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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 #2

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This paper describes the development of a process-based model to simulate global nitrogen fixation in terrestrial soils and then couples that model with previously published model(s) predicting N dynamics (Terrestrial Ecosystem Model, TEM) and N2O emissions from soils. The goal was to quantify the contribution of biological nitrogen fixation (BNF) to global N2O emissions and to better match the simulated emissions with observed data. The BNF calculation exercise is interesting and probably a worthwhile contribution to the N general model. However the major focus on tying that to N2O emissions is more problematic and the paper could benefit from some redirection.

Some issues that need to be addressed concerning assumptions made, methodology, conclusions, and clarity of presentation.

1) The term 'natural' is frequently used throughout the text without attempting to define the context in which it is intended. 'Natural' has many implications, connotations, and hidden assumptions. An explicit definition of the term, as it is used on the paper, is needed. How well do the field sites selected relate to that definition? Indeed, since estimates are that humans have doubled the amount of fixed N applied to the terrestrial landscape of the planet (see the Galloway et al., 2004 reference), how does that relate to the 'natural' sites? More importantly, how can global extrapolation of BNF relate to global N2O emissions given the substantial contribution that the anthropogenic fixed N (ANF) must have made? While the paper does not completely ignore the importance of ANF, it does not make clear distinctions between the two relative to N2O emissions.

2) The paper only mentions the TEM and the N2O emissions model in passing. Little information is provided as to how the BNF model is integrated into those pre-existing models to derive N2O emissions. Where does the newly fixed N enter into those models? Was it considered to increase the soil organic-N pool size? Or was the assumption made that newly fixed N was all immediately taken up by plants, or both? Were other parameters, such as soil inorganic-N supply, in the previous TEM model modified when the BNF model was included?

3) The general conclusion is that including BNF resulted in additional N that led to -5% to +20% changes in seasonal soil N2O emissions. The main differences occurred in the winter months. That was the range, but what was the central tendency of the effect? Figure 5 suggests that there was generally little change in emissions overall. Indeed, one could easily conclude from Figure 5 that including BNF in the larger model did not have a substantial impact on N2O emissions. Perhaps that is not too surprising when one considers that the total fixed N pool size (plant biomass + soil fixed N + atmospheric input) must be substantially larger than the annual amount of newly fixed N from BNF. The abstract states that: "This study highlights that there are relatively

large effects of the biological nitrogen fixation on ecosystem nitrogen cycling and soil N2O emissions." The results shown in the paper and the discussion do not at all agree with that conclusion relative to N2O emissions.

4) Given the large overlap in tables and figures between this paper and Yu and Zhuang 2019, one wonders whether the incremental contribution of this paper relative to N2O emissions represents a publishable, stand-alone contribution over and above Yu and Zhuang 2019. How does this differ from a laboratory experiment that adds little to no new insight into what is already known? In my opinion, the paper can be, and should be, strengthened, by including additional considerations, such as ANF or the differential effect(s) of N speciation on BNF. Or perhaps more radically by reducing or eliminating the focus on N2O emissions all-together and refocusing on how the BNF inclusion changes the N cycle fluxes in the TEM model. In short, the paper has too much emphasis on N2O emission given the Yu and Zhuang 2019, paper while more could be done overall concerning the BNF contribution to the model.

5) Can N2O emission data from 8 (line 170), or 6 (line 197), or 5 (Table 3) sites (which is it?) be reasonably extrapolated globally? Those sites were chosen because they were "affected by legumes." What is the implication of that to the extrapolation? Yes, it was subsequently tested on 35 other (?) sites. But there was little N fixation measured for almost half of those sites.

6) The paper would benefit greatly from careful editing. There are numerous errors and discrepancies, particularly between the text and the figures and tables. Many are listed below.

Other, more minor comments:

Line 63. The EPA reference is missing.

Line 104: "and for spatial limitation". How does that relate to C demand?

Eqn 1: Nfix is not defined in the text.

Line 143: use the same terminology throughout the paper. Upper threshold is given here. In the table it listed as 'upper bond' (sic).

Line 155: change to read "every unit of nitrogen fixed..."

Line 161: the units for Cr in the text do not match the units given in Table 1.

Line 170: 8 sites are indicated, but only 5 are listed in table 3.

Line 186 and throughout: use past tense.

Line 186: Table 2 lists 7 ecosystem types, not 11.

Line 197: Table 3 lists 5 field sites, not 6.

Line 201: should be "sensitivity testing."

Line 205: Figure 2 is a result, not a method.

Line 214: no standard deviation is given.

Line 214: What is the rationale for "removing these data?"

Lines 216-217: "simulations are closer . . .in temperate forests. . ." Close to what?

Line 228: There is no N2O data in Figure 3.

Ines 244-247: Sentence starting with "Here" is unclear. Is that referring to the previously cited study or this study? Usually that term refers to the current study, but it appears to be referring to Bruijnzeel et al.

Lines 229-231: "The comparison between measured and simulated data further shows the influence of BNF for different ecosystem types. . ." This is unclear and needs further explanation.

Line 248: 32.5 Tg N does not match the number in Table 4.

Line 248-249: give the numbers in text and refer to Table 4.

Line 279-281: This sentence should include some mention of the high nitrate concentrations typically found in desert soils.

Lines 297-298: What was the overall mean and standard deviation of the model results when BNF was and was not included in the N2O emission simulations?

Line 374: change slightly to slight.

Lines 381-384: Not all agree with this statement. See Heden et al., 2009, Ann. Rev. Ecol. Evol. Syst. 40:613. Alternate explanations should be included here.

Table 1: Change bond to bound throughout. Provide units for all parameters and ranges for coefficients. The description for the Michaelis-Menton constant is incomplete. What process is that a constant for?

Table 2: Column headers should be "measured N fixation rate" and "simulated N fixation rate."

Table 3: Wagga Wagga is in Australia.

Table 5: N_pot parameter format and units do not match Table 1. What are units for fNup? Units for Kc do not match Table 1.

Figure 1: The grey to blue colors are hard to distinguish against the green and blue background shading. Use the same units for N2O emissions and N fixation rate here and throughout the paper.

Figure 3: What do the lines represent? Regression lines forced through zero? If so, what is the rationale for doing that?

Figure 5: Point out the y-axis scale differences. The scales chosen for the two tropical forests is rather misleading and are based on what appear to be outlier observations. Suggest using the same scale with a y axis break to include the outliers so that these two panels can be more easily compared. What ecosystem is panel e?

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