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

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Wiltshire and co-authors nicely document their additions of a nitrogen cycle and vertically resolved soil biogeochemical model to the JULES model for use in UKESM1. The offline simulations include documentation of simulated vegetation and soil carbon and N pools and fluxes and their change over the historical period. A comparison with some observations is provided for model evaluation

Major concerns

My major concerns aren’t that substantial, but stem from contradictions in what’s expected from the paper and what’s actually delivered.

The paper sets off comparing the C only, CN and CN_Layered implementation of the model, but a number of display items omit results from the CN_Layered configuration. Specifically, Fig. 10-12 & Table 2 do not show results from the layered model, why? Should these effects of vertical soils also be discussed in 5.3? Because these results are not presented, I think major revisions are warranted.

These were not all included so as to simplify the story. However, we will re-examine and add the CN_layered simulations where it is most appropriate in the revised version. This has involved a significant re-write of the Results section which is more comprehensive.

Are there meaningful differences in plant distributions simulated with the new N enabled or CN_layered models?

We have added a Figure showing the pft distribution of the different types of vegetation. This configuration of the model has not yet been brought together with the new height competition which is included in UKESM1 so the exact PFT distributions will change with extra vegetation types and a height-based competition. Therefore, the results are just an indication of the effects of changing the model configuration.

The multi-layered canopy model is introduced in section 3, but never really discussed in section 5. Should it be? Are there any interesting insights enabled by this new feature of the model?

The section has now been extended. The idea behind this section was to document the link between leaf level photosynthesis and respiration and the interactive N scheme. The section has been restructured and updated.

Minor and technical concerns: These are more numerous but intended to clarify and improve the paper.

I like the high-level overview of the main findings summarized in the abstract, but I wondered if more quantitative results should also be provided (pending length requirements for the journal)? We have added a couple of sentences discussing the values of the nitrogen limitation and the carbon use efficiecy to the abstract.

Paragraph starting on line 70. I appreciate how clearly model assumptions are laid out. For example, the approach here looks at the "large-scale role of nitrogen limitation on carbon use efficiency", but I wonder if there's evidence to support this common assumption made in models in real ecosystems? What is the assumed impact of N limitation on NEP? The net results it that is dampens

The introduction has been changed significantly to include these additional bits of information, plus the additional text suggested by the other reviewers.

Can paragraphs around lines 60 & 90 effectively be combined? Both paragraphs seem to have a common purpose of documenting the model connections and history. It's also not really clear how JULES fits into UKESM (also called UKESM1) vs. HadGEM2

We agree this is unclear. We have reworded this so as to make it clearer. We have combined line 90 on into the beginning of the model description section when more details are required.

Is section 2 subheading really warranted? Maybe just combine subheadings for 2 & 3 into one longer section?

Done

There are some redundancies in the text (section 3) where sentences are repeated at different points.

Section 3 has been altered so it is now a general introduction to JULES and the model description below.

Line 162. I'm confused why "These stoichiometric functions already exist in the model" for MR fluxes. This suggests the new work here is just to explicitly represent the Npools that were being implicitly assumed in the carbon only model? Separately, is it worth documenting the source for vegetation stoichiometry (presumably used in Cox et al. 2011)?

This section has been revised to make clarify what is existing and what has had to be extended to have a fully interactive N scheme. The vegetation stoichiometry is also referenced - Enquist, B. J., Brown, J. H., and West, G. B.: Allometric scaling of plant energetics and population density,Nature, 395, 163–165, 1998

Fig 1: The assumption that 'roots' in the model have a lower (or equal) C:N than leaves seems surprising to me, but this but seems contradicted by 'Ratio of root to top leaf nitrogen' (Table 1), please clarify. Roots have wide variation in C:N (Iversen et al. 2017), but if anything I'd assume they should have a higher C:N ratio than leaves (Kattge et al. 2011).

Roots have the same C:N ratio as the top leaf, but as N concentration decreases through the canopy the current formulation means that the C:N ratio is lower. Future work will explore parameterising root C:N ratios directly. We note this in the discussion.

Table 1: "Top leaf nitrogen concentration": listed twice

Removed

Line 175, this statement doesn't seem to be true for grasses, which have declining C:N with height (Fig 2).

Corrected.

Section 3.1.1, oh no, why define N fixation (which should limit NPP) as a function of NPP in the model?! This isn't the first modeling group to make this assumption, but a brief discussion and literature review seems warranted (see Vitousek et al. 2013; Thomas et al. 2105; Wieder et al. 2015; Meyerholt et al 2016)

We have inserted further discussion, including the references suggested.

Section 3.1.1- I think inputs from N fix lead off these details of the CN model because that's where the N cycle 'starts', which seems logical, but putting it under a "Vegetation carbon and nitrogen" (subheading 3.1) seems odd, especially since Nfix contributes to the soil N pool (not plants). Maybe different names for the higher level subheadings (3.1 and 3.2) would be warranted? Alternatively, use Fig 1 to group these fluxes together.

We have tried to sign post the different components of the nitrogen cycle better. The fixation is now included in the inorganic nitrogen section.

Line 182 What is potential NPP? (eq. 9). How does this different than the ‘actual’ NPP? If not discussed here, please reference where this is described (3.1.4).

This section been moved to the Inorganic nitrogen section. NPP_pot is defined very clearly in the vegetation growth and allocation section..

Line 225 where is the multi-layer canopy approach included in these simulations? I’m assuming with with CNlayered, but this isn’t clear in section 4 (line 495)

This has been changed to - “JULES-CN$_{layer}$ is a version of JULES-CN which has identical above ground processes to JULES-CN but additionally includes vertically discretised soil biochemistry.”

What is ‘spreading’ in the model (section 3.1.4)? Is this prescribed by some land use time series dataset or prognostic (more like a DGVM)? Text on page 11 makes me think it’s the later.

This has been added: “Biomass can also increase by spreading through an increase in covered area” where the term spreading has been itnroduced.

The assumptions made in the phenology and allocation section are thoroughly defined, but it’s hard to understand for readers not familiar with TRIFFID how N limitation is implemented in the model. It seems like it’s an instantaneous down regulation of NPP, with extra carbon respired by plants that are N limited? With that N limitation calculated by the tissue and pft specific stoichiometry defined in the model?

Yes, this is correct. The model description has been updated to make it clearer.

Eq. 25-28. I don’t really understand how the soil model is wired based on these equations. If R_DPM and R_RPM are the respiration terms from litter pools, how do some of these fluxes go back into the BIO and HUM pools, which themselves are respired (and also simultaneously included as inputs to BIO and HUM)? It seems that soil respiration fluxes to the atmosphere are actually R_tot*B_R, if so, the R_* fluxes should be some kind of soil turnover term (not respiration).

This has been changed to make the respiration/turnover clearer. New text -”, $R_{tot} = R_{DPM}+R_{RPM}+R_{BIO}+R_{HUM}$ where R_{tot} is the total turnover in kg\,[C]\,m^{-2} s^{-1}. $(1-\beta_R)$ is the fraction of the total turnover that is respired to the atmosphere. β_R depends on soil texture and ranges from 0.75 for a clay soil to 0.85 for a soil with no clay content. From this the respiration to the atmosphere can be defined as $(1-\beta_R)$ R_{tot}.\\”

It seems like B_R is a critical number here, as it controls the soil carbon use efficiency and the amount of N required during litter decomposition (eq. 35). Is this parameter value defined somewhere?

Beta R is now defined in a new equation: \beta_R = \frac{1}{4.09+2.67e^{(-0.079clay)}}

Eq. 29-32 do the N fluxes need to include I_DPM + I_RPM?

No - immobilisation is a microbial process in which inorganic nitrogen is made into new organic matter. Microbes don't make new plant litter (plants make that!), they only produce BIO/HUM. The I_DPM and I_RPM terms are there in I_tot. They're somewhat confusingly named. I_DPM is the immobilised nitrogen that *originated* from DPM.

Line 355, as above can this be called potential turnover, not "potential respiration"?

This has been changed

Eq. 33. I'm trying to wrap my head around the vegetation controls over decay rates and how that may feedback to a CN model that has vegetation with very different stoichiom- etry and N demand (woody vs. grass pfts; Fig 2) but that allows for plant competition (on a single soil column). I assumed the maps of nutrient limitation (Fig. 6) reflect differences in vegetation N demand (per unit of C), but are decay rates also slower for grasses (increasing the N limitation in these ecosystems)?

The interactions and feedbacks are potentially highly complicated given the ability for the PFTs to compete. The grasses produce a higher fraction of decomposable plant material relative to the tree PFTs (0.67 to 0.25, now in Table 1). In turn, decomposable plant material decays approximately 300 faster than resistant material. Grasses therefore have a faster turnover of nutrients. Our interpretation of Figure 6 (now Figure 4) is that it reflects the vegetation N demand. However, more work is required to understand the savannah grass response.

Eq. 36, Is this still a potential decomposition rate, as it's 'limited' by N availability?

This has been changed

Line 385, what are 'these two pools'? I think it should be DMP and RMP, but it's not clear in the text?

This has been clarified – indeed there are the two litter pools

What happens to wood in the soil CN model? How is it allocated to the pools de- scribed?

The ratio of dpm to rpm is a PFT dependent parameter so implicitly takes into account the proportion of wood in a PFT. It is lower for a woody pft and higher for a grass pft. This is discussed at the top of Section 2.2.

Eq. 39, is there anything that prevents this flux from being negative? Are there times when immobilization > mineralization?

Fluxes will have been limited by Fn to make sure this isn't negative. If it hits the minimum pool size, it calculates a correction term (neg_n) and that correction term is then included as a negative gas flux. But that is applied just as an 'extra' gas flux and not applied to minl and immob. So Eq 39 is never negative, but gas flux can be, if that makes sense! This has been added: ". f$_N$ limits the nitrogen fluxes so that (M$_{tot}$ - I$_{tot}$) is always positive. However, if pool sizes become too small N$_{gas}$ could become negative to ensure nitrogen is conserved."

Eq. 39, Should the N loss description go into 3.3 (inorganic N) instead of the soils section (3.2)?

I think it is clearer to have this first component of gas loss here because it is defined using the mineralisation/immobilisation which is discussed here. I agree that it is on the boundary between inorganic and organic nitrogen.

Where does N_turnover flux (eq. 46) go in the model, the atmosphere? How large is this tuning flux relative to other loss terms?

N-turnover flux it has been renamed N_gasl and goes to the atmosphere. This is now discussed in Figure 4 and we state the proportion of loss via this process is about 90% of the total gas loss.

Eq. 46, where does N_gas (eq. 39) fit into the N budget summarized here? Section 4, How does the model handle agricultural fractions of grid cells?

This has been added: The total gas loss is the sum of N_{gasl} and N_{gas} from Equation \ref{eq:ngas}. There are no agricultural fractions represented by this model. Ive stated that there are two gas loss terms.

Section 5.1 I'm used to fluxes and pools being roughly proportional in models like this. If NPP is 11% lower in the CN model, why are the vegetation stocks roughly equal in the C and CN model? Similarly, if the vertically resolved model has a similar NPP to the CN model why are vegetation C pools so different?

This is because the turnover times change – the vegetation and soil turnover times are now plotted separately

Fig 4, 8 and others, Since the text is organized with C, CN, and CN_layered should the display items be similarly organized?

We put JULES-CN first because that is the configuration we are describing as the main focus of the paper. JULES-C is only included in the paper for comparison purposes and JULES-CNlayered is included last because is an extension of JULES-CN. We will check through the text and make sure it is that way in the text. This was particularly relevant when discussing the "historical simulations".

Fig 4 what is the 'N-loss' flux supposed to represent? As drawn, I think this is a gaseous N loss, but as labelled it's not clear how this connects with N_gas and N_turnover fluxes (see above).

The N loss term has been changed to a N Gas term and it is the sum of the gas losses from the inorganic N pool and the organic N pool. This has been added to the captionN- gas is the sum of N_{gas} and N_{gasl} with N_{gasl} approximately 90 \% of the total gas loss

Fig 4, how deep are the soils being represented, this is especially important to consider in the vertically resolved model and should likely be described in methods (3.2.1)

This has been added to section 3: These configurations of JULES adopt the standard 4 layer soils with a maximum depth of 3 m. However it should be noted that \cite{burke2016gmd,chadburn2015gmd} adopt a configuration which increases both the maximum soil depth and number of soil layers.

Line 535, doesn't this just mean the model is at equilibrium as it should be given your spinup procedure?

However, the model could still be in equilibrium with a slower recycling rate. The recycling rate through the system is a characteristic of the model.

Section 5.2 seems out of place, as the extent of N limitation should be preceded by a more thorough comparision of the model states and fluxes. One suggested organiza- tion could be comparing the 1) Spatial distribution of present day stocks / fluxes and residence times (e.g. Figs 4, 10, 7, 9, & 8 in that order) and 2) Temporal evolution of relevant stocks / fluxes over the historical period (e.g. Fig 11 & 12) 3) N limitation (Fig 6, 5) as diagnosed by NPP_Potential / NPP and its evolution over time (Fig 11b).

The results section has been re-worked to make a clearer flow through the figures. Stocks and fluxes followed by N limitation.

The title for Fig 5 (and associated text) implies that you conducted a N fertilization experiment (see Wieder et al. 2019), but I don't think this is accurate. Instead you're calculating a N limitation diagnostic (NPP_pot/NPP) and comparing that to results from an observational synthesis.

Title changed to "Response ratio (NPP$_{pot}$ / NPP achieved)" as in Figure 6. This is defined as "the response ratio, is the ratio of the potential amount of carbon that can be allocated to growth and spreading of the vegetation (NPP$_{pot}$) compared with the actual amount achieved in the natural state (NPP)". Text about the observations is also changed to include: "which summarises a meta analysis of nitrogen addition experiments. The black bars showing the mean of the observations and the red lines the uncertainty."

I'd suggest flipping the order of Figs 5 & 6, as they both show the same information, but Fig 6 is less processed model output, with 5 serving to summarize biome-specific information and related it to observations.

We have switched the order of Figures 5 and 6 as suggested and changed the associated text.

Fig 6, Line 553. It seems like the model is more strongly limited in grasslands, which have much higher N requirements / unit of C (Fig 2). This doesn't really show up in results for 'tundra' or 'grasslands' (only for Savannah). I wonder why?

We have looked at this again. We have decided to use the median of each biome rather than the mean of each biome to calculate the results shown in Figure 6. This is because JULES does not necessarily simulate the correct vegetation for the whole of each biome and outliers will influence the mean. These will not influence the median in the same manner. Looking at Figure 6 the results are more comparable with what we might expect – JULES is not limited enough in the forest biomes but it is now more appropriately limited in the tundra and the savanna. We have also changed the scale of they-axis in the figure to make the results clearer to see.

Should multi-paneled figures be labeled ('a', 'b', 'c') and accordingly described in the figure caption?

This has been done

Fig 7, can legends be smaller or moved into the figures (as in Fig. 9) so the data are easier to read?

This figure has been improved as suggested.

Fig 9, should the bottom panel be labeled soil C residence time and also include data from the C-only model?

This has been deleted and the soil residence times plotted instead in Figure 8.

Fig. 11b what is the time series of 'response ratio anomaly'? Is this the change in NPP_Pot / NPP_act that used to diagnose N limitation shown in Figs 5 & 6? If so, is this what you're calling 'progressive N limitation' (line 599), in which case this should be clarified on and expanded in the text.

We have moved this sub figure to sit alongside the other discussion of Nitrogen limitation and expanded upon it in the text.

Fig 12 & section 5.2.3 The low bias in NEE (0.5 Pg / y, roughly 25%). This would lead to an underestimation of the land carbon sink of about 25 Pg over the period from 1960-2010 (or about 12 ppm CO2 in the atmosphere). Thus, while the IAV of NEE looks better here, that overall magnitude of the land sink may be too low with the CN version of the model. This isn't a deal breaker for the paper but time implications of the low bias with the CN (and CN_layer) model should be discussed in the text, especially since JULES_CN is included in UKESM1.

A discussion has been added in the text. The relationship between JULES-CN and UKESM1 has been made clearer – they are related, but the configuration of JULES in UKESm1 (JULES-ES) has a whole bunch of other components which are not included here and will affect both the vegetation distribution and the NEE.

Section 5.3. Is it just frozen soils that are causing this? it seems the differences in Veg C pools extend down to 40 degrees north (Fig 7). Is this somehow connected to assumptions about the fraction of N that plants have access to in the vertically resolved model (e.q. 51)?

Indeed, the plants with the shallower roosts preferentially take nitrogen from the shallower soils so this process will also contribute to the nitrogen limitation in JULES-CNlayer. This has been added: "This additional limitation of nitrogen uptake caused by frozen soils and the dependence of plant N uptake on root distribution"

Line 611, as noted in Fig 12, the low biases in land C uptake seems notable if you're trying to capture changes in the atmospheric CO2 growth rate.

This is stated here slightly further down: ". Due to nitrogen limitations on CO2 fertilization, mean NEE in JULES-CN (1.66 Pg C/yr) is lower than in JULES-C (2.06 Pg C/yr), and also lower than the estimate from GCP (2.11 Pg C/yr)"

Line 671 what is "climate-induced mineralization" I'm assuming this has something to do with accelerated decomposition from climate change increasing N mineralization rates?

Changed to "accelerated soil decomposition caused by climate change leading to increased mineralisation rates"

Is there a data availability statement required for the journal?

As a model description paper we provide access to the code as documented here and the rest of the JULES model subject to a freely available non-commercial licence agreement. In addition to further encourage and support the use and application of standard 'configurations' we provide access to the 'suites' used here.

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