure you make sum from i = 1 to Nsoil, or to number of soil? I guess it should be the number of soil, but Nsoil is the available N in specific soil layer as you explained.

Re: Yes, it is the number of soil. We changed its symbol from N_{soil} to n.

7. Page 4121, line 8, soil temperature or air temperature?

Re: We clarified this as soil temperature.

8. Page 4121, Eq. 11, what are the $\gamma_{tmp,ly}$ and $\gamma_{sw,ly}$? Are they parameters, or how do you parameterize them?

Re: $\gamma_{tmp.ly}$ and $\gamma_{sw,ly}$ are nutrient cycling temperature and water factors, which are calculated by Eq. (9) and Eq. (10), respectively.

9. Page 4124, where did you get the Eq. 19? How did you define the threshold for γsw,?

Re: We got Eq. (19) from Neitsch et al. (2011). See Eq. (10) for γ_{sw}.

10. In section 3.5, you mentioned that all of the fertilization activities occurred after late June. Could you please show the fertilization records for this site? To my knowledge, the fertilization is quite different for different kinds of crops; for winter wheat the fertilization should not be so late, but for summer crops it can be. Another question is that how do you represent the crop rotation?

Re: The fertilization application data are plotted in Figure 1 (or Figure 8e in the revised manuscript).

It is true that different crops have different dates of fertilizer applications. The large amounts of fertilizer applied shown in the above figure are mostly for corn.

The question on crop rotation is very important. Currently, Noah-MP does not have a crop submodel and hence cannot simulate crop rotation in the model. The crop model for Noah-MP is under development at the National Center for Atmospheric Research.

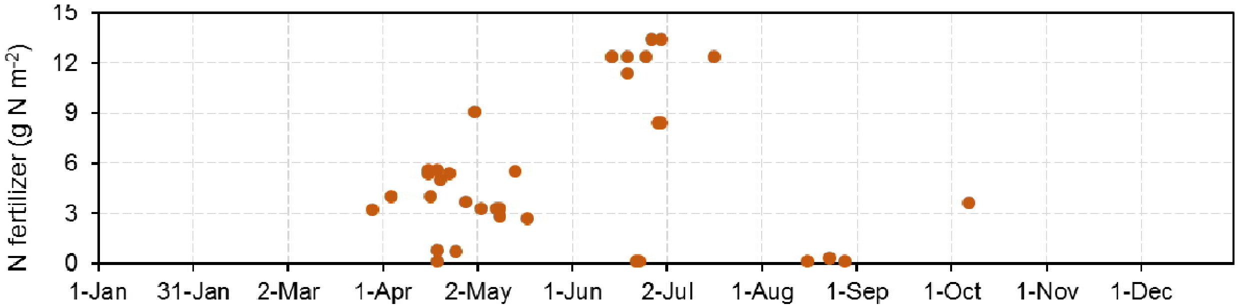

Figure 1 Actual nitrogen fertilizer application amounts and dates as recorded in the agronomic log.

11. Page 4132, line 11: with the default? Or with the observation? I did not see the default model results on figure 8.

Re: With observation (reality). We changed the wording to “real case”.

12. In figure 8, you state that the N leaching is more in default simulation that the others; did you perform the t-test? This comment is applicable to others similar comparision!

Re: The default simulation can significantly produce more N leaching than the Apr 15 experiment at 90% confidence level. However, the difference between the default and Jun 30 experiment is not significant.

13. There are few grammar errors throughout the paper.

Re: We have tried our best to correct these grammar errors. We appreciate the reviewer reading so carefully and catching the errors.

References

Fisher, J. B., Sitch, S., Malhi, Y., Fisher, R. A., Huntingford, C., and Tan, S. Y.: Carbon cost of plant nitrogen acquisition: A mechanistic, globally applicable model of plant nitrogen uptake, retranslocation, and fixation, Global Biogeochem. Cy., 24, 2010.

Neitsch, S. L., Arnold, J. G., Kiniry, J. R., and Williams, J. R.: Soil and Water Assessment Tool theoretical documentation version 2009, Texas Water Resources Institute, Texas A&M University, College Station, TXTechnical Report No. 406, 2011.