Geosci. Model Dev. Discuss., 8, C3920–C3927, 2016
www.geosci-model-dev-discuss.net/8/C3920/2016/

Geoscientific Model Development Discussions

Interactive comment on "An integrated Dissolved Organic Carbon Dynamics Model (DOCDM 1.0): model development and a case study in the Alaskan Yukon River Basin" *by* X. Lu and Q. Zhuang

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Received and published: 1 February 2016

Contribution: The authors have developed a physically based catchment-scale model of DOC dynamics in permafrost areas. The scientific community would certainly benefit from a sound physically based DOC model and I applaud the authors' attempt. Unfortunately, the implementation and presentation of the model lack scientific merit and clarity. The study is in its current format not reproducible. Interpretations are generally unsubstantiated and do not agree with any previous observations of DOC dynamics in northern areas, as far as I am aware. As is, this manuscript is far from publishable.

As far as I can tell, the physics seem ok, although I have not done a thorough dissection of the mathematics. However, I do have some criticism regarding the implementation, interpretations and communication. I have detailed my concerns in a number of general comments below. I have also identified some of the main issues in a couple of detailed comments at the end.

1. What is the purpose of this model (and study)? Important objectives of modelling are e.g. to test hypotheses or to use the model to test scenarios and making predictions. In both of these cases, but especially when making projections, the interpretations of the simulations are dependent on the model performance when simulating historical data. The problem here is that model results are not compared to any observed data. In the Supplement there are figures comparing simulated and observed discharge, but there are no statistical measures of model performance (goodness of fit). In addition, the simulated DOC concentrations are compared to concentrations estimated from remote sensing, which are highly uncertain. Why is that? There are observations of river DOC in the Yukon River and some of its tributaries. Why was not those data used? Besides the results currently reported, I would like the authors to include figures showing time series of simulated and observed DOC concentrations in the river. Judging from the figures in the manuscript, the model does a pretty bad job of simulating river DOC. Why should I trust a model that cannot reproduce historical observations?

2. The authors should consider and discuss model uncertainties in depth. Such a complex model, with a lot of parameters, will be inherently uncertain. Could you provide uncertainty bounds of simulations? See work by Keith Beven (e.g. "A manifesto for the equifinality thesis", Journal of Hydrology, 2006 or his book from 2009).

3. I lack assessments of how realistic the model results are. There are several results reported that I found strange. Generally, the study lack comparisons of simulations to actual observed data in the area. For instance, are the results concerning soil DOC concentrations and dynamics realistic? The authors state that the model simulates low soil solution DOC concentrations in summer, although DOC production apparently

is high in summer. Does this correspond to observations in the area? It is definitely opposite to observations in other areas, e.g. boreal forests (see e.g. Fröberg et al., 2006, Biogeochemistry), where maximum soil solution concentrations occur in late summer-early fall. Are amounts and patterns of overland flow realistic? Is snow depth realistic? A snow depth of <1 mm seems strange in an area that, according to the authors, receive >500 mm or precipitation each year. I would guess that at least 50% of this comes as snow. See further comments about figures and tables below.

4. Why the focus on overland flow? I would guess overland flow is a minor component of the hydrology in this system. Boreal wetlands, which are near saturation all year around, get diluted during snow melt, but overland flow is still a minor contributor to streamflow, except for short periods of time when "new" water is up to 70% (see e.g. Laudon et al., 2004, 2007 and 2011). The authors claim that subsurface flow is only 1-2% of the total water flux. How much of the streamflow is routed through overland flow? There are lots of studies showing that subsurface flow is the dominant contributor of streamflow in most environments. McNamara et al. (1997) found that 50-80% of streamflow during summer storms in an Alaskan watershed was "old" water, i.e. water routed through the subsurface. Also, Olefeldt & Roulet (2014) found that streamflow in a subarctic catchment generally was dominated by subsurface flow. Only during snow melt was streamflow dominated by precipitation/snow (still <75%). I find the numbers reported here unrealistic for a natural environment. Subsurface flow of 1-2% could perhaps occur in impervious areas in urban environments.

5. Equally confusing is the large difference in water flux and DOC flux. Water flux from the subsurface is only 1-2% but DOC flux is 30-50%. Once again, this is contrary to previous findings (see e.g. Ledesma et al. (2015), Grabs et al. (2012) and Winterdahl et al. (2011)). The research groups of Kevin Bishop and Hjalmar Laudon have studied this in detail in boreal systems and found that DOC export in the soil follows the water flow.

6. R2 of simulated versus estimated concentrations are reported in the Supplement,

but a better measure would be to use e.g. the Nash-Sutcliffe efficiency index. The same goes for discharge, as noted by one of the reviewers.

7. Contrary to one of the reviewers, I do not find the manuscript well written. There are several spelling errors, grammatical errors and confusing or strange wording. The authors should especially note the use of tense and the usage of the definite article (the word "the"). The structure of the manuscript could be improved. Now, parts that are results are reported in some kind of methods section. At the same time, some of the methods are not mentioned until the Results and Discussion section. I suggest adding a Methods section describing all analyses used in the manuscript. The description of the model is at times very confusing. The structure of the description could probably be improved and combined with more illustrating figures. The authors include two schematic figures to illustrate the model structure but they leave a lot of processes out. Why not have an overarching schematic figure of the entire model, but then detailed figures of each compartment (e.g. soil column, river water, soil:river interface, vegetation). Each arrow in these figures could be labeled with the corresponding equation numbers in the text. The use of parameter and variable symbols is confusing. C is e.g. used for both concentrations and head capacity. The only difference between the two is the number of apostrophes (′) following the C. The use of the apostrophe is unfortunate since this symbol sometimes is used for derivatives in mathematical notation (i.e. $f'(x) = df/dx$). A table with all parameter and variable symbols would help here. All in all, the description of the model is currently not entirely reproducible. Parts of the text is unclear making it difficult to separate the results of this study from previous results or background (which is without references).

8. Some of the terminology is unclear to me and several terms remain undefined. One term that I found particularly confusing was "overland depth". Is this the amount of water in overland flow or is it the depth of overland flow, i.e. the actual sheet of water flowing at the ground surface? And how is overland flow defined here? Is it Hortonian flow, i.e. saturation excess overland flow, or something else?

9. The description of the study area is very short and leaves out a lot of relevant information (e.g. observed temperature, precipitation, topography, type and extent of permafrost). And why was this particular catchment chosen for this study? In addition, the description of the study area should probably come before the model description in this case, or at least before the Validation section.

10. Why the rough DEM (4 km)? There are DEMs with more or less global coverage with a resolution of at least 90 m (SRTM). A pixel size of 4 x 4 km in a catchment of 6000 km2 is very rough.

11. How does the estimated river geometry relate to estimates based on equations in Raymond et al., 2012, Limnology & Oceanography: Fluids & Environment?

A few detailed comments:

Abstract: The abstract is staccato with a lot of short, incoherent sentences.

Page 10414 line 9-13: What kind of models do you refer to? Soil column models? Catchment models? If catchment models, you might want to consider including Futter et al. (2007, WRR), Boyer et al. (1996, Ecol. Mod.), Seibert et al. (2009, HESS), Winterdahl et al. (2011, WRR) and Jutras et al. (2011, Ecol. Mod.).

Page 10415 line 10: So, is your assumption that overland flow is the dominant pathway of DOC transport? This does not agree with most studies of DOC dynamics in catchments.

Page 10416 line 23: Here you use general units (e.g. L/T), but further on you use specific units (e.g. J/m3/K or J/kg on page 10418). Why is that? You should be consistent in your use of units.

Page 10423 line 3: Why 1/6? Is there any justification for this choice?

Page 10427 line 2: So here K is transmissivity. Does it vary with depth? Otherwise it is rather hydraulic conductivity that you refer to.

Page 10428 line 22: Is Darcy's law = equation 21?

Page 10429 line 21-28: This section should go into the Results and discussion paragraph.

Page 10430 line 4: What do you mean by "not significantly affected by the exterior stream sources"?

Results and discussion: The discussion part is incoherent and many of the interpretations differ from most (if not all) studies on DOC dynamics in northern environments. There are also several unsubstantiated claims without any references or data to support these claims. As a matter of fact, there are almost no citations at all in the discussion. In addition, there are several analyses in the discussion that are not related to any methods. I suggest the authors include a Methods section including detailed descriptions of all methods used including statistical analyses.

Page 10430 line 22: the point is red in the figure. Why did you choose this point? Seems arbitrary.

Page 10432 line 8-13: What do you base these interpretations on? Do you have any statistics that back this up?

Page 10432 line 19: What is "compound topographic index"? Topographic wetness index? If so, cite the reference and include the description of this analysis in a Methods section.

Page 10432 line 26: The maximum DOC concentration you mention is that in the soil or in the river? If it is in the soil, it is contrary to what has been found previously (see e.g. Neff & Hooper, 2002).

Page 10432 line 27-29: I do not follow the logic in this sentence.

Page 10433 line 14: Where does the rest of the water go? To evapotranspiration (ET)? If temperature is increasing, ET should also increase and thus decrease the amount of water transferred to deeper soil layers.

Page 10433 line 24: How can overland flow have high or increasing concentrations? Overland flow, i.e. addition of event water, usually dilute stream water thereby resulting in decreasing concentrations in surface waters.

Page 10433 line 27: Define "poor organic matter content".

Table 1: Check for spelling errors! None of the processes are spelled correctly!

Table 2: Are these numbers correct? If I understand the table correctly, the DOC production can only account for about 9% of the sorption of DOC, 43% of the mineralization, and 75% of the transport of DOC in 1976. Where is all the other DOC coming from? Could we please have figures of time series of DOC concentrations in soil (at a certain depth) and in the river (at a certain pixel)?

Figure 4: I would say the model does a pretty bad job of simulating river DOC, especially in B and in the upper portions of the catchment. What are the differences in %? In certain places it seems to be about 100%.

Figure 6: Is the snow depth realistic? I guess it is reported as water equivalents but then the maximum snow depth is about 0.7 cm. How does that compare to observed snow depth at the site? Also, how can the overland depth, whatever that is, be about 15-20 mm during snow melt, while the actual snow melt is less than 2 mm, infiltration is about the same as snow melt and the snow depth is less than 1 mm? Is all that water coming from uphill?

Figure 7: Are soil concentrations realistic (<15 mg/l)? Studies from boreal forests indicate substantially higher concentrations. Why do you use the unit g/ml?

Figure 8: Are the units correct in figure 8? For example, the depth of overland depth is indicated in meters, but in figure 6 the overland depth is in mm. Also, in figure 8 you use the unit g/m3 for DOC. Why is that? In a previous figure you use g/ml. Confusing. Adding to the confusion is that colors in figure 8 differ between panels

c) and d) compared to e) and f). If I read this figure correctly, overland flow is non-existent in the lower portions of the catchment and around the river. Is this realistic? Considering the wetness index, I would expect these areas to receive most of the water and thus being the wettest areas of the catchment. Instead, there are areas in the upper parts of the catchment with overland flow up to 0.3 m deep (if the units are correct)! I find this unrealistic. If the area is entirely impermeable this would mean that these pixels receive 300 mm of water in one day! 300 mm is about 50% of the annual precipitation (according to the numbers in the manuscript).

Figure 9: Changes are reported as fractions? Wouldn't % be a more intuitive unit? And what is the change in relation to? Between years? The figure does not mean anything by itself; you really have to read the text to get anything out of it.

Figures F1 and F2: Could you add panels with log scale on the vertical axes to these figures? The model does a poor job of simulating discharge in summer, but it is difficult to see during low flows. Log scales would make comparisons easier. Some of the problems with hydrological and biogeochemical models probably stems from our lack of understanding of permafrost hydrology. This is e.g. evident in figures F1 and F2; the model simulates no streamflow during winter, but in reality there is flowing water in the river, albeit with a very low discharge. The same applies for all large rivers in permafrost environments; there is streamflow during winter so there must be moving water in the soil.

Interactive comment on Geosci. Model Dev. Discuss., 8, 10411, 2015.