Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-513-RC1, 2020

Biogeosciences Discussions

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Interactive comment on "Nitrogen Cycling in CMIP6 Land Surface Models: Progress and Limitations" *by* Taraka Davies-Barnard et al.

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Received and published: 7 February 2020

Authors compare the behaviour of coupled terrestrial N and C cycles in five models that are contributing results to sixth phase of CMIP (CMIP6). The subject of the manuscript is of clear and significant interest to the Earth system modelling community as more and more land components in ESMs explicitly represent terrestrial N cycle and given the large spread among land C cycle models. However, in its current state the manuscript appears to be written hastily with several points unclear, statements that are weakly supported, some incorrect statements, and at places the analysis of results is as simple as which model produces high values of a given quantity and which low.

I have three major comments.

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First, nitrogen used efficiency (NUE) as introduced in equation (1) is simply C:N ratio. In the current literature NUE is typically defined as an efficiency indicator for the utilization of nitrogen in agriculture and food systems (Fageria and Baligar, 2005). That is, higher the NUE the lower amount of applied N enters the environment. I suggest, to avoid confusion with existing definition of NUE authors simply use C:N ratio in equation (1).

Second, the authors have compared the results of two experiments, $+CO_2$ and +N, from models with observation-based estimates. I feel that, the observation-based estimates and the experiments they were based on have not been properly introduced. Nor do the authors discuss limitations of these real world experiments whose results are used to evaluate models. For example, the results from $+CO_2$ experiment used to evaluate models are based on the Baig et al. study which is a meta-analysis but a reader is never told about this. How many studies does this meta-analysis summarizes results from? Similarly, for the +N experiment, the LeBauer and Treseder (2008) study is also a meta-analysis. Both these meta-analyses, results from which are used to evaluate models, should be properly introduced and their limitations discussed. For example, the $+CO_2$ type experiments done are based on instantaneous doubling of CO_2 while in the real world CO_2 is increasing gradually. Similarly, in +N experiments additional N application rates, I think, are increased instantaneously while in the real world N deposition rates have increased gradually. In addition, can the average results from meta-analysis be used to evaluate the globally-averaged response. The photosynthesis theory says that the CO_2 fertilization effect must be strongest in the tropics. How does one account for this? Were the studies used in meta-analysis uniformly distributed geographically speaking? As a modeller myself, I realize, the business of evaluating models is difficult but as long as limitations of observation-based estimates are mentioned, it allows readers (and authors too) to make a rationale and informed expectation of the extent to which observations and models should compare well with each other.

My third comment is that as a reader, after reading this manuscript, I am not sure if I know anything more about N cycling in models than I did before. I feel, the results from these models need to be analyzed and reported in a much more clever way to provide overarching conclusions. Note that the ability of models to simulate recent trends in GPP and NBP is not due to N cycle. Models without N cycle can achieve this too as is seen in the TRENDY intercomparison which contributes results to annual Global Carbon Project studies (Le Quéré et al., 2018).

Other comments

Page 1, abstract, lines 26-28. Upon reading these lines it is clear that 200 ppm CO_2 and 50 Kg N/hectare.year N deposition increase are both hypothetical. But as a reader I was wondering what observations are used. At this point in the abstract the reader is not aware that model results are being compared to results from meta-analyses later in the manuscript.

Page 3, line 85. Please consider rewording "All models ran a global spin-up for all ecosystem pools up to the year 1860" to "All models pools were spun up to equilibrium using climate and other forcings corresponding to year 1860".

Page 4, Section 2.2 and 2.3. Please consider summarizing in a table the runs performed. After the pre-industrial spin up, it seems, three runs have been performed – a 1861-2015 historical simulation, a +CO_2 simulation for the period 1996-2015, and a +N simulation for the period 1996-2015.

Page 4, equation (1). Please use C:N ratio in this equation as opposed to NUE.

Page 4, equation (2). This equation is incorrect. Change in NPP cannot be simply determined by multiplying the changes in NUE and N uptake. Please see https://en.wikipedia.org/wiki/Product_rule which explains the product rule of differentiation.

Page 4, equation (3). Please define delta N (which implies N balance, I think) properly in words. It seems it is the change in total amount of N in the land (Tg N). But the right hand side terms of the equation are all fluxes which implies the units of N should be Tg N/year. I am confused. The term "N balance" is used throughout the manuscript. It is an important term and yet in the absence of clear worded definition and units it is difficult to follow the context in the rest of the manuscript where this term is used.

Page 5, lines 136-137 reads "This generation of N models are generally consistent within observational constraints, showing an improvement compared to CMIP5 N models". However, nowhere in the manuscript have model results from CMIP5 models been shown so how can one conclude CMIP6 models are better than CMIP5 models. Please reword this sentence.

Page 6, line 168. Please consider replacing "non-N model structure" with "C cycle related processes".

Page 6, line 174 reads "Across the ensemble there is a slight correlation between the global GPP total and NEP". Please note that for the pre-industrial spin up models' NEP is zero since the model has been spun up to equilibrium. This implies for the pre-industrial state there is no correlation between GPP and NEP. Over the historical period, there is no reason to expect a strong correlation between absolute GPP values and NEP. What is expected is a strong correlation between rate of increase of GPP and NEP since it is the rate at which GPP increases that determines the land C sink.

Page 6, lines 175-176 are unclear.

Page 6, line 186 reads "BNF on the other hand has a wider observed range . . .". For a reader it is unclear where the observed range of BNF comes from.

Page 7, lines 202-204 read "Looking at inputs and losses excluding anthropogenic N addition (BNF + N Deposition – N Loss), all the models have a surplus of N and could be said to be 'open' systems with regard to N balance". I am not sure what this means. Recall that after the pre-industrial spin up the sum of all model input N fluxes should ideally be the same as output model fluxes. Was this evaluated? During the transient simulation additional N deposition and fertilizer input leads to increased gaseous losses of N, perhaps increased leaching, and accumulation of N in organic and inorganic pools. I am unsure what 'open' and 'surplus of N' means– does it mean all the additional N input is lost as gaseous fluxes and to leaching. We all know BNF (especially due to increase in crop area) and N deposition increase over the historical period so N balance, as defined in the manuscript, will always be +ve. What's more important here is where does this additional N ends up?

Page 7, line 206 reads " . . . Soil+Litter C is generally low, compared to observational estimates . . .". Does the observation-based estimates contain C from peatlands? The CMIP6 models, I suppose, do not account for C in peatlands and perennially frozen C in permafrost. Could this be the reason for low model estimates.

Page 7, line 209. "Comparing the C:N of Soil+Litter global total weight the ratios are similar across models . . .". This sentence doesn't read properly. Also, this section reports the reason for higher C:N ratio of the soil organic matter in the JSBACH model as "The higher ratio for JSBACH is due to the 10:1 ratio for slowly decomposing soil carbon (humus) and larger ratio for litter". I cannot follow what this sentence is trying to imply. This is true for all models. Soil C always decomposes slowly than litter.

Page 7, Section 3.1. In Figure 4, sub panel, it seems the global model response is compared to observation-based estimate from Baig et al. 2015. Is the Baig et al. 2015 average representative of the whole globe or weighted heavily towards certain geographic regions.

Page 8, lines 226-227 read "Therefore, although the models reach a majority consensus on +CO2 NPP effects overall, the important regional details are still contradictory". Does OVERALL in this sentence means globally? When the manuscript says the "important regional details are still contradictory", I think, it is meant that regional response to +CO2 do not agree amongst models. I think, it doesn't mean they the models contradict some observations because there aren't any regionally aggregated

+CO_2 observation-based responses. Please reword this sentence.

Page 8, lines 235-239. I wonder, if there a way to quantify or plot this dichotomy between +N and +CO_2 responses.

Page 8, lines 255-256. "The largest responses to +N and +CO_2 of input and loss do not necessarily correlate with either N uptake or changes to productivity". I am not sure what this sentence means.

Page 9, line 267. "In contrast, JSBACH has less than half the increase in loss of JULES in the +N simulation". By the time, a reader reaches this sentences he/she may forget what quantity is being referred to. Does this sentence refers to plant N uptake?

Page 9, lines 270-271. "Two of the most important factors for plants' use of N are the availability and demand for N use. The variability of these processes is determined primarily by the BNF and NUE respectively, which are both known to be affected by increased CO_2 and N". This statement is not entirely correct. Variability in N demand is not primarily governed by C:N ratio (which is referred to as NUE in the manuscript). C:N ratio of plants changes gradually. The variability in N demand comes primarily from variability in NPP in response to interannual variability in climate. N availability on the other hand depends on pool sizes of ammonia and nitrate. While, BNF is the primary natural mechanism of inorganic input to soil the subtlety here is that pool sizes do not vary substantially from year to year while BNF does. So, I think, variability in N availability has to be very small. Plant N uptake on the other hand will likely be more variable because both passive and active N uptake depend on variability in climate. Please consider rewording this statement.

Page 9, line 275. "The BNF responses to +CO_2 of the models differ from the average response recorded in a global meta-analysis of CO_2 manipulation (Liang et al., 2016)". Here, Liang et al. is yet another meta-analysis that is being used to evaluate models without properly introducing it first.

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Page 9, lines 279-284. This discussion about BNF is hard to follow.

Page 10, lines 300-301. “The large variations in signal and sign of BNF and NUE response between models suggests there is still progress to be made”. Perhaps reword this as “The large variations in the magnitude and sign of BNF and NUE responses to +N treatment between models suggests there is considerable uncertainty in our understanding”. There are now several meta-analyses (including that of Liang et al. 2016) that clearly show that elevated CO2 leads to increased BNF and studies that show elevated N input decreases BNF. This is also intuitively expected. So, I think, there is sufficient evidence to suggest a real world sign (+ or –) on the response of BNF to these two drivers (+CO2 and +N).

Page 11, line 343, reads “The models mostly represent high latitude northern hemisphere regions less well than other parts of the world, in part because of the unique challenges these areas set for models”. I am unsure how can it be concluded that high latitudes are represented “less well than other parts of the world”. There are no gridded observations for +N experiment. Does this refer to the fact that the models do not agree at high latitudes. If yes, please say so explicitly.

Page 11, lines 345-349. If I am following the manuscript as the authors intend, it seems the complex processes at high latitudes including potential for release in methane, albedo changes with vegetation expansion, and large amounts of C in soil are mentioned as why the +N response in this region is higher than the average seen in LeBauer and Treseder (2008) meta-analysis. I am not sure if I follow this reasoning because it hasn’t been explained how these complex processes are linked to N cycle processes. In addition, were any of the individual studies in the LeBauer and Treseder (2008) meta-analysis performed in the tundra region? If yes, what was their response to +N? What is the northern most study in the LeBauer and Treseder (2008) meta-analysis?

Page 11, lines 357-358 reads “For +CO2 there is the potential for increased NPP because the NUE increases, giving productivity increase without an increase in LAI". I am unable to follow this argument. Isn't is that the productivity increases in the +CO2 experiment simply because of the CO2 fertilization effect? The increase in NPP (due to CO2 fertilization effect) results in a higher C:N ratio of vegetation (which is referred to as NUE), and not caused by C:N ratio as this sentence seems to imply.

Figure 1. Please plot continental boundaries.

Figures 2 and 3. Using similar shades of green and blues for only 5 models is confusing. Please consider using other colours as well.

Figure 3. The arrow for heterotrophic respiration (rh) should come out of the SOM+Litter C pool not the vegetation pool.

Figures 4 and 5. The ratio of small numbers are always misleading and not as meaningful. I am wondering if the geographical plots in Figures 4 and 5 would provide more information if plotted in gC/m2.year rather than percentage change. I realize that the observation-based estimate is in the percentage.

Figure 7. The y-axis titles "BNF response" and "NUE response" are perhaps better written as "BNF change" and "NUE change", although please use C:N ratio instead of NUE .

References other than those that are already in the discussion manuscript

Fageria, N. K. and Baligar, V. C.: Enhancing Nitrogen Use Efficiency in Crop Plants, Adv. Agron., 88, 97–185, doi:10.1016/S0065-2113(05)88004-6, 2005.

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