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Interactive comment on "JULES-CN: a coupled terrestrial Carbon-Nitrogen Scheme (JULES vn5.1)" *by* Andrew J. Wiltshire et al.

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General comments

Introducing a prognostic nutrient cycle, here the nitrogen cycle, into a land surface model (LSM) is a challenging task. As the importance of nutrient limitation on productivity has been clear for a while and we have gone from one LSM with a prognostic N cycle in CMIP5 to several in CMIP6 this is a step all LSM are taking. So for undertaking this task and finishing an LSM that have included all the major N related processes I congratulate the authors. Some processes have been left quite simplistic (e.g. Ngas with its additional turnover) but this is a natural step in the process of developing a modelling framework. The paper goes through the steps they have taken to incorporate the

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Discussion paper

key terrestrial N cycle processes and show how different model setups behaves over historical simulations. These simulations have then been analysed on a global and biome scale and have shown that the model simulates the carbon and nitrogen pools and fluxes comparable to the limited available observations.

The main reason to include a prognostic nutrient cycle is to represent a limitation on plant productivity. The authors have shown that their N limitation is within observation on the biome level, but the global spatial distribution still puzzles me (see general comments). It would also be interesting to see how N limitation affect PFT distributions or at least some mention of it even if N limitation doesn't have any direct influence. In general, it would have been nice to see some perturbation experiments to see how the N cycle would react. Especially BNF and N limitation on productivity. But as this is covered in another paper (Davies-Barnard et al. 2020) it could have been good to refer to those results more than in just a short note in the introduction.

I think this is an excellent model description paper. All the relevant equations and model structures are well documented and described. I would like to congratulate the author to a job well done! Hope my comments will be to some help.

Specific comments

Section 3.1.1 – Biological Nitrogen Fixation feels misplaced in Section 3.1 Vegetation Carbon and Nitrogen. Would fit better in section 3.2 Soil Biogeochemistry together with other N sources and losses that are described here.

Section 3.1.3 – With eqn 12 and that z is the fraction of canopy above current layer, the canopy will always have the same C:N ratio and it will not depend on LAI as it was in Mercado et al. (2007). In Davies-Barnard et al. (2020) it is stated that leaves have flexible C:N ratio. How have I misunderstood this? Yes, leaves have flexible C:N ratios, but the canopy as a whole have a fixed C:N ratio. If the canopy C:N ratio is fixed then there will be a mismatch between canopy N and irradiance compared to Mercado et al. (2007) as irradiance will decrease exponentially through the canopy depending on LAI but leaf N will not. Will this affect the photosynthesis?

L245-248: "If not enough inorganic nitrogen is available, the system is nitrogen limited and an additional term is required in the carbon balance representing excess carbon which cannot be assimilated into the plant due to lack of available nitrogen (Ψc). A positive Ψc results in a reduction of carbon use efficiency." – N limitation only affects NPP and not GPP with an additional respiration term decreasing the CUE. As GPP isn't affected by N limitation then the water demand will stay the same. So the water "cost" for NPP will by higher in JULES compare to models that let N limitation directly affect GPP. Is this something that has been considered during the development?

L271: "The nitrogen available for growth is the total available nitrogen multiplied through by ($1 - \lambda$)." – I assume that the "nitrogen available for growth" is Navail and is used in L283. Navail isn't defined until L378. Please clarify this in the text.

Section 3.2.1 – Does litter and diffused SOM enter frozen soil layers? Could be the reason we see a higher soil C for CNlayer at higher latitudes (Figure 7).

L430-436: – The additional turnover of inorganic nitrogen is a great solution to a well-known issue when soil N starts building up uncontrollable due to N deposition or BNF.

Section 3.2 and 3.3 – A table with constants from sections 3.2 and 3.3 similar to Table 1 for section 3.1 would be a nice addition to the manuscript.

L532-534: – N leach is very small. Any idea why it is so small? Have you considered some adjustments to get the number to increase? Change the value of β?

L538-539 and Figure 4. – Net N mineralisation and N uptake seem to be very small. Are the units for them really Tg N yr-1?

L564-565: "This is a consequence of the higher nitrogen limitation on JULES-CN leading to less litter fall and subsequently less soil carbon." – I guess N limitation on SOM decomposition isn't strong enough to make the SOM pools increase in size? Could it be that the fixed plant C:N ratios prevent feedback of poorer litter quality under higher

N limitation that would result in a slowdown of SOM decomposition?

Figure 1. – Fixation seems to enter the vegetation in the figure, but section 3.1.1 says it enters inorganic N pool. Update figure.

Figure 6. – Is the increased soil C at high latitudes for CNlayer mainly due to the additional decay rate modifier per depth or is it due to N limitation on decomposition? Because with a lot less vegetation C the input of litter must also be less. So something else needs to dictate the build-up of soil C as this is opposite to what is stated in L564-565.

Figure 6, 7 and 9. – Figure 6 is the result we are after when introducing an N cycle, N limitation on productivity. The N limitation spatial distribution puzzles me to some extent. That you haven't investigated the reason for the strong N limitation in tropical savannah (L550-551 "Further work is required to understand why tropical savannah is so limited.") is something I think should have been done. And also that Northern Europe doesn't see any N limitation, but Western Europe does is also strange. I would have liked to have maps for figure 7 and 9 to try and understand this better, now a lot of information is hidden within the latitudinal bands. Also, a figure with annual net mineralisation would be of interest to understand what is happening.

Figure 6, 7 and 9. – How can it be that CNlayer has stronger N limitation at higher latitudes than CN (less Veg C in figure 7 and more yellow in figure 6) when there is more inorganic N in the soil (figure 9)? This needs to be explained better. Is it due to the root profile and that all N isn't available?

Technical corrections

L9: "Biological fixation and nitrogen deposition are external inputs. . ." – From section 3.1.1 it is clear that BNF isn't an external input. Please revise this sentence

L204-205: "We therefore a new parameterisation of retranslocation and labile nitrogen that is dependent on the phenological state" – please revise this sentence

L278: "... is is ..." – remove one is.

L474: "... Equation51 ..." – change to "... Equation 51 ..."

L646: "... residence tome of carbon ..." – change tome to time.

L675: " ... model model ..." – remove one model.

Figure 4. "... period 19960-2005 ..." – correct to 1960.

References

Davies-Barnard, T., Meyerholt, J., Zaehle, S., Friedlingstein, P., Brovkin, V., Fan, Y., Fisher, R. A., Jones, C. D., Lee, H., Peano, D., Smith, B., Wårlind, D., and Wiltshire, A.: Nitrogen Cycling in CMIP6 Land Surface Models: Progress and Limitations, Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-513, in review, 2020.

Mercado, L. M., Huntingford, C., Gash, J. H., Cox, P. M., and Jogireddy, V.: Improving the representation of radiation interception and photosynthesis for climate model applications, Tellus B, 59, 553–565, 2007, https://doi.org/10.1111/j.1600-0889.2007.00256.x.

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