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 produc- tivity 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 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.

We have added a figure showing the fractional distribution of the vegetation and how it changes with the different configurations. We have also extended the discussion section to include next steps and a description of the results in the Davies-Barnard paper.

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.

Thank you for your helpful review comments. As you note we have endeavoured to develop a parsimonious scheme for application in the UK Earth System Model. This is a first step in enabling further representation of the role of nutrients including fully coupling with gas phase chemistry.

In revision we will include reference to the Davies-Barnard paper and other relevant results from CMIP experiments.

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.

This has been moved to the soil inorganic nitrogen section and sign posted earlier on in the text.

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?

Thank you pointing out the issue. In Davies-Barnard, there is an error, in that leaves have a variable C:N ratio with canopy depth, which is not the same as flexible stoichiometry. We will endeavour to correct this in the Davies-Barnard paper through a correction.

Agreed, there is a mismatch between canopy N and irradiance in the current formulation. This is being investigated and will be documented separately and addressed in subsequent configuration updates.

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?

You are correct that N limitation doesn’t directly impact water demand. However, there is an in-direct affect via the coupling between N limitation and LAI. This is something we are aware of and will be taking into account in analysis of CMIP experiments and future model developments. .

L271: “The nitrogen available for growth is the total available nitrogen multiplied through by (1 λ).” – 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.

Corrected

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).

This has been added to the model description: $D(z)$ is the diffusivity in m^2 s^{-1} and varies both spatially and with depth \citep{burke2016gmd}:

\begin{equation} \label{diff}

D(z) = \begin{Bmatrix}

D_o & ; & z \leq 1 m \\

\frac{D_o}{2}(3 -z) & ; & 1 m < z < 3 m \\

0.0 & ; & z \geq 3 m

\end{Bmatrix}

\end{equation}

Without permafrost, D_o (m^2 s^{-1}) is given by a bioturbation mixing rate equivalent to 1 cm^2 year^{-1}. When permafrost is present, the mixing represents cryoturbation and D_o increases to a value equivalent to 5 cm^2 year^{-1}. This parameterisation of $D(z)$ means that the soil organic pools can transfer between permafrost and non-permafrost soils albeit at a relatively slow rate.

We have expanded the discussion around Figure 7 and the vertically resolved soil biogeochemistry to include the "The soil in JULES-CN$_{layer}$ has more organic carbon (Figure \ref{fig:zonal_stocks}), organic and inorganic nitrogen (Figure \ref{fig:fluxes_stocks}). The parameterisation of the vertically resolved soil biogeochemistry means that once JULES-CN$_{layer}$ is spun-up the soil carbon and nitrogen within the frozen soil is relatively stable because of the low temperatures."

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.

Agreed. It is something we plan to investigate in greater depth in the future.

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.

This has been added as Table 2.

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?

We have changed the value of the effective solubility of nitrogen in water and can get an increase in the leaching by doing this. However, it is still fairly small compared with the estimates in Figure 4. One of these reasons is that, in reality, some component of the leaching is from the fertilizer which is not yet included in JULES-CN. We have added a comment to this effect in the document.

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?

These were in the wrong units and have now been updated

L564-565: "This is a consequence of the higher nitrogen limitation on JULES-CN lead- ing 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?

Yes, it is feasible a shift to a lower C:N plant ratio would decrease little quality in turn slowing decomposition. The impact will be dependent on the balance of processes and any change in total litterfall.

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 1 has been eliminated because it is very similar to Figure 4 and supplies no additional information over Figure 4.

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.

In the Nhlat when JULES-C is compared with JULES-Clayers there is a large increase in organic carbon (see Figure 6 in https://gmd.copernicus.org/articles/10/959/2017/gmd-10-959-2017.pdf). In both JULES-CN and JULES-CNlayered the N limitation on decomposition is relatively small. The vertical profile of soil temperature has a big impact on the decomposition in the layered models and allows soil carbon to build up in the deeper soils. The layered model is expanded upon further in the text.

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.

Interestingly, I have changed how to extract the biome specific information out of the model results (medians instead of means) and now we get the savannah and tundra forest being OK limitation-wise but the tropical forests not being limited enough. (it's a bit scary how different the use of a slightly different metric can make the results appear!). We do think, however, that the new Figure 5 and 6 are a more appropriate reflection of each other. This means that we are now interested in why tropical forests aren't limited enough - Phosphorus?. This has been added to the discussion.

We have also added an additional figure which includes of the more relevant N stocks and fluxes and a discussion about this impact of this figure.

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?

Indeed, there are two inorganic nitrogen pools in the layered model - the total pool and the inorganic N that is available to the plants. This depends on the root distribution and on whether the soil is frozen. There may well be less available inorganic nitrogen in JULES-CNlayered than total inorganic nitrogen in JULES-CN meaning that the plants could be more nitrogen limited in some regions. This discussion is expanded in the discussion about JULES-CNlayered.

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

Corrected

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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, Biogeo- sciences Discuss., https://doi.org/10.5194/bg-2019-513, in review, 2020.

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