

Review #2

The paper present a new parameterization of biological nitrogen fixation in the LPJmL model. This new parameterization, compared to the original one, takes into account the nitrogen limitation and a carbon cost for acquisition of the BNF. This is a very important improvement as it means that nitrogen fixation is directly linked to the biological activity and nitrogen limitation, which was not the case before. Hence, the total BNF fixation is reduced compared to the original formulation, which is more in agreement with observations. So it is an important improvement for LPJmL. The paper is sound and well written. So I have only few remarks, only some suggestions to improve the paper:

Thank you very much for the positive evaluation of our paper. We highly appreciate the feedback and give a point by point response with our suggested changes below.

Even if the original approach of simulation of BNF fixation has already been published, it would help the reader to present the original equations and then to show in detail what are the difference between "original" and "C-costly" parameterization. For instance, we understand only in the discussion about BNF fraction that the 2 parameterization are different not only on the calculation of N but also on the way this N is taken by the plant, directly in the new parameterization and mixed with soil mineral N in original which is also an important difference. Then it is important to give more details about the parameterization and how they differ. More generally, it would be also interesting to compare the new parameterization to parameterization used in others DGVMS that implements BNF.

We agree that this will facilitate the comparison of the two approaches and will include a description of the original approach. We will also include a conceptual comparison to BNF approaches of the models synthesized in Kou-Giesbrecht et al., 2023 and Liu et al., 2011 as also explained in our response to major comment three of reviewer one.

The results focus only on BNF, but it would be interesting to see also at global scale what is the impact of the new parameterization on the carbon cycle (for instance impact on NPP, NBP). Only the impact on legumes yield is shown if figure B1.

As the carbon and nitrogen cycle are closely linked, the overall change in NBP will qualitatively be similar to that of the overall N balance (see Fig. S5). However, we agree that explicitly showing the main C balance components will increase the informative value of the paper and will add a figure in the style of Fig. 4 for the C balance.

Also on figure 4 we see the relatively large impact of the new BNF parameterization on N emissions. It would then be interesting to show a comparison of these simulated fluxes to observations, as it is done for BNF. Especially for N₂O emissions. It is obviously an important component of the GHG budget. So, with the new BNF parameterization, does it improve the simulated fluxes of N₂O ?

We included a comparison to global literature estimates of N emissions in Tab. B1. We propose to move it into the main text and extend the respective sections of results and the

discussion. We will also include additional literature on N₂O emissions in the discussion (e.g. Scheer et al., 2020).

Minor remarks:

I 268: The authors seem surprised that the new approach does not limit the crop yield. But if I understood well the model, it is not so surprising for me. Since in condition when NPP is not a limiting factor for BNF (that should be the case for crops) and, as the model try to fulfill the N limitation, then the simulated BNF should be sufficient to fill the N demand of the plant and then should not produce N limitation ? Then it could explain why even if the different approaches give different BNF there is no impact of yield. This is exactly what we expect from the new formulation compared to original: define the BNF to avoid N limitation but without N excess... This is also the reason why it would be interesting to show the global impact on NPP: We should expect a decrease in NPP on carbon and N limited ecosystems, as the C-cost or N stress could be too high to be fulfilled by the BNF fixation. On the contrary, we should have no change in ecosystem with few limitations even is the BNF is reduced.

Thank you for bringing this up. While NPP may not be limiting for BNF, we expected it to be lower because of the investment cost for BNF which is subtracted from the NPP, thereby reducing NPP available for plant growth and grain formation. We therefore expected a reduction in legume crop yield compared to the implementation in which the crops got all the N they need for free. We show that global yields of soybean and pulses are reduced (L248, Fig S3 and S4). However, we expected a stronger reduction. We provided one explanation which is that the reduced respiratory losses of NPP balance expenses for BNF (L269-273 and Fig. B2).

Nevertheless, we agree that including the additional insights from the C balance assessment (major comment two) will further improve the discussion of this aspect.

Figure 2: what are the percentage indicated in blue and red in a) ?

These indicate the overlap between simulated and observed ranges. We will add the explanation to the caption and provide the formula in the evaluation section.

Figure 3: the DBf term is not defined. I guess it is the observation, but it should be described

Thank you. This was also pointed out by reviewer 1 and is indeed referring to the Davies-Barndard and Friedlingstein data. We agree that it needs to be explained in the caption.

Bibliography

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