Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-238-RC2, 2019

Interactive comment on "Global database and model on dissolved carbon in soil solution" *by* Joep Langeveld et al.

Anonymous Referee #2

Received and published: 20 July 2019

The authors present a database which they compiled for soil dissolved organic and inorganic carbon alongside a regression model to calculate top and sub soil DOC concentration. I find the paper poorly written and organised and the model is not well constructed, missing major parameters controlling soil DOC. While I appreciate the effort of data collection which could be useful for model evaluation, I do not find model outputs and analyses reliable and do not recommend publishing this manuscript in Biogeosciences in the present format.

Major comments:

The main problems that I see with this manuscript are as follows: Authors constructed a model based on oversimplified parameters which are not fully representative of the processes that are controlling soil DOC. Additionally, they did not use a proper climate

data period. Perhaps their model could work in theory, but in this case it has not as can be seen in results. The bright side of this work was the time that authors spent on collecting the measurements from various studies, although most of them were already reported by Camino-Serrano et al. (2014).

Following some major comments:

1) I search the author's name and paper name on PANGAEA.de and I could not find any database or model codes. It will be useful if the authors provide a direct link to such data. However, I extracted the model results from the provided netcdf file attached to this manuscript. First of all, the time dimension in the model output has 7 layers which seem almost identical to me. What are those 7 layers and why are they almost identical?. Secondly, the model output is for 1970 to 2000 while their database is not in this time period. Thirdly, the model outputs are significantly lower than their reported database and previous studies. I get average of 16.5 mg C L-1 (median:13.7) for the topsoil and average of 2.1 mg C L-1 (median: 0.9) at the subsoil layer. This shows the weakness of model in representing the processes which are controlling the DOC concentration (mentioned above).

2) I noticed in your dataset there are many sites that you only have DOC flux reported. If you do not want to analyse this flux, all those sites should be removed from your dataset. Keeping just sites with observed DOC concentrations will leave only 550 measured points where plenty of them have same sample ID. Hence the claim of having 762 entries and 351 sites is not really true for soil DOC. Also 94 points are non-yearly averaged, with at least 40 of them are only once measured. These data should be removed from data analysis as they cannot be representative of a site. The future corrected model should be run again excluding these points and all result should be corrected.

3) I do not see any model vs measurements validation for subsoil.

Comments/Question:

p1.l15: 2.9 Pg C yr-1 is not the processed fraction but the terrestrial transported flux.

p1.l18: For the most part, every fraction of terrestrial leached C is missed in previous studies, not only groundwater leaching, leading to overestimation of sink capacity of land (Jackson, Banner, & Jobbágy, 2002; Janssens et al., 2003)

p2.l2: Which fraction of DOC you are talking about? Leached or soil solution?

p2.l5-7: Needs reference.

p2.l8: Not correct. Kalbitz found strong or positive influence of pH and C:N on DOC concentration and no trend/influence on C leaching flux

p2.l11: concentration in soil or leaching flux? Moreover, soil DOC concentration changes within depth regardless of transporting period due to organic matter availability within different soil column (Jobbágy & Jackson, 2000).

p3.l7: remove “on”

p3.l17: Which classification was used for your final modelling?

p3.l23:What do you mean by SI1? give a right address to files in that folder

p3.l25: Subsoil DOC concentration cannot be calculated based on topsoil concentration using only simplified “soil class-dependent decay coefficient”. The subsoil DOC concentration, similar to top soil, is mainly controlled by total available SOC not soil class (Jobbágy & Jackson, 2000).

p3.l34: Provide the R script which was used for data analysis

p4.l4: How many sites at the end were used for modelling at the end?

p4.l21: 40 collected samples would be enough for developing a process-based model.

p4.l25: Explain why you exclude the Histosols

p4.l27: Explain the reason for the decreasing DOC concentration with depth, e.g. the

top soil concentration is controlled mainly by production, decomposition and leaching of DOC while subsoil concentration is controlled mainly by advection, diffusion and leaching.

p5.l15: The production of DOC and thus its concentration is controlled by factors such as temperature, C:N ratio, vegetation cover, soil moisture and microbial decomposition. I do not see in your model any of these factors directly applied.

p5.l20: Not true. You could use a global data product, for instance for SOC and pH for the points where you do not have the reported values. You cannot omit these parameters when it comes to representation of soil DOC

p5.l22: As I say, you cannot just ignore the soil properties which are directly controlling DOC processes and flux when global products are available that could be used.

p5.l24: soil class cannot solely represent all the physical and chemical characteristics of conditions which influence the soil DOC concentration. You must include environmental parameters such as soil moisture (DeLuca, 1992; Kalbitz, 2000; Lundquist, 1999; Michalzik, 2001), temperature (Michalzik, 1999; Moore, 2008; Raymond, 2010), pH (Fröberg, 2011; Scheel, 2008), C:N and N effect (Gödde, 1996; Kindler et al., 2011) and soil texture (Davidson et al., 2006; Filip, 1971; Sollins, Homann, & Caldwell, 1996; Stotzky, 1967; Vogel et al., 2015) to have a realistic representation of DOC.

p5.l26: You can find HWSD product which reports SOC directly (Nachtergaele et al., 2010).

p5.l26: No you cannot simply represent temperature and moisture condition by climate zones. You have temperature in your data set. Why not use that as a model parameter? and use global products for soil moisture and missing temperature data.

p5.l30: What do you mean by testing? it is not explained in the method. However, you used a dataset from 1961 to 1990 to represent a DOC concentration until year 2000? how did you fill the data from 1990 to 2000?

p6.l2: As SOC is the main source of soil DOC, all these patterns could be simply explained by SOC distribution in different biomes studied by Jobbágy and Jackson (2000).

p6.l5-11: This belongs to method

p6.l7: You should be careful to not include the above ground or litter DOC measurements in top soil DOC measurements.

p6.l16: The warmer regions, since higher temperature increases the decomposition of DOC, would exhibit lower concentration. But this would not be true in all the cases as the production of DOC can also increase during high temperature (Michalzik et al., 1999) and leaching of DOC out of soil will decrease due to the higher evapotranspiration and reduced soil water (Raymond & Saiers, 2010), resulting in an increase of DOC concentration in some regions. Hence, the authors' model based on climate zones is not valid.

p6.l26: There are many global or regional datasets that you can use for soil texture, SOC and pH.

p6.l27: Where are the results for this statement?

p6.l30: Again, where are the results for this?

p6.l31: First of all this should be in method not result. Secondly, as I mentioned above, you cannot represent correctly the processes which are influencing the soil DOC by these oversimplified factors. I suggest reconsidering your approach. As I see your results Fig.7, your model for topsoil is not capable of capturing properly the measurements at all, low concentration modelled for high measured points and vice versa.

p7. This whole page is poorly written. Back-and-forth between method and some pieces of results, with scattered arguments to support poorly constrained results.

p8. The whole same story defined for the "Top soil" section.

p8.l16:32: This all belongs to method not results. However, I am not satisfied that the subsoil concentration can be represented by only a simplified "soil class-dependent decay coefficient" which is not also well explained in this manuscript.

p9. "Application and perspective": There are more parameters that should be included in your model as mentioned above.

p10.l7: No you cannot simply apply the water flux to the soil DOC concentration and get the leaching of DOC as the DOC removal from soil column applies the changes to the other processes involved in production/decomposition of soil DOC, resulting in change of concentration in soil as well.

Reference:

Camino-Serrano, M., Gielen, B., Luysaert, S., Ciais, P., Vicca, S., Guenet, B., . . . Janssens, I. (2014). Linking variability in soil solution dissolved organic carbon to climate, soil type, and vegetation type. Global Biogeochemical Cycles, (September 2015), 497–509. https://doi.org/10.1002/2013GB004726.Received

Davidson, E. A., Janssens, I. A., Marks, D., Murdock, M., Ahl, R. S., Woods, S. W., . . . Loffler, J. (2006). Temperature sensitivity of soil carbon decomposition and feedbacks to climate change. Nature, 440(7081), 165–173. https://doi.org/10.1038/nature04514

DeLuca, T. H., Keeney, D. R., & McCarty, G. W. (1992). Effect of freeze-thaw-events on mineralization of soil nitrogen. Biol. Fertil. Soils, 14, 116–120. https://doi.org/10.1007/BF00336260

Filip, Z. (1971). Clay Minerals as a Factor Influencing the Biochemical Activity of Soil Microorganisms, 74.

Fröberg, M., Hansson, K., Kleja, D. B., & Alavi, G. (2011). Dissolved organic carbon and nitrogen leaching from Scots pine, Norway spruce and silver birch stands in southern Sweden. Forest Ecology and Management, 262(9), 1742–1747. https://doi.org/10.1016/j.foreco.2011.07.033

Gödde, M., David, M. B., Christ, M. J., Kaupenjohann, M., & Vance, G. F. (1996). Carbon mineralization from the forest floor under red spruce in the northeatern {USA}. Soil Biol. Biochem., 28(9), 1181–1189.

Jackson, R., Banner, J., & Jobbágy, E. (2002). Ecosystem carbon loss with woody plant invasion of grasslands. Nature, 277(July), 623–627. https://doi.org/10.1038/nature00952.

Janssens, I. A., Freibauer, A., Ciais, P., Smith, P., Nabuurs, G., Folberth, G., . . . Dolman, A. J. (2003). Europe ' s Terrestrial Biosphere Anthropogenic CO 2 Emissions. Science, 300(June), 1538–1542. https://doi.org/10.1126/science.1083592

Jobbágy, E. G., & Jackson, R. B. (2000). The vertical distribution of soil organic carbon and its relation to climate and vegetation. Ecological Applications, 10(2), 423–436. https://doi.org/10.1890/1051-0761(2000)010[0423:TVDOSO]2.0.CO;2

Kalbitz, K., Solinger, S., Park, J.-H., Michalzik, B., & Matzner, E. (2000). Controls on the Dynamics of Dissolved Organic Matter in Soils A Review. Soil Science.

Kindler, R., Siemens, J., Kaiser, K., Walmsley, D. C., Bernhofer, C., Buchmann, N., . . . Kaupenjohann, M. (2011). Dissolved carbon leaching from soil is a crucial component of the net ecosystem carbon balance. Global Change Biology, 17(2), 1167–1185. https://doi.org/10.1111/j.1365-2486.2010.02282.x

Lundquist, E. J., Jackson, L. E., & Scow, K. M. (1999). Wet-dry cycles affect dissolved organic carbon in two California agricultural soils. Soil Biology and Biochemistry, 31(7), 1031–1038. https://doi.org/10.1016/S0038-0717(99)00017-6

Michalzik, B., Kalbitz, K., Park, J., Solinger, S., & Matzner, E. (2001). Fluxes and concentrations of dissolved organic carbon and nitrogen–a synthesis for temperate forests. Biogeochemistry, 52, 173–205. Retrieved from http://link.springer.com/article/10.1023/A:1006441620810

Michalzik, B., Michalzik, B., Matzner, E., & Matzner, E. (1999). Dynamics of dis-

solved organic nitrogen and carbon in a Central European Norway spruce . . .. European Journal of Soil Science, (December), 579–590. https://doi.org/10.1046/j.1365-2389.1999.00267.x

Moore, T. R., Paré, D., & Boutin, R. (2008). Production of dissolved organic carbon in Canadian forest soils. Ecosystems, 11(5), 740–751. https://doi.org/10.1007/s10021-008-9156-x

Nachtergaele, F., Velthuizen, H. van, Verelst, L., Batjes, N. H., Dijkshoorn, K., Engelen, V. W. P. van, . . . Montanarela, L. (2010). The Harmonized World Soil Database. Proceedings of the 19th World Congress of Soil Science, Soil Solutions for a Changing World, Brisbane, Australia, 1-6 August 2010, 34–37. https://doi.org/3123

Raymond, P. A., & Saiers, J. E. (2010). Event controlled DOC export from forested watersheds. Biogeochemistry, 100(1), 197–209. https://doi.org/10.1007/s10533-010-9416-7

Scheel, T., Jansen, B., Van Wijk, A. J., Verstraten, J. M., & Kalbitz, K. (2008). Stabilization of dissolved organic matter by aluminium: A toxic effect or stabilization through precipitation? European Journal of Soil Science, 59(6), 1122–1132. https://doi.org/10.1111/j.1365-2389.2008.01074.x

Sollins, P., Homann, P., & Caldwell, B. a. (1996). Stabilization and destabilization of soil organic matter1.pdf. Geoderma, 74(1–2), 65–105. https://doi.org/10.1016/S0016-7061(96)00036-5

Stotzky, G. (1967). DIVISION OF ENVIRONMENTAL SCIENCES: CLAY MINERALS AND MICROBIAL ECOLOGY*,†. Transactions of the New York Academy of Sciences, 30(1 Series II), 11–21. https://doi.org/10.1111/j.2164-0947.1967.tb02449.x

Vogel, C., Heister, K., Buegger, F., Tanuwidjaja, I., Haug, S., Schloter, M., & Kögel-Knabner, I. (2015). Clay mineral composition modifies decomposition and sequestration of organic carbon and nitrogen in fine soil fractions. Biology and Fertility of Soils, 51(4), 427–442. https://doi.org/10.1007/s00374-014-0987-7

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-238, 2019.