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***Interactive comment on* “Quantification of DOC concentrations in relation with soil properties of soils in tundra and taiga of Northern European Russia” by M. R. Oosterwoud et al.**

Anonymous Referee #1

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General comments The regulation mechanisms of the DOC release from tundra and taiga soils in view of the prospected climate warming in that area is of central scientific interest and consequently within the scope of Biogeosciences. The authors present field data on DOC concentrations and DOC fractions (FA, HA, Hydrophilic) from the organic layer and the mineral soil along two soil transects each one from a tundra and taiga area. Field sampling was done over two months covering the final stage of the snow melt and the early summer season. Field data were accompanied by laboratory data on soil properties encompassing soil pH, water extractable amounts of organic carbon and on the contents of different fractions of sesquioxides. However, the ms needs mayor revision, editing and language checking. There are a lot of uncertain-

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ties and shortcomings with regard to the methodology, the site description, the use of technical terms regarding the DOC fractions and with regard to the degree to which conclusions are supported by the data.

Specific comments Title The present title does not match the idea of this ms. Therefore, I suggest to change the title into “Variability of DOC concentrations related to soil properties of tundra and taiga soils along two toposequences in Northern European Russia“.

Introduction Please check the definitions you used on fulvic and humic acids on the one and hydrophilic acids on the other hand, or describe the fractionation method applied in more detail (see below). So far, I have the feeling that this is a mix-up of two different methods fractionating DOC. Fulvic and humic acids are determined by the classical humus fractionation method using acids and bases, whereas the term hydrophilic/hydrophobic acids/compounds is based on the Leenheer fractionation according to differences in molecular charge characteristics consequently distinguishing into 6 fractions: hydrophobic and hydrophilic acids, bases and neutrals. Please check by these references (Leenheer and Huffman (1976) Classification of organic solutes in water macromolecular resins. J. Res. U.S. Geol. Survey 4:737-751. AND Leenheer J.A. (1981) Comprehensive approach to preparative isolation and fractionation of dissolved organic carbon from natural and waste waters. Environ. Sci. Technol. 15:578-587.) With regard to the variety of already existing terms for different fractions of DOC please stick to the term WSOC (water soluble organic carbon), which is already established in ecosystem research, instead of this new one called EOC (extractable organic carbon). Due to the low amount of new information, please leave Fig 1 out. Please, use Corg for the organic matter content in soils instead of CT.

Materials and methods Site description and sampling strategy Here, I miss climatic data on the rain amounts and distribution and on the daily air and soil temperature during the sampling period in June and July 2008. How big were the two catchments and where in this catchments were the soil transects established? At which locations,

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the stream samples were taken? Please make it clearer by supplying a map on the required topographic information. Fig. 3: What do the different colors/gray tones mean (lacking legend)? Please, add more detailed information on the sampling procedure: How many replicates, how long were the sampling intervals, what kind of suction was applied to gain which kind of soil solution from which pore sizes (macro-, meso, micro pores)?

Chemical analysis You stated that EOC was extracted from field moist soil using a soil to water ratio of 1:10. How did you correct the soil to water ratio for the differing soil water contents and did you relate the EOC amounts to kg dry weight? In view of the highly variable soil water contents between 6 to 1180 % g/g this is of central importance. Please, describe the DOC fractionation method in more detail (see also comments above).

Results and discussion **Field description** In general, the descriptions of the organic layers (main source for DOC), the soil types and the underlying bedrock are a little bit too short, due to the fact, that the amounts of the analysed sesquioxides are basically inherited from the geological conditions and modified by soil processes. So, what kind of “organic top layers” or forest floor did you identify? Mull, moder, raw-humus type? Regarding soil type classification according to the WRB, the descriptions of the diagnostic horizons building up the different soil types appeared quite rough (Tables 1-3). Just to mention one soil type as an example: The Histic Gleysol of the tundra area is described as Of (?), OAm, AC/BC (Table 2). What does that mean? A Gleysol needs an oximorphic Bg horizon and a reductomorphic Cr horizon. From the descriptions you provided, the soil classification did not become transparent. Please, add some more information on the diagnostic properties of the soil horizons.

Soil properties In general, in Fig. 4, 6, 8, 9 which data were used for these regressions?

CEC The CEC is determined by the soil texture (clay content), the humus content and principally by the pH. Since the soil pH varied considerably within the soil profiles,

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especially in the tundra soils, did you check for a relation between EOC and soil pH? Or a stepwise regression between EOC, pH, humus content (Corg multiplied by 1,72) and soil texture (if available)?

Soil solution chemistry DOC P 11, L 20/21: The authors stated that “Soil solution of taiga soils have larger DOC concentrations than tundra soils (Table 3), despite that both have comparable total organic carbon”. What does this mean? Comparable Corg contents? To relate DOC concentrations to soil organic carbon or other soil properties, it would be more useful to calculate the Corg pool per horizon (taking into account the depth of every horizon and its bulk density) rather than the Corg contents. So, could you please provide the Corg pools for every horizon within the soil profiles and correlate this to (mean) DOC concentrations. DOC concentrations are largely governed by the amount of rain water passing the soil profile and by the organic material from which they derive. Since data on rain or water fluxes and litter decomposition rates (e.g. litter bag experiments) are missing, the authors’ assumptions on OC decomposition and DOC production are not tested and should be therefore left out.

Soil solution leaching into streams In general, I do not understand the caption of Fig. 7. Where are the data of the tundra, where of the taiga site and what does “(b): June and, (c): July” mean? How realistic are stream water temperatures between 5 and 23°C? P 16, L. 21-26: The authors suggest different water flow paths for the tundra and the taiga transects through seasons affecting the DOC concentrations. However, studies on the water flow paths were not carried out. Additionally, solution sampling just covered two months, so that this statement appeared too far-reaching.

Potentially soluble organic carbon and DOC The authors compare amounts of EOC and CT (g C/kg soil) rather than C pools (gC/ m² and soil depth) therefore a comparison with regard to proportions EOC in Corg between soil profiles and experimental sites is not possible. This might be the reason why the authors did not find a correlation between Corg and EOC (P 18, L 11-13).

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Conclusions To me the conclusions drawn from this study appear too generalizing and far-reaching. Besides, due to the short-term (two months) measurements of DOC concentrations and the lack of data on climatic conditions, hydrological flow-paths, soil organic carbon pools and decomposition rates, the comparability of results and thus the degree to which conclusions are supported by the data is not given.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/7/C1357/2010/bgd-7-C1357-2010-supplement.pdf>

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