Geoscientific Model Development Discussions

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Comment on gmd-2021-260

Anonymous Referee #1

Referee comment on "Modeling symbiotic biological nitrogen fixation in grain legumes globally with LPJ-GUESS (v4.0, r10285)" by Jianyong Ma et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-260-RC1, 2021

General comments:

The biological nitrogen fixing (BNF) in legumes is an important biological and chemical process in ecosystems. The paper under review was high capicity scientific load and added two legume crops with BNF capacity into a DGVM model LPJ-GUESS to simulate global nitrogen fixation in legume-based cropping systems. The topic of this manuscript is very interesting and the BNF calculation exercise is probably a worthwhile contribution to other N general models. Readers are glad to see such a progress in modelling N fixing crops in the DGVM models community (to my knowledge, a empirical function between BNF and ET (or NPP) is often used in some DGVM models to estimate the annual N fixation across all terrestrial ecosystems). Additionally, just for curiosity, I checked all the observed data authors used for site-scale evaluation and simulation (10.5281/zenodo.5148255), which matches with the original literature listed in the text and provides an opportunity for other models' evaluation. Overall, this manuscript is well-written and easy to follow, with reasonable BNF representation and solid evaluation results, I thus suggest to publish it in GMD after minor revision.However, a few of minor issues that need to be further addressed concerning assumptions made, methodology, and clarity of presentation:

(1) Authors mentioned that N fixation is a high energy consumption process, with ~4-16% fixed C cost. But why gave the assumption of up to 50% daily NPP used for BNF in LPJ-G? I fully understand the purpose of setting up MAXbnfcost, in case of extreme cases taking place when modelling N fixation, but why the cost proportion is 50%, rather than 20% or 30%? Obviously, the value of 20% is much more close to the measured upper bound of 16%, no? Furthermore, carbon cost per unit fixed N (in the manuscript a fixed value of 6 is used in two legumes) is varying between crop growth stages (literally high C consuming at soybean V4-R2 stages), have you taken this into account?

(2) Regarding crop phenology days, authors used field-measured phu (Eq.1, it's actually 'degree days' if I understand correctly) to represent soybean growing period, Is the phu a

fixed or dynamic value across all evaluated sites? I'm just aware that the discrepancy in various phu values basically has huge impacts on harvested yield and accumulative N fixation because of the different growing periods. Also, how does LPJ-G represent the crop growing period on global scale? Sowing date and harvest date? Or multi-cropping systems in tropics are considered when comparing with FAO statistics?

(3) Authors highlighted that their model performed better in the top10 soybean-producing countries, but what about other countries? Look at the map of yield bias between simulation and observation (Fig. 9), the reported yields in most African countries were almost overestimated by 300%, any explanations? Also in terms of N fixation map in Africa, the simulated BNF rate can be as high as 300 kg N/ha (Fig. 10), this value is far from reality and does not make sense to me. The N fixation in African smallholder is greatly variable, even fixing zero N from the atmosphere sometimes because of the acid soil (see Vanlauwe et al., 2019, AEE). Moreover, using grain legumes as green manure to replace the industrial N-fertilizer is not common in African and east Asian farmer's fields (N fixing grasses are much popular), It'll be much interesting to add relevant discussion on modelling forage legumes.

Specific comments:

Line 50-65 : is it necessary to list the model name in the brackets?

Line 94: "two temperate C3 crops with sowing carried out in spring and autumn", what is the specific name of these two C3 crops? wheat?

Line 100: "faba bean as a second as a representation of pulses more generally", it's unclear to me, you mean to simulate other pulses using faba bean parameters?

Line 150-152 (Fig. 1): Interesting findings, is the data from Penning De Vries et al. (1989) the only source for fitting the soybean assimilate partitioning in the study? Any other possible measurement for comparison?

Line 185: why is soil temperature at 25 cm depth? No nitrogenase activity below the 25cm of depth?

Line 213-215 (Eq.13): To give readers a general idea on how MAXbnfcost is varying over

the growing season, would be better to plot its function somewhere. Maybe in Fig. S5 in the Supplement.

Line 245: 'to convert plant C mass to dry matter, a conversion factor of 2.0 was used'. The conversion factor of 2.0 seems a little bit lower than that data based on the tons of published literature. did it impact the conclusion of the paper?

Line 255-257: 'Gridded daily climate data .....were used from GSWP3-W5E5', why not field-based weather data? I understand the observed climate may not available at some sites, but the discrepancy in climate may bias the evaluation results. I suggest authors make a comparison between gridded climate and observed data at several field sites, where the recorded weather data are available.

Line 285: would be helpful to plot the map of N fertilizer (mineral + manure) applied to soybean and add it to the Supplement.

Line 345: I noticed that soybean shoot biomass was largely underestimated in Fig.5a, is it a common result across most of sites or only happening at the U.S. site? If it is common among all evaluated sites, authors should check their fitted assimilate partitioning in Fig.1 furthermore, and the C allocation scheme to plant organs should be further improved.

Table 2: For the observed and modelled soil N uptake, why is non-nodulation experiment significantly higher than nodulation one? different N-fertilizer application?

Line 397: 'as a consequence of the inoculation implemented.....', just want make it clear to me: inoculation is only available in the unfertilized treatment, but not in the N300 treatment; thus N fixation from N0 is greater than N300, resulting in slight difference in total N uptake between two treatments?

Line 417: 'Modelled low yields were found in some arid and semi-arid countries (e.g., Egypt, Iran, and Turkey)', the reason is water constraint on photosynthesis or on BNF or both? Look at the map of environmental limitation to BNF (Fig. S4, in the Supplement), in these regions water is a key factor affecting N fixation.

Line 485: As I mentioned earlier, the conversion factor of 2.0 is low, authors also discussed here. Why not use the new factor of 2.24 in the model evaluation?

Line 542: 'with the reported range of 14-32%', why the cost fraction becomes to 14-32%

here? it is described as 4-16% in the Instruction section (see Line 59)

Line 557: ‘with the estimate of 19.4 Tg provided by Herridge et al. (2008)’ . I don’t think 19.4 Tg N is the correct value derived from Herridge et al. (2008), in which a total of 21.45 Tg N is fixed by all legume crops every year globally.