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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

Anonymous Referee #2

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General Comments

The authors have developed a process-based model to evaluate DOC dynamics and transport in a small watershed within the Yukon River basin in Alaska. The model incorporates many important components, including vegetation, hydrology and soil thermal dynamics, and DOC cycling parameters. The development of this model is an important to the broader scientific community, as the flux of DOC from high-latitude watersheds is an important part of the terrestrial carbon balance, and high-latitude soils store large amounts of organic carbon. I am not a modeler, and cannot evaluate all of the

parameterizations in detail. However, the authors have done a good job of providing source citations and describing equations, both in the primary manuscript and in the supplemental materials. The manuscript is generally well written, although I do have some more specific comments below.

Specific Comments:

1. Page 10413, Line 15 – Changing wording from “lost” to “stabilized” 2. Page 10413, Lines 25-26 – I would omit Dutta et al. 2006 as a citation, since that was primarily a fractionation and incubation study. Perhaps cite something more specific to high-latitudes, such as:

Finlay et al. 2006. Snowmelt dominance of dissolved organic carbon in high-latitude watersheds: implications for characterization and flux of river DOC. Geophysical Research Letters

3. Methods Section – It would be useful to highlight similarities and differences between this model and other similar models. For instance, how does this model compare to recent publications by Jiang et al. (2012) regarding heat and water movement? How does the subsurface flow component compare to models such as SUTRA-ICE (McKenzie et al. 2007) or MODFLOW (Walvoord et al. 2012)? 4. Section 2.3.1 - Sorption may be an important process in some locations where active layer thickness is increasing, resulting in an increase in mineral surface area and sorption potential (Striegl et al. 2005; Kawahigashi et al. 2004, 2006). I’m having a hard time determining if the model can account for this process, and to what extent are these parameterization depth-dependent in the soil profile. 5. Section 2.6 – Does the small watershed used as a case study have a name? Why did you choose this particular site? 6. Figure 2 – Add more text to the legend to better describe flows and compartments 7. Figure 4 – I don’t think the river network and background images is helpful in illustrating these patterns between satellite-derived concentrations and simulated concentrations. I would consider re-plotting as concentration vs. longitudinal distance along stream reach. You

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could add a second panel to plot modeled vs. observed using regression techniques too. 8. Figure 6 – Increase font for axis labels and tick labels; also “precipitation” is misspelled. 9. Figure 7 – Increase font for axis and tick labels 10. Figure 8 – What are panels e and f? They are not described in the figure legend. 11. Discussion – There have been significant advances in field and lab studies related to DOC lability and reactivity in recent years in the northern permafrost zone. See for instance: Mann et al. 2013, 2015; Spencer et al. 2015; Vonk et al. 2013, 2015; Wickland et al. 2012. I think describing the path forward by coupling these field observations to improve model performance and to reduce uncertainties could strengthen the Discussion.

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