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Biogeosciences
Discussions

Interactive comment on "Impacts of biological nitrogen fixation on N_2O emissions from global natural terrestrial ecosystem soils: An Analysis with a process-based biogeochemistry model" *by* Tong Yu and Qianlai Zhuang

Anonymous Referee #1

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General comments

Yu and Zhang present an interesting approach to address an ongoing gap in our collective knowledge: we simply do not know how much nitrogen is being fixed on earth. The range of estimates for this process remain very large, and approaches to improve the accuracy of these estimates are necessary.

As such, efforts such as this modeling approach, are welcomed additions to the debate. This manuscript lays out a modeling approach focused on the contribution of legumes

to global rates of nitrogen fixation. They estimate that globally nitrogen fixation introduces $\sim$ 61 Tg N yr-1 into terrestrial ecosystems. This number is on the lower order of experimental scaling compendiums, however, well within the bounds of estimations made on the basis of isotope box models. However, as this manuscript points out, there are significant pathways of N-fixation not represented by this model (free-living fixation via heterotrophic bacteria or cryptogamic crusts) that could contribute significantly to terrestrial fixation, which would presumably increase their mean estimate towards that calculated via the relationship between N-fixation and evapotranspiration.

Overall, I think this is a nice manuscript, and a good contribution to this debate. However, I had a hard time following some modeled controls on N-fixation (see my comments), and was also confused by the purported relationship between N-fixation and nitrous oxide production. I could be wrong, but there is not, to my knowledge, a linear relationship between natural fixation and nitrous oxide production. It's clear that areas with high fixation are also sometimes areas of high N2O production, however, this is more to do with conditions at the site than a direct link? Furthermore, N2O production is a far more episodic process than N-fixation. Therefore point measurements of N2O can represent significant underestimates, or overestimates, of annual N2O production depending on the conditions that measurements took place under. Add to this the role of nutrient use efficiency in retaining or releasing nutrients within or from plant biomass, which varies depending on species and stand age. For these reasons, it seems problematic to compare N-fixation with N2O production, and I would ask for a expanded justification for this aspect of the study.

I also think the title is somewhat misleading. As I see it, the relationship between fixation and N2O production is not the central feature of this manuscript. This manuscript describes a new module within an existing model, and the factors controlling the rate of fixation. The majority of the discussion is concerned with comparing the estimates generated here with estimates from previous studies.

Another issue I found was that estimates for fixation span from the tropics to arctic

regions, yet the model itself does not represent the dominant fixers within arctic regions. While I don't doubt the generalized findings that the arctic regions have lower fixation than tropical regions, it seems that the model cannot accurately estimate fixation rates for Arctic regions with it's current structure leaving me to wonder how to interpret the values reported for Arctic regions?

Finally, while the quantitative nature of the manuscript is appreciated, the quality of the writing is quite poor. A few examples are given below, however, I believe prior to resubmission the authors should correct the grammatical mistakes throughout. In general, the writing needs to be tightened up throughout too.

Specific comments

The above mentioned grammatical issues include, but are not limited to,

Ln 30: This sentence needs to be tightened up, but the second half could be changed from, 'and decrease from the equator', to 'which decreases from. . .'.

Ln 34: Remove 'the' prior to 'fixation'.

Ln 36: Change 'types', to 'type'.

Ln 36: Remove 'the' before 'biological nitrogen fixation'.

Other examples are apparent throughout the manuscript (e.g., Ln. 97; Ln 100, Ln 109, Ln 256, etc.).

Ln 51: Gruber and Galloway, 2008, Nature, 451(17), 293-296, would help constrains the quantitative aspect of this sentence.

Ln 80: I think it would help to have a model schematic here - I understand a schematic of the broader model has been published previously, but repeating that schematic and focusing on the newly integrated processes would help the reader.

Ln 112: What data is required to derive these estimates?

Equation 4: What is the origin of the soil nitrogen inhibition values? Please reference the manuscript these values were taken from.

Ln 174: I think the authors mean 'a priori values', no?

Ln 226; Should this value be -5 % rather than 5 %? I think the value of -5 % is reported elsewhere in the manuscript.

Ln 229: What controls N2O fluxes within the model?

Ln 297: What does 'affected' mean here? It would help to be more specific about direction, for example, does it mean enhanced or reduced?

Ln 368: Change subheading 'Major controls to. . .', to, 'Major controls on. . .'.

Table 5 needs references to explain the origin of and biome specific variability in these values.

Seems like Fig. 1 is a repetition of data in the tables? Is Fig. 1 needed? Particularly because there is far more information in the tables.

Please improve the quality of the figure 2, which is quite poor. Why are the x-axis values just floating in the middle of the figure? And why abbreviate them?

Technical comments.

Ln 84: It's not clear to me if the published ARA studies used to calibrate the model have all themselves been calibrated with 15N measurements? The ARA approach is notoriously difficult to interpret without reliable calibration, and the conversion of acetylene reduced to nitrogen fixation ranges significantly depending on various factors include the specific nitrogenase enzyme.

Ln 124: I'm confused by this temperature relationship, is this a Gaussian distribution similar to that laid out by Houlton et al., (Nature, 2008, 454, 327-330)? It doesn't appear to be - this relationship sounds like there is a very broad plateau whereby temperature

does not limit fixation across a wide range (12 - 35 C). Is this correct?

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