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Biogeosciences Discussions

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 #2

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The manuscript of Joos et al combines ice-core derived terrestrial N2O time-series with process-based N2O simulations to derive constraints on terrestrial nitrogen dynamics. The manuscript is well written and easy to read despite a little long and redundant. I suggest to be cautious about points mentioned below before the final acceptance. One of my concerns is that the conclusion related to biological controls on N acquisition is already pre-included in the assumptions/definitions based on which the model is built. BNF in the manuscript refers to any N inputs, other than atmospheric N deposition, that satisfy ecosystem N demand. With a constant annual N deposition rate, any changes in

ecosystem N content and losses are attributable to BNF. Here BNF incorporates both biological and non-biological sources, which might come from weathering, be undiscovered N sources existed in pre-industrial time, or errors from assuming constant N deposition rate. A second concern is about the adjustments of global inflow of reactive N on multi-decadal to century time scales derived from N2O dynamics. As the authors mentioned, there are multiple-steps and many factors come into play in global N cycle. Different N2O production pathways may evolve through time, alternations of soil organic matter decomposition, stoichiometry and other N loss pathways are likely to shift N2O emissions. These adjustments are likely to occur without significantly alteration of real biological nitrogen fixations. For example, nitrifying and denitrifying microbes may have different temperature sensitivities vs. BNF. Vegetation and microbial evolution are largely unconsidered in this study. There is no strong evidence that N input flux would adjust as quickly as that of N2O emissions. A third concern is related to insights to be learned from this study. Is it necessary to conduct spatially explicit model simulation to test constant vs. dynamic BNF? The magnitude of N2O emissions can be easily tuned through RN2ODN, whereas the "openness" or 'tightness' of N cycle is, to a large extent, conceptual and not new in literature. The spatial pattern is also within our general understanding of global ecosystems as the model is built upon the contemporary (not paleo-) biogeochemistry and driven by historical climate. I feel the climate sensitivity of N2O emissions are valuable information that worth exploring for models like LPX-Bern. Specific points: 1. P6L25-30. Does it worth discussion on losses of plant-available vs. plant-unavailable (e.g., through fire and leaching of DON) N and how losses of plant-unavailable N alter system dynamics? 2. P10L5. It is unclear when the upper limit of denitrification is used

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