Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-118-RC1, 2019


# *Interactive comment on* "$N_2O$ changes from the Last Glacial Maximum to the preindustrial – Part II: Terrestrial $N_2O$ emissions constrain carbon-nitrogen interactions" *by* Fortunat Joos et al.

**Anonymous Referee #1**

Received and published: 1 May 2019

This paper takes new ice-core data on nitrous oxide emissions over the deglaciation and compares it to modelled results so as to better understand biological nitrogen fixation (BNF) in the same period. There follows a section on the contribution of different climate drivers on nitrous oxide emissions in separate attribution simulations. The abstract is rather misleading, focusing on the work of the 'part I' paper which describes the ice-core data, and BNF rather than attribution modelling. However, there are two problems with the main BNF part of this paper: the model's representation of BNF, and the hypotheses posed. These two issues combined make the model runs virtually



meaningless and the conclusions baseless.

The model BNF

The model BNF (page 9, 13 and Fig.1) is 523 TgN/yr globally in the pre-industrial simulation. The authors acknowledge this is "higher than the published range" but are selective about what range they are referring to and what the implication of this is. Their BNF is an order of magnitude more than low budget-based estimates (Vitousek et al., 2013) (44/58 TgN/yr) and almost 200TgN/yr larger than the upper model estimate (Xu-Ri and Prentice, 2017) (340 TgN/yr) they reference. The authors cite Cleveland et al., (1999) (195 TgN/yr), but interestingly fail to cite the more recent paper by the same author, revising the estimate down to 127.5 TgN/yr (Cleveland et al., 2013). The authors' reference to Lenhart et al., (2015) (page 9, line 16) as part of the "published range" of global BNF estimates is baffling, as the paper discusses nitrous oxide and methane emissions, not BNF. Moreover, the authors fail to mention that of all estimates of BNF in the last half century, only one (Xu-Ri and Prentice, 2017) is over 300 TgN/yr and most are around 100 – 150 TgN/yr.

These global BNF estimates of around 100 TgN/yr are because field experiments show that nitrogen fixation is relatively unusual in the terrestrial biosphere. Whilst individual nitrogen fixing plants or organisms have the potential to fix large amounts of nitrogen, they are best suited to 'pioneer' environments with low soil nitrogen, are usually found at very low densities in mature ecosystems, and may be facultative (rather than obligate) fixers. Taking one example, tropical forest is generally thought to be the highest BNF region due to its high NPP and low nitrogen limitation. Recent work by Sullivan et al., (2014) found that tropical forest in Costa Rica has BNF of ∼0.5 gN/m2/yr. From the map of pre-industrial BNF in LPJ-Bern (Figure 5 A) it seems BNF in Costa Rica is modelled at ∼10 gN/m2/yr. i.e. the model overestimates BNF in the tropics by a factor of ∼20. Though no present day BNF value is given in the paper, from the information available the present-day modelled value for Costa Rica is likely higher. Even compared to the upper bound for tropical forest of 6 gN/m2/yr from the meta-analysis done by Cleveland et al., (1999), the values in LPJ-Bern are high. Cleveland et al., (1999) said their upper limit was "extremely unlikely" and the global BNF from those upper values was 290 TgN/yr (compared to LPJ-Bern's 523 TgN/yr).

The BNF in LPJ-Bern is entirely disjointed from reality. This puts significant doubt on the ability of this model to produce meaningful results about or based on BNF. The authors infer on page 9 that the high BNF is irrelevant to their results. They describe two sensitivity experiments with lower global BNF and the nitrous oxide emissions are broadly similar. However, the two sensitivity experiment global BNF values (310 and 188 TgN/yr) are still unrealistically high. Therefore, these sensitivity experiments reveal that the problem with BNF in the model may not be a parameter based issue but could be something more fundamental.

The hypotheses

The hypotheses set out on page 7 present a false dilemma. They are based on the premise that the nitrous oxide emissions are attributable to a nitrogen system that is either 'open' or 'closed'. The idea of either an open or closed system stems from the assumption that the nitrogen cycle is in equilibrium during the deglaciation (stated by the authors on page 6, line 5). However, there's no evidence either way on this question. Even if the assumption of equilibrium is accepted, an open system is not the only mechanism of increasing nitrous oxide emissions, thus the hypotheses are a false dilemma. The changes in nitrous oxide could also be caused by changes to the internal dynamics of the system (e.g. soil nitrogen turnover, or flexible C:N ratios, or the authors' assumption of homogeneous nitrous oxide yield fractions over space and time (page 8, line 26)).

The answer to the false dilemma presented is pre-decided by the definitions the authors give on page 6. Under the false dilemma, an 'open' (high-input-high-output) system is presented as the only mechanism that can produce high terrestrial nitrous oxide emissions. We know we have increasing (nitrous oxide) output, so under the assumption



of either 'open' (high-input-high-output) or 'closed' (low-input-low-output), there is only one possible answer. This pre-determined result is exacerbated by the model. The high BNF in the model means the N cycle in the model must be 'leaky' and 'open' otherwise there would be no N limitation at all (contrary to the evidence, see LeBauer and Treseder, (2008)). Some models take a more 'closed' approach (often resulting in low BNF). But it stands to reason that closing the input of N in a model reliant on high N input will cause the model to produce results inconsistent with reality. These simulations might inform somewhat about the model but can't say anything about real world BNF.

The combination of an invalid hypothesis and an inappropriate model is results that mean nothing and conclusions that mislead. An unwary reader could easily take it at face value that BNF increased by 72 TgN/yr during the deglaciation and an 'open' terrestrial nitrogen system was the only, or most likely, way the observed nitrous oxide changes could have occurred. But there is no reliable evidence for either of these assertions.

The second part of the paper on attribution appears more sensible (issues with the BNF representation in the model not withstanding). This part of the paper might be appropriate for resubmission separately, without the BNF model results. With the two presented together it's difficult to assess the attribution section fairly.

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Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-118, 2019.