

Reviewer #2:

The authors represent a model which calculates the DOC concentration to inland waters. They extended the JULES model for DOC, including soil carbon processes and leaching. This manuscript is a step towards a carbon model for aquatic systems and their export to the oceans.

In general, I think that the manuscript is well structured, but the description of the model needs some improvement. I recommend publishing the manuscript in GMD after revision.

Thank you very much for your careful comments. We improved the manuscript following the reviewer's suggestions. Details are given below.

Before I start with my comments, I must point out that I am giving feedback from a modelling point of view. My work on the global aquatic C cycle has just started, but I have a lot of expertise on global modelling.

From that point of view, I was very happy that both the abstract and the introduction start with sentences about global transport to the oceans. The importance of lateral transport is emphasized, but the model description itself does not contain a word on lateral transport. The model is actually a 1-D model and the outcome could be used to transport in the river network.

The second remark is the mentioning of the C cycle in the abstract "A model that represents the whole continuum from atmosphere to land and into the ocean would provide better understanding of the Earth's C cycle and hence more reliable historical or future projection" and introduction "Hence we need to move towards a boundless C cycle model which accounts for lateral fluxes". Why did you choose, after emphasizing C (as in total C) transport, to represent DOC only instead of modelling other species like POC, SOC and DIC as well?

We emphasize the importance of lateral transfers and C cycling along the land-to-ocean aquatic continuum in the abstract and the introduction, as this marks the ultimate goal of our model developments. Nevertheless, our manuscript represents just a first step in that development. At later stages of the overall model development, other processes and C species will be dealt with as well. This is now clarified in the abstract:

"A first and critical step in that direction is to include processes representing production and export of dissolved organic carbon in soils. Here we present an original representation of Dissolved Organic C (DOC) processes in the Joint UK Land Environment Simulator (JULES-DOCM) that integrates a representation of DOC production in terrestrial ecosystems based on incomplete decomposition of organic matter, DOC decomposition within the soil column, and DOC export to the river network via leaching."

As for representing DOC only, instead of POC, SOC and DIC, the reviewer is right, that ideally, we should represent carbon exports in all forms.

According to Meybeck (1982), DOC exports to the coast represent about 37% of C taken up on land from the atmosphere and being laterally exported along the river network. DIC is also a large source of carbon to rivers (potentially larger than DOC), but DIC sources are driven by very different processes such as rock erosion, that are not directly connected to soil organic carbon and the terrestrial carbon cycle. As a first priority, we then decided to focus the JULES developments on DOC processes. Different forms of C will need different processes to be represented in future steps of implementing the land-to-ocean aquatic continuum into the representation of the global C cycle. For instance, the simulation of POC transports would require the representation of erosion, sediment transport and autotrophic production. The representation of DIC would require the representation of weathering processes and water-air gas exchanges. This is now clarified in the introduction:

"Other forms of C need different processes to be represented to fully represent the land-to-ocean aquatic continuum of the global C cycle. Hence future work should include DIC and POC export from soils as well as the fate of all exported carbon in the river system."

Comments/Questions

Abstract line 29-30: I think that part of the leaching to the riverine system is explained by this model. The flux from groundwater or other sources going to the riverine network are not explained by this model. You could shortly elaborate on the relevance or importance of groundwater and give a short explanation on why you ignore it for now. The model comparison is done in the soil and not in the river network.

As in most global land surface models, a ground water aquifer is not directly represented in JULES-DOCM. Runoff from soils is simply represented as two components, a surface runoff and a subsurface runoff. The subsurface runoff includes the drainage from the bottom of the 3m soil column, and thus somehow mimics the ground water base flow, in terms of water as well as in terms of DOC exports. This information is now added to the leaching section in model (p.7, line16-17):

“However subsurface runoff is also representing the drainage from the bottom of the 3m soil column, and thus mimics the groundwater base flow, in terms of water as well as in terms of DOC exports”

Please, also note that in this manuscript, we focus on DOC cycling within the soil column, and we do not yet represent C fluxes in the rivers. For that reason, we compare our simulation results against observed DOC concentrations in the soil solution. Carbon fluxes in rivers are in addition affected by decomposition of DOC, additional sources of DOC from the decomposition of POC and the evasion of CO₂ to the atmosphere. Comparing the simulated leaching of DOC to the river against observed DOC concentrations at some downstream sampling location would not be valid because of the non-conservative behaviour of DOC in the river.

Introduction: Should be more clear on the objective/aim of the model study. Same for abstract.

We agree with reviewer #2 and revised abstract and introduction accordingly.

Page 3, line 12: Why 3 meters deep?

JULES default soil depth is set at 3 meters. The root profile, the soil C stocks and the soil hydrology are all simulated over that 3-meter soil profile, which we used here for the representation of DOC. Moreover, soil depth was not always available at measurement sites. Therefore, we decided to keep the default values for which the JULES model was developed. This information is now added to revised text (p3, l.11):

“The aim of this study is to include a representation of DOC produced in terrestrial soils down to 3 meters (as soil hydrology and Carbon are simulated over 3 meter soil profile in JULES)”

Page 3, line 24: 9 PFTs at global scale. What about crops? They are mentioned in Figure S1. Names and the number of PFT do not match with Figure S1.

We thank the reviewer for this comment. Table S1 is giving Z0 for the PFTs as described in Jobbágy & Jackson data, not the JULES PFTs, sorry for the confusion. We added another table (Table. S2) giving Z0 for the JULES PFTs.

As for crops, in this version of JULES crops are classified as C3 and C4 grasslands. Note that there is a separate version of JULES with improved representation of crops (Osborne et al., 2015), not used here as our main focus is on natural ecosystems

Page 4, eq 2: I think dz should be without subscript (2x). I don't see why it is important to calculate x ?
Remove eq 2?

We corrected the notation of dz .

Please note that x is the ratio of SOC content within the first 1 meter of soil relative to the 3-meter profile for different biomes as given by Jobbágy & Jackson (in their Table. 3) (Jobbágy & Jackson 2000) which is used to extrapolate a profile of soil C concentrations. This is clarified in the revised manuscript.

Page 4, eq 3: I think $z=1$ and $z=4$ should be replaced by $i=1$ and $i=4$. This 1 and 4 is not explained yet (I think they are the four soil layers that will be used).

Corrected. We replaced z with i . This is indicating the normalized weighting factors for all four soil layers (i).

Page 4, line 30-31: These lines do not say anything. Which measurements? When and where taken? Why this remark here? The DOC is not mentioned here. Why are there continuous lines for measurements? For the modelled results? Eq. 3 only gives four outcomes.....

We largely rewrote section 2.2.1, clarifying the approach to distribute organic carbon (calculated as a bulk stock) in the vertical to serve as input for the DOC model.

Page 5, line 3: In figure 1 I see four carbon pools added (two for lock and two for free).

Corrected.

Page 5, eq 4: k is indicator for labile or recalcitrant. But none of the other parameters is dependent on k , so why is k included?

We thank reviewer #2 comment on this equation. We added a subscript k for the soil C stocks Sc (now Sc_k), as the soil carbon pool defines which amounts of DOC produced go to labile and refractory DOC, respectively.

Page 5, line 15: add i subscript in $F_S(S)$ and $F_T(T_{soil})$

Corrected.

Page 5, line 17: RothC formulations. Reference needed.

Added.

Page 5, eq 5: What is the unit of silt and clay?

Fraction. Now it is added. We also changed the values from % to fraction in table 4.

Page 5, eq 6,7,8,9: What is $S_{CARB,DPM}$? Why twice subtracting R_{DPM} ? What is $F_{DOC,DPM}$? Please make the parameter names consistent. This system is solved for each soil layer, so why "i" is not in the equations? These formulas are not clear.

We thank reviewer #2 comment on these equations.

There was some typo in Equation 5. This is corrected now. Variable names have also been checked and made consistent. We also corrected the equations adding “i” to the updates of pools based on the sum of DOC processes in all layers.

Page 5, eq 8,9: I don't understand why part of respiration (R_s) is flowing to BIO and HUM?
Can you take another parameter name for β_R ? Confusing. What is $F_{BIO,IN}$?

In RothC model the assumption is that part of decomposed carbon (B_r) is released to the atmosphere and the remaining fraction ($1 - B_r$) is feeding microorganism in biomass (BIO) pool or is stored in the soil as the recalcitrant form, humus (HUM) with a slowest decomposition rate. These terms are fully described in JULES description model (Clark et al. 2011).

These are clarified in the revised manuscript:

“where in RothC model fraction (f_{DPM}) of litter fall (A_c) is directed to DPM and RPM depending on vegetation type. C pools are subjected to decomposition. Part of decomposed C as a fraction ($1 - B_r$) of total respiration ($R_s = R_{DPM} + R_{RPM} + R_{BIO} + R_{HUM} + R_{DOC}$) is partially feeding microorganisms in soil (BIO) and partially stored as recalcitrant C in soil (HUM) depending on soil texture and the rest (B_r) is released to the atmosphere.”

We changed β_R to B_r , to avoid confusion with the β we use in equation 3.

$F_{BIO,IN}$ is CUE fraction of decomposed DOC which is going back to biomass pool (described in eq 11)

Page 6, line 3: R_s neglects R_{DOC} but it is called total respiration?

R_s in code is indeed the total respiration including the R_{DOC} it in code. This was a typo that we corrected to:
 $R_S = R_{DPM} + R_{RPM} + R_{BIO} + R_{HUM} + R_{DOC}$

Page 6, line 12: add i subscript in S_{DOC} and k subscript in K_{DOC}

Corrected.

Page 6, line 13: add i subscript in $F_T(T_{soil})$. Is this the same parameter as mentioned on the previous page?

Corrected. Yes, it is the same parameter. This is added to revised text:

“ $F_T(T_{soil})_i$ is the soil temperature rate modifier within each soil layer (i) same as in eq.4”

Page 6, eq 11 and 12: Should there not be a sum over k (labile and recalcitrant) in these formulas?

Corrected. We added the sum sign indicating that at the end the BIO_{IN} flux will be the sum of both labile and recalcitrant decomposed DOC.

Page 6, line 24-26: “The assumption ... (k).” I don't know what you trying to say here....

Page 6, eq 14: I don't understand. The size of the labile DOC pool is the old value minus a flux plus total size of the adsorbed pool??

Page 7, eq 15: I don't understand. This means that size of adsorbed pool is equal to F_{AD}_i ???

We thank reviewer #2 for the comment on the adsorption/desorption.

We revised the manuscript:

“For adsorption/desorption, a constant sorption equilibrium distribution coefficient (K_D) is used to partition DOC in dissolved and adsorbed phases. The assumption is that DOC in the labile or recalcitrant pool is proportionally distributed between adsorbed DOC ($S_{DOC_{ad}}$) and dissolved DOC pools (S_{DOC} in soluble phase) depending on K_D from each soil layer(i) and DOC pool (k). Hence if the potentially adsorbed DOC fraction (AD_{pot_i}) compared to the size of the actually adsorbed DOC ($S_{DOC_{ad_{k,i}}}$) is positive then this fraction will be adsorbed and added to the adsorbed DOC pool, and if it is negative then this fraction will be desorbed and added to dissolved DOC pool per model time step.

These terms for DOC labile and recalcitrant pools in JULES-DOCM are as follow (arrow: i and j, Fig. 1):

$$AD_{pot_i} = S_{DOC_{k,i}} \times K_D \times \frac{BK}{\theta v_i} \quad (eq.13)$$

$$S_{DOC_{k,i}} = S_{DOC_{k,i}} - (AD_{pot_i} - S_{DOC_{ad_{k,i}}}) \quad (eq.14)$$

$$S_{DOC_{ad_{k,i}}} = S_{DOC_{ad_{k,i}}} + (AD_{pot_i} - S_{DOC_{ad_{k,i}}}) \quad (eq.15)$$

Also in order to make it easier to read we replaced “locked DOC” with “adsorbed DOC” and “free DOC” with “dissolved DOC”.

Page 7, line 4: add i subscript θv_i .

Corrected.

Page 7, line 7: add k,i subscript C_{DOC} .

Corrected.

Page 7, line 8: do you mean distance between midpoints of the soil layers?

Yes. We replaced “the distance (z_i) between every two soil depths” to “distance (z_i) between midpoints of the soil layers”

Page 7, eq 16: add i subscript in the formula (C_{DOC} and z).

Corrected subscript.

Do I miss which direction the diffusion goes. Does it always go from layer 2 to 1 or layer 2 to layer 3? Then there should be a subscript i,j or something....

Agreed. We changed the subscript to: subscript i for downward flow, and j for upward flow of diffusion.

Page 7, eq 17 and 18: add subscript k and i to the formulas and in the text.

Corrected. We also added that top soil is the sum of first and second soil layer, and bottom soil is sum of thirds and fourth soil layer.

Page 7, eq 17 and 18: It is confusing to have another θ with another unit in these formulas.

We changed θ_s with T_s .

Page 7, eq 19: I should expect that F_P is negative?

F_P is the production of DOC, it is never below 0.

Add all the k and i subscripts to this equation.

Corrected.

Page 7, line 29: What do you mean by main DOC model parameters? In what sense?

Default model parameters. We changed “main” to “default”.

Page 8, line 32: explain A_l/A_p , A/E and C_g

Reference added for soil horizons.

Page 9, line 29: analytical spin-up? What does that mean? Why the assumption that it must be a steady state?

We removed the terms “analytical spin-up”. In order to have the present-day C, we did the spin-up looping 300 times over each site until we reached the steady state for C in soil. This is revised in the manuscript now. “The model was first spun-up looping over period 1996 to 2014 until all the soil variables reached a steady state.”

Page 10, line 1: HWSD global data. Reference needed.

Added.

Page 10, line 6: “test the sensitivity of DOC related model parameters” On what? DOC leaching?

Sorry, this was unclear. We tested them on the DOC concentration in different depths of the soil profile. We added this information in the revised manuscript:

“In order to test the sensitivity of DOC related model parameters on the DOC concentration in different depths of the soil profile, simulations were performed with varying values for z_0 , τ_z and DOC controlling parameters such as $K_{DOC(labile)}$, $K_{DOC(recalcitrant)}$, D_f , CUE , K_D and D (Table 1).”

Page 10, line 6-7: Why are these parameters chosen? These parameters can say something about the inner-sensitivity of the model. But how about the inputs like for example assumptions on PFT or precipitation, temperature, and so on? What about choosing different number of soil layers?

We ran the sensitivity analyses on the rate constants which we took from the literature and which could be subject to a recalibration. Simulation results may as well be sensitive to forcing data used, but that is not the point of this model development study where we used on-site observations of climate instead of global forcing data which would be subject to more uncertainties. In JULES-DOCM, the soil profile depth and number of layers is fixed and cannot be changed, because of the dependence on the representation of soil hydrology.

Page 10, line 6: How can you change β_z ? It is calculated in eq. 3. But that is a normalization?? Should you not change z_0 ? And what are you changing? β_z for each layer?

Reviewer #2 is absolutely right. We did indeed changed z_0 and based on that got the new normalized β_z for each layer. We clarified this in the revised text

Page 10, line 8: Remark. The method of changing one parameter at the time. This is a popular method. However, it renders no information on the effects of interactions of the parameters and that it covers only a limited part of the entire parameter space.

We agree with reviewer #2 that there could be some interactions between sensitivity of different parameters, but testing this was beyond the scope of this study.

Page 10, line 8: Why 50%? 10 or 5% was also enough to say something about the sensitivity around the default values.

Since the derived model parameters from literature already had their own level of uncertainty, for instance CUE which has more than 50% or K_{DOC} with 5-40% of uncertainty, we took the 50% of change to test all the parameters at the reasonable degree.

Page 20: Figure 1 is confusing. All 8 boxes are defined for all the four soil layers, but the diffusion and soil depth give the impression that for example DOC_{lock_labile} only are defined in the deeper soils. Suggestion: split up the figure with the 8 pools (left hand side) and the righthand side the diffusion. Leaching and soil depth and 4 boxes (which are of the lefthand side type).

Corrected as suggested.

Table 2 should be updated with the right subscripts and so on. “i” has not the unit m....

“i” is corrected.

Subscripts are defined within text. In Table 2, we omitted subscripts for reasons of readability.

Table S1: Z_0 should be with lower case characters

Corrected.

Table S4: What are the numbers in the matrix? Leaching? Unit?

Table S4 lists the values for Figure 8 (Relative change in simulated DOC (%) for a +50% and -50% changes in model parameters). We added this now to Table S4 description.

General

Page 15, line 7: “Hence, it is important to introduce a depth-dependence decay rate for these parameters.”. Now the sensitivity is used to draw this conclusion. But why not show the contribution of the DOC leaching of each soil layer? That should give a clear picture. I miss a kind of analysis of the importance of the different processes. Because the model that is proposed here has a lot of parameters. Is it possible to reduce the number of assumptions/soil layers/flows between the different pools? A broader sensitivity analysis could help.

Please note that we are not representing the DOC leaching from each soil layer. As we described in leaching of model, the first and second layer together are considered as the top soil and leaching is taken from it. The update

of these two layers will be based on the proportion of leached DOC compared to the DOC concentration in each of these layers. The same applied to the third and fourth layer as the sub soil leaching.

We are representing only the key processes for DOC including production, decomposition, adsorption/desorption, diffusion and leaching. Hence, we do not feel we could significantly reduce our model assumptions here. That being said, we find that adsorption/desorption was not making any significant change to our simulation and is probably of second order in the estimate of DOC soil concentration and export. We revised the sensitivity section in discussion:

“The sensitivity tests indicate that the parameters controlling SOC concentrations in the soil profile (Z_0 and τ_z) and the recalcitrant DOC residence time (K_{DOC} (recalcitrant)) have the most significant effect on soil DOC concentration, which indicates the importance of factors controlling DOC sources.”

Regarding the change of soil layers/flows please look at answer to your comment on page 10, line 6-7.

What is the used spatial distribution? And the temporal resolution of the input?

As mentioned in our response to reviewer #1, the evaluation of model was performed at plot-scale using 1 dimensional climate forcing, thus no spatial resolution. Temporal resolution of the input is 30 minutes. This is now added to manuscript (p.10, l.6):

“The meteorological forcing is provided at the measurement site level (no explicit spatial resolution) and includes the downward shortwave and longwave radiation at the surface ($W\ m^{-2}$), rainfall ($kg\ m^{-2}\ s^{-1}$), snowfall ($kg\ m^{-2}\ s^{-1}$), wind speed ($m\ s^{-1}$), atmospheric temperature (K), atmospheric specific humidity ($kg\ kg^{-1}$) and air pressure at the surface (Pa) at half an hour time step”