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Interactive comment on "Carbon dynamics in boreal peat-lands of the Yenisey region, Western Siberia" *by* E. D. Schulze et al.

Anonymous Referee #4

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This study uses ^{14}C measurements of peat in combination with extensive plant residue analyses to describe organic matter accumulation dynamics in a Siberian peatland. This is a very comprehensive and useful dataset. However, I recommend that the authors consider clarifying several aspects of the manuscript, especially detailing for a non-specialized audience the rationale for the method used to fractionate peat for ^{14}C analysis, as well as clarifying the terminology employed, which is potentially confusing with respect to the definition of DOC.

Overall, the study would benefit from posing clear motivating questions up-front as to why the authors would expect accumulation rates and ages to differ in this region as opposed to other previously-studied Siberian peatlands. Presumably, a main question of interest is whether infiltration of younger DOC to deep peat layers confounds inter-

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pretations of when peat initially began to develop in the region, and by proxy at other boreal sites. However, this question does not explicitly emerge until well into the Discussion. Highlighting this point might increase the accessibility and relevance of the paper to a broader audience. For example, you could add text at the top of 11282 where you propose that these published ^{14}C dates may underestimate the time period when peat began developing in the region.

A distinct aspect of the paper is the use of a base separation on a restricted particle size fraction to isolate samples for ^{14}C analysis. More discussion of the rationale and justification for employing this method should be included. I infer that the reason for doing so was to remove younger DOC to get at the ages of initial peat deposits. However, comparisons with other published papers at the regional scale are then complicated two-fold. First, only ^{14}C ages for the $>$36 and $<$63 um fraction are presented, thus excluding larger and smaller particles. Secondly, a base extraction would not only remove DOC in the peat matrix derived from other sources, but also in-situ decomposition products that were adsorbed to the particulate matrix. Thus, this appears to be a good method for isolating oldest peat to assess the time of vegetation establishment, but cannot be used to infer "peat age" per se, which is a conceptually different measurement. Thus, comparisons with other studies are analogous to an "apples to oranges" comparison. It would be very useful to know the fractional contribution of the $>$36 and $<$63 um fraction to the bulk peat as a whole. Is this the dominant size fraction, and why was it chosen? Why not just conduct a base extraction on the bulk peat?

Finally, I would argue against describing the supernatant solution of a base extraction as "dissolved organic carbon." This generates confusion with the traditional definition of DOC as carbon that is soluble under ambient environmental conditions. Rather, your supernatant yielded "base-soluble organic carbon" and should be described as such so the casual reader does not take the data out of context with how the term is usually used.

Finally, I do not agree with the statement made in the abstract: "This peat is older

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than previously reported mainly due to separating particulate organic carbon (POC) from dissolved organic carbon (DOC), which was 1900 to 6500 yr younger than POC." Rather, the peat matrix may be older than previous reports of bulk peat ages, but we cannot make an apples-to-apples comparison here. Comparisons with other systems would need to be made on the same basis as the other measurements. The bulk peat ^{14}C is an informative ecological measurement, and it would have been helpful to present this data, especially for comparison with the other studies.

Specific comments

Figure 3: Convention in soil figures is to have deeper depths on the bottom of the figure for ease of visual interpretation.

Section 2.2 presumably reports data collected using methods described in section 3.2, so thus more properly belongs in the Results section.

Section 3.3: I am concerned about the bulk density measurements; a 3.5 cm diameter core is quite narrow and would presumably compress the sample. How was this accounted for?

Section 3.4: The methods for separating DOC are unclear to me.

Section 4.3: as a point of clarity, citations comparing the present results to previously published data more properly belong in the Discussion, not the Results

Page 11291, line 20: This paragraph belongs in the Results.

Page 11293: "Following Darcy's law (Nobel, 1991) the flow of water through a saturated substrate is determined only by the pressure difference between the peat surface and the drainage system and not by the hydraulic conductance"

This statement appears incorrect, as Darcy's law states that Q/A = -k dh/dl,

so flow thus depends on hydraulic conductance, which should vary with bulk density and other peat properties.

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Figure 7 is not cited at all in the results, where this data should be presented.

11293, line 9: I am unclear how Figure 7 supports this statement, please explain.

11293, line 17 “The present hydrological balance of the growing season is close to nil” I am unclear what you meanâĂŤdo you mean that the Pine forest is never flooded during the growing season?

11295, line 7: Also presumably by diffusion and advection, given your subsequent statements.

3.4 20: One does not “compare the ^{14}C spectra of the AMS with the ^{14}C standard.” Do you mean that you used Δ ^{14}C of the samples for calibration with Oxcal?

4.1 20: Do you mean the ages of DOC and POC were related?

5.1 5: Difficult to compare your ages with other work given that you did not measure ^{14}C on the entire soil volume.

Interactive comment on Biogeosciences Discuss., 12, 11279, 2015.