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Discussions

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

A. J. Dolman (Referee)
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Received and published: 22 July 2015

This is well written paper based on an enormous amount of work in harsh conditions. The paper presents much needed data on age and carbon accumulation rates in peatlands near the Yenisey region in Central, West Siberia. The authors find that the age is about 200 years older than previous reports and that this is due to DOC contamination in bulk samples. They find that when splitting C into DOC and POC, the DOC is generally much younger. In fact this is one of the main methodological advances of the paper, and this may merit a little more highlighting than currently done. The authors conclude that regional hydrology determines to large extent C uptake of peats.
The papers is experimentally and theoretically sound and the supplement presents relevant additional information.

I have a few minor comments.
It would be good to cite and refer to Yu, Biogeosciences 9, 4071-4085 to present the results in a wider context, The authors appear to have "missed" this paper.
Indeed, I missed the Yu paper. Thanks for pointing it out. The paper is valuable for me because it quantifies recent and older peat accumulation. I will cite it in this context.

11292 L26-P11203 L16. While the papers generally sticks close to facts the discussion on the landscape development in these lines is overall very speculative, and distracting from the key messages of the paper. My recommendation is to delete most of that part, particularly the line mentioned here.

On the one hand I agree, on the other, sorry to say, I disagree. I agree that the separation of DOC and POC is a major step forward in peat research, but it is mainly a technical advance. The ecological question, which factors formed this landscape, remain poorly understood.

The field observations led us to a landscape question: how can a fen accumulate several meters in depth at large scale in a landscape that is a freely draining surface water system. That landscape maintained shallow surface water over several thousands of years. The processes leading to thick layers of fen peat in river-systems remain under discussion. It is an important aspect of landscape ecology, hydrology, and geomorphology.

Thus, few lines of speculation may be appropriate. The hydrology of the Ob-Yenisey watersheds remains obscure. Even exact data on the subsidence of the Tulugan depression are not available.

One suspicion is that the migration of nomadic people into this region, who were living on fish, affected the beaver populations in the main drainage channels. We discussed this with zoologists, and got very positive responses, even though the archaeological evidence is weak. We did not search for artifacts in the field, but having in mind the collection of artifacts which I saw with Zimov at Cherskii, we should have searched along river banks. It is a pity that I will not be able to do this myself in the future.

I will shorten the text, but like to maintain the main ideas.

11293 L23. Figure 8 does not present a water balance but shows a landscape and landscape units.

Fig. 8 needs a better legend. The landscape indicates that the water flows from "upland" pine forests towards "lowland" Sphagnum peat as indicated by the green patches of sedge vegetation. The remaining nutrients are filtered at the forest edge, but the water volume contributes to the water balance of the Sphagnum peat land.

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

A. Saveljev
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Received and published: 28 July 2015
To me it is pleasant that in References there is our article (Ducroz et al 2005). Authors have very shortly mentioned about possible influence of the engineer of the wetlands - beaver - on formation of bogs in region of research. Also have bound it with the further expansion of ancient Ket-peoples on Yenisey River banks approx. 2000 years ago.
I believe that authors have the (full) right to develop this thought more thoroughly as historical publications about presence of the beaver at this region and archeological artefacts with motives of the beaver are known many. Especially it is many in collection of a National Khakassian museum in Abakan.
Thanks for the comment concerning the role of the beaver. I am pleased to read that there are arguments in support of our hypothesis. We still have no prove. Maybe a careful inspection of present river banks could uncover old beaver dams. I am sorry to say that I will not be in a position to carry out such work.

In the text some grammatical mistakes are noticed:
Page 11292 line14: Different writing Jennisey (comp. in title: Yenisey)
Thanks, we struggle with this word. There are about 3 ways of spelling

Page 11297. line 16. Institute for Biogeochamistry...
Page 11298.5. delete: and Sibir, Z.
Page 11298.6. 65 vln. Let: This should be "mln let"

Page 11298.18 Est Siberia
Thanks for pointing at these typos. They will be corrected

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

D. Mollicone
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Received and published: 21 August 2015
On the potential role of beaver
In the paper there is only an indication on a potential role of the beaver on the hydrology of this region. The role of the beaver in the creation and in the long term maintenance of wetlands has been very well documented in the scientific literature (Naiman et al. 1988; 1994; Butter Malanson, 2005). The indication in the paper is suggesting that the colonization of the region by the Ket people occurred around 2000 yr ago could have impacted the beaver population and consequently the hydrology in the region.
The Ket were the last group of hunter-gatherers to survive the spread of pastoral peoples

across landlocked northern Asia, only abandoning their mobile lifestyle during the forced Soviet collectivization campaign of the early 1930s. They subsisted entirely on hunting, fishing, and the gathering of wild plants (Vajda, 2011). As reported in the paper, the Ket people are not responsible for the nearly extinct beaver population in the region. However, a concrete hypothesis could be that the Ket people may have removed beaver dumps from the second order of the western tributaries of the Jennisey to use the river system as communication network. In such a flat landscape the removal of beaver dumps could result in an increased drainage capacity of the western tributaries, thus affecting the wetland species composition. A description of a similar landscape dynamic is provided in Naiman et al 1991 : “In North America, beaver (Castor canadensis) provide a good example for linking long-term population dynamics to ecosystem-level processes. At the time Europeans arrived in North America, the beaver population exceeded 60 million individuals (Jenkins Busher 1979). These beaver created extensive wetlands throughout their 15 x 106 km2 range. Yet, by 1900 AD the beaver was nearly extinct and much of their former habitat had reverted to dryland (Naiman et al. 1988). At the beginning of this century, with a relative absence of predators, laws regulating trapping, and an abundance of forage and habitat, beaver began a rapid population increase throughout most of their former range. Beaver alter the landscape by cutting forests within about 100m of water courses and by changing the hydrologic regime through dam building. These activities are readily quantified from aerial photographs taken since the mid-1920s (Johnston Naiman 1990a, b).”

References

Butter DR Malanson GP (2005) The geomorphic influences of beaver dams and failures of beaver dams. Geomorphology 71: 48-60

Jenkins SH Busher PE (1979) Castor canadensis. Mammalian Species 120: 1-8
Johnston CA Naiman RJ (1990a) The use of a geographic information system to analyze long-term landscape alteration by beaver. Landscape Ecol. 4: 5-19

Johnston CA Naiman RJ (1990b) Aquatic patch creation in relation to beaver population trends. Ecology 71: 1617-1621

Naiman RJ, Johnston CA Kelley JC (1988) Alteration of North American streams by beaver. BioScience 38: 753-762

Naiman RJ, Manning T, Johnston CA (1991) Beaver population fluctuations and tropospheric methane emissions in boreal wetlands. Biogeochemistry 12: 1-15, 1991

Naiman RJ, Pinay G, Johnston CA Pastor J (1994) Beaver Influences on the Long-Term Biogeochemical Characteristics of Boreal Forest Drainage Networks. Ecology 75: 905-921

Vajda EJ (2011) Siberian landscape in Ket traditional culture. Landscape Culture in Northern Eurasia Pp. 297-314

Dear Danilo, thanks for this additional information. Nevertheless, we have no proof (i.e. an ancient dam). Thus, I think we should follow the recommendation of Han Dolman, and keep this paragraph short.

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

B. Guenet (Referee)
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Received and published: 29 September 2015

The manuscript presented by Schulze et al., presents interesting data on carbon dynamics in boreal peatlands. Thanks to different measurements (14C, DOC, POC, vegetation surveys, plant macrofossil) they reconstruct the history of the peatland formation.
The paper is quite well written and the reading is pleasant. The authors have an important amount of data and they did a great job to organize them and find a story that makes sense. I believe that this would be a important paper in particular because it shows that thawing permafrost peat does not automatically induce carbon emissions into the atmosphere.

Nevertheless, I have one main concern. At page 11290, the authors mentioned the presence of ashes differently distributed in depths and between the profiles. I guess that those ashes came from vegetation burning and are not lignite (in this case the age

of C in ashes is close to infinity and the determination of the age is almost impossible). The 14C content of ashes coming from vegetation burning are close to the amount 14C in the vegetation (see Regev et al., 2011 for instance). Therefore the age of the C is biased to younger ages. Since the distribution of the ash is not uniform it might add noise to the data and it is not clear how the authors deal with this.
Thanks for this comment. We were not thinking about fires. We did not find charred coal, and it is unlikely that the water covered fen burns. The high ash contents stem from import of silt by flooding, and are most predominant in the gyttia and the transition towards fen.

The increase of ash content in the upper part of peat profiles is not connected with flooding (!), since studied fens and bog surface are not flooded at all. Flooding can determine the high ash content in the bottom profiles only. However, dust from the upland, which consists of alluvial sand deposits, would be possible, especially after fires in the forests.
Some increase of ash concentration in the upper layer of profiles may partly connect with the anthropogenic factors - increase of area with damaged vegetation cover, building of sand roads etc.

We will clarify this in the text

Minor comments:
p11289 l-5: This sentence suggests that the stocks are linearly related whereas Fig. 4 shows that this is the case but for the age. Please clarify.
Thanks, we will clarify this in the revision

p11291 l-22: Please clarify how the 2% value is calculated.
It was calculated on the basis of total number of 14C measurements.
If we base this fraction on the number of cores, the fraction would be 10%

p11297 l-9: Please correct "seperation"
Fig. 2 I guess the bold numbers are the age, please clarify the legend.
Thanks for pointing this out.

All minor comments will be considered in the revision

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

Interactive comment on "Carbon dynamics in boreal peat-lands of the Yenisey region, Western

Siberia" by E. D. Schulze et al.

Anonymous Referee #4

Received and published: 2 October 2015

This study uses 14C 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 14C analysis, as well as clarifying the terminology employed, which is potentially confusing with respect to the definition of DOC.

Following your suggestion (see below) we will clarify the definition of DOC (<0.45um, but, as far as we know, there is no general agreement).

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 interpretations 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.

The motivation of this study was not to solve the genesis of peat in Siberia, but to providing data for atmospheric measurements at the tall tower of Zotto. Only AFTER we completed this survey, we realized that the region has a specific history of peat development. I do not find it appropriate to give ad-posterior reasons for a study. This is why we put this into the discussion.

Due to the long fen stage, the average peat accumulation is lower than in other areas where bogs shifted into a Sphagnum stage in early Holocene. We think that this is a major point of 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 14C dates may underestimate the time period when peat began developing in the region.

I hesitate to add text, because I do not want to attack older literature on this topic. The reader must draw his own conclusions. The difference between DOC and POC-age has been described earlier, and this work has been cited.

A distinct aspect of the paper is the use of a base separation on a restricted particle size fraction to isolate samples for 14C 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.

Thanks for this comment. This is exactly the rational, which we stated. We will revisit the wording.

However, comparisons with other published papers at the regional scale are then complicated two-fold. First, only 14C ages for the >36 and <63 um fraction are presented, thus excluding larger and smaller particles.

We must apologize for an error, and we are grateful that the referee points at this.

Our DOC-fractionation was 2 steps, a centrifugation (2889 g, 30 Minutes) and a filtration (glass filter). Thus, the particle size is < 1,6 and NOT <36 um.

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?

We did not use any chemicals for extraction (bases), but only gravity and mesh size.

We measured the 14C-age in the >63um fraction. It was not significantly different from our POC. The main reason to discard the >63um fraction was that it contained inorganic sand (dust deposits?).

We measured the bulk samples in an initial trial. In most cases the bulk measurement is between DOC and POC, but there are also younger values. We have to few data-points and cannot provide a general relation between bulk and our POC.

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.

See above. I think that our DOC is very close to the conventional DOC. To our knowledge there is no agreed standard filtering for DOC and in most studies the particle size is not even mentioned. Here we present the maximum particle size for our DOC. We agree, that changes in wording may help, but conventional DOC is usually also not obtained by sedimentation (which would take very long, and the sedimentation time is not defined), but by centrifugation and filtration. The main difference is, that we used glass fiber filter, because we needed a C-free filter. This is why we end with 1.6um.

Finally, I do not agree with the statement made in the abstract: “This peat is older 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 14C is an informative ecological measurement, and it would have been helpful to present this data, especially for comparison with the other studies.

Thanks for this suggestion. We will change the wording in the text.

We made some initial bulk measurements, but these are not sufficient to provide a correction factor. Thus, we agree that such a correction factor would be helpful, but it is probably dependent on the water content. This would be a separate study which we cannot do with the material we have.

Specific comments

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

We know this, but we do not agree. Depth is not a dependent variable

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

We thought that this way of presentation helps the reader.

Section 3.3: I am concerned about the bulk density measurements; a 3.5 cm diametercore is quite narrow and would presumably compress the sample. How was this accounted for?
For the top layers of Sphagnum we used an area based subsampling, as we describe in methods. For deeper layers that are water saturated, there is no compression.

Section 3.4: The methods for separating DOC are unclear to me.
We will add text to methods to clarify the DOC extraction (centrifuge+filter)

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
We think that minor clarification can be made in the results. It helps the reader.

Page 11291, line 20: This paragraph belongs in the Results.
We think it helps the reader at this point

Page 11293: "Following Darcy's law (Nobel, 1991) the flow of water through a saturatedsubstrate 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.
Thanks for this comment. Under stationary conditions, which we may assume after thousands of years, the Darcy law simplifies to the Laplace equation where the hydraulic conductivity cancels. The flow through the system is then only dependent on height and the boundary conditions. We change the text

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.
This was also criticized by another referee, and we will change the caption

11293, line 17 "The present hydrological balance of the growing season is close to nil" I am unclear what you mean that the Pine forest is never flooded during the growing season?
No, the pine forest is uphill and never flooded.

11295, line 7: Also presumably by diffusion and advection, given your subsequent statements.
Thanks, this will be changed

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 _ ^{14}C of the samples for calibration with Oxcal?
Thanks, yes, this is what I meant to say

4.1 20: Do you mean the ages of DOC and POC were related?
Yes, indeed I think that this is the case, but there is a time-offset

5.1 5: Difficult to compare your ages with other work given that you did not measure ^{14}C on the entire soil volume.
Sorry, but 14C analyses are not sufficient to make this comparison

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Discussions

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

Anonymous Referee #5

Received and published: 20 October 2015

General comments: This paper applies and straightforward approach to reconstruct the peatland carbon dynamics of the Yenisey region. I like this approach to separate the TOC in POC and DOC for dating; this seems to be a crucial step for evaluation a potential error/bias in the basal ages of peatlands.

Thanks for your comment.

The main issue I have with this paper is that the methods section is not informative enough. The applied methods are not explained in detail (except the dating),

Our intention was, to be as short as possible to restrict the overall length of the paper. There is no problem to add information.

there is no explanation for the used linear regressions, no uncertainty definition etc.

We will add details on the statistical analysis

Please add your reasons, why you choose the proxies, parameters, what the parameters are used for and which assumptions are connected with the parameters an proxies. E.g. you measured ash content, but you do not refer to this parameter in the discussion.

It is true, that we do not discuss the ash content. We thought, that a peat paper should not be published without the ash information, even though nothing exciting happened in that region over all these years. We will add discussion.

Language: Peatland vs peat-land vs peat land. Please be consistent. Sometimes very long sentences, intricate sentence structures and word order.

Thanks. We will try to improve this. It truly reflects the situation, that 3-non-english mother tongs contributed to the text.

Figures: 1: The legends and numbers of Fig 1 are too small. The colors are hard to differentiate. Please be consistent with the Russian transcriptions like Enisey vs Yenisey

Thanks for this comment. We try to improve the design of Fig 1.

3 and 4: Pleas add n=XX to the regression descriptions. Pleas define abbreviations in the caption to make them readable without needing the text.

Thanks, this will be revised.

8: please add a scale to this image. Like the diameter of the big lake

Thanks. This is indeed a very good suggestion. We had thought that the trees give a scale.

Comments on the sections: Abstract: The abstract should consist all the sections of the paper, please add an introductory sentence

Thanks for this comment. We will revise the abstract.

Introduction: To state the hypothesis or research questions at the end of the introduction is excellent. Please discuss it in the discussion section and finally answer it in the conclusion section explicitly.

Thanks, we had thought that we had done so, but we will follow your suggestion more explicitly

Study area: Very detailed, much longer than the methods section

I think that this detail is needed, because most readers will not be familiar with the region.

Methods section: This section needs major revision. Please add details on the applied methods. Some unanswered questions are: How did you measure TOC. Did you measure POC and DOC amounts separately?

We will add information on TOC, DOC and POC methodology

Why did you do not choose standard cylinders for bulk density sampling.

This is indeed not possible in a 6 m deep peat profile. We will comment on this

Add details on the used devices, e.g. what kind of microscopes, lenses.

This information will be added

What kind of statistics did you use statistical programs/software (R, Matlab:::). What means the in the manuscript, standard deviation (data normally distributed?), interquartile range, confidence intervals?

Thanks for the comment. This information will be added

Results section: No comments

Discussion section No comments

Conclusion: Please repeat/relate to your research hypotheses (introduction) an answer

this scientific problem

Thanks, As stated above, we will revise the conclusion

We are specifically grateful for the detailed comments. I am sorry to admit that I did oversee some mistakes.

Detailed comments: Page 11280, line 18: Please change Schurr to Schuur here and

the following pages including the reference section

will be corrected

Page 11281, line 1: peat land, peatland or peat-land? Please check here and the entire

manuscript.

Indeed, this needs to be changed.

Page 11281, line 2: un-frozen or unfrozen? Please check here and the entire

manuscript.

This will be changed

Page 11281, line 7: 40 to 50% compared to 40% on page 11282, line 20. Did I misun-

derstand the percentages or why these numbers are different? Please add references

here.

Thanks, this will be edited

Page 11283, line 10: Please be consistent for 20 thousand or 20 000

Thanks, this comment and the following comments will be taken care of

Page 11283, line 26: main or mean?

Page 11285, line 22: Please define releves shortly

Page 11286, line 1: Please add details on the used corer.

Page 11287, line 21: please change the webpage address with details on the version you used

I used the web page as indicated, which was available at that time. I will try to find a new source.

Page 11288, line 9: Please delete "and not from gyttia"

Page 11288, line 13: Please change to ": : : River. The oldest: : :" or add a comma here

Page 11290, line 21: Please cite Schuur with the number you are using for your comparison. Moreover, please refer to the NCSCD, published by Hugelius et al 2014 (e.g. Figure 3, Biogeosciences, doi:10.5194/bg-11-6573-2014) and Hugelius et al 2013 (ESSD, doi:10.5194/essd-5-393-2013), who calculated the 0-3m kg/m$_2$for this region already. Moreover, the Schuur et al. 2015 synthesis paper is based on this numbers of the updated NCSCD papers

Thanks. I was not aware of this paper.

Page 11293, line 9: Please change "Figure 7 suggests" to e.g. "In figure 7 we suggest : : :". Same for line 22

Page 11293, line 17: please change nil to zero

Page 11294, line 23: Please change aapa to Aapa

Page 11296, line 14: "as long as rainfall exceeds evaporation". It is easy to say everything will stay the same if the conditions stay the same. Please discuss the predictions (e.g. models, trend is the measurements) for your study region here.

This is indeed a challenging comment. I am not sure about the certainty of the predictions. We had long discussions about the present hydrological balance of the region, because of the uncertainty of snow melt-flooding. I will contact Martin Heimann, who runs the Zotto tower, about his predictions.

Page 11296, line 16: Please add Hugelius et al. 2014 here as well (as stated above) and cite the number you refer to with “3 to 5 times as much”

Thanks. This and the following comments will be changed

Page 11296, line 21: pleas change line to zone

Page 11296, line 23: please state what you mean by “extremely long”. What does it mean in years?

Page 11297, line 1: “could potentially” is very vague. Please describe a likelihood or estimation or describe why it is not possible to be more concrete.

Again, I will try to give a more precise likelihood. I am aware that such concrete estimates are badly needed.

Thank you for this paper and best regards!

Please also note the supplement to this comment:

http://www.biogeosciences-discuss.net/12/C6776/2015/bgd-12-C6776-2015-supplement.pdf