

Interactive  
Comment

## ***Interactive comment on “Properties of dissolved and total organic matter in throughfall, stemflow and forest floor leachate of Central European forests” by S. Bischoff et al.***

**J. Van Stan (Referee)**

jvanstan@georgiasouthern.edu

Received and published: 6 December 2014

This manuscript’s topic is of interest to BG readers, especially considering the scope of chemical analysis (dissolved + particulate organic matter, where manuscripts typically examine only one of these components), variety of landscapes represented for *Fagus sylvatica*, and the interspecific comparison of *F. sylvatica* with *Picea abies* plantations. The water collection/filtration/storage and <sup>13</sup>C NMR spec methods are carefully performed, leaving some minor details to be clarified (see below). Still, there are some concerns regarding the incomplete conceptualization of canopy-based organic matter biogeochemical processes in the introduction/discussion - only discussing phyllo-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

sphere processes with little mention of cortisphere (branch and trunk bark) processes (only a few lines on P15096L9-11). There is also a concern regarding lack of site details that would support the merit of researching the overarching hypothesis at the selected sites. A final concern is regarding the length of time between storm events themselves and sample collection (two weeks per P15092L12) – since portions of terrestrially-derived OM are highly labile (being processed in hours-to-days). As such, I believe this manuscript is appropriate for publication should the authors revise to be inclusive of both leaf and bark canopy-based OM-transformation processes, provide some necessary site characteristics, and provide greater detail regarding how long samples were in the collectors prior to sampling/discuss potential impacts of the sampling procedure on their OM character results.

Specific comments: 0) The abstract is not detailed enough and most of the writing past line 7 is unclear. For example, is the TOM versus DOM comparison in lines 9-12 for all samples regardless of species? What is meant by “fresh” POM in line 8? What is meant by a “tree species-related effect on the origin of OM composition [and] properties” – tree structural effect, tree-specific leaching characteristics, tree phyllosphere and cortisphere microbial community differences? Also, more information is needed in the abstract, like: (a) what is the list of OM characteristics and metrics derived from the <sup>13</sup>C NMR spec (see Table 1 and Table 2)? (b) How many total samples were analyzed? (c) Which results indicated species-specific throughfall, stemflow, or litter leachates may differ in allelopathic potential (see discussion P15096L27-&)? Etc. . .

1) An overarching hypothesis formed regarding differences in TOM and TOM structural transformations between the two species (P15090L8-11) has two issues in its current form: (a) it focuses solely on how leaf structural differences for *F. sylvatica* versus *P. abies* can alter OM vis-à-vis microbial communities and substrate interaction in the phyllosphere. . . this neglects the fact that these species also have quite different bark structures and branching architectures hosting microbial communities and substrates capable of interacting during the throughfall or stemflow process to alter OM. (b) it ne-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

glects to mention that interspecific differences in phyllosphere and cortisphere microbial community structure/function may also play a critical role in altering TOM travelling through the canopy along hydrologic flow paths.

2) A key introductory component supporting the need for this research is that POM might be particularly relevant to other nutrient cycles (specifically through microbial decomposition processes [P15089L11]). But, the authors don't cite any studies in support of this statement, nor do they provide explicit explanation of how POM's C:N ratios (or myriad other microbially-attractive qualities) support this statement. Please make this connection more explicit and substantiated by citations as it is a piece of the foundation supporting "why" your work is necessary.

3) Citation issues in introduction: P15088L22: the statement "only a few have investigated the dynamics" is lacking necessary citations – please cite which few studies have done so. P15089L16-18: the statement "The chemical nature of mobile OM is. . ." currently lacks citation. Please provide. P15089L28: The citation Levia et al., 2012 is incorrectly cited, as the authors' statement discusses cation cycling and the citation does not investigate cation cycling – only DOM. If the authors wish to stick to that research group's work for a citation, a more appropriate reference would be Van Stan et al. (2012, The effects of phenoseason and storm characteristics on throughfall solute washoff. . ., Sci. Tot. Environ., 430: 48-58) for throughfall ion work, or Levia et al. (2011, Atmospheric deposition and corresponding variability of stemflow chemistry. . ., Atmos. Environ., 45: 3046-3054) for stemflow ion work.

4) Study site description is lacking some details that are necessary, in my opinion, for contextualizing/interpreting results (even though a previous site description exists in Fischer et al. [2010]). Specifically, the authors lack a comparison of stand structure for the plots to substantiate that the interspecific structural differences described in P15090L8-11 are present. Although a basic description of the *F. sylvatica* plots is supplied (P15091L13-17), the *P. abies* plantations are not described. . . are they also similarly-aged or -sized compared to the *F. sylvatica* plots? How do stand characteris-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



tics (density, basal area, height, dbh, etc) between *P. abies* and *F. sylvatica* compare? Why were *P. abies* plots only chosen at one observatory when, according to Table 1, there were plots available in the Schwaebische Alb site? Doesn't the sole selection of *P. abies* sites from Hainich-Duen diminish the authors' aim for comparison of these species across different environmental conditions? I believe these questions could be answered (briefly) to the benefit of the manuscript while still leaving some details to be found in Fischer et al. (2010).

5) Can the authors please provide some further details regarding their methods: P15092L10-12: What were the sample collection procedures, more specifically? E.g., what type of bottles were used to collect/store the water? How long were samples permitted to remain in the field after a storm (this has a big impact on what DOM character will be observed)? Were bottles acid washed and triple-rinsed with sample prior to collection? P15092L19: Are the "cooling boxes" simply refrigerators? Regardless, at what temperature were the samples stored?

6) Results: Figure 1 was absent. . . Not sure if it is something I did during the download or if Figure 1 is really just not there. . . So, I took the authors at their word during descriptions of the  $^{13}\text{C}$  NMR spectra.

7) Discussion: (a) The introduction links TOM structure in canopy-derived hydrologic fluxes to canopy-based microbial communities, yet the discussion generally avoids this topic. Is this because no bulk precipitation samples could be analyzed via  $^{13}\text{C}$  NMR spec? (b) Depending on how long samples were allowed to sit in the field, could the authors please provide a discussion of how this would affect the character of their DOM and POM measurements? (c) P15096L3-6: Please discuss why the aromatic C-region intensities for FF leachates of Sanderman et al. (2008)'s mixed redwood stand were so much higher than observed for your Spruce sites.

Editorial comments: Abstract, P15088L7-8: The phrase "echoed in structural differences" is unclear, please revise. The verb "echo" literally means "repeated" or "rever-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

berated”, but can more symbolically mean “shared characteristics with”. How does *F. sylvatica* leaf surface-derived POM “repeat” or “have shared characteristics with” total organic matter (TOM) structure differences? Moreover, TOM differences in what (species? landscape? source?)?

Introduction, P15088L22: Please replace “but” in the statement “but none the character of water-bound TOM...” with “and”. Also, please insert a verb into the statement between “none” and “the”. Perhaps the statement could read “and none investigated the character of water-bound TOM...”?

Introduction, P15090L12: In the statement “fresh beech leaves exhibit” – do you mean to say “fresh beech leaves leach”?

Methods, P15092L134: Two issues: The verb tense “were” should be “was”. And, the terms “count” and “terminated” could be replaced with clearer terms, like “release” and “over” respectively.

Results, P15095L5-6: The statement “as of SF samples in general” is unclear. Do you mean that there are no studies reporting on stemflow <sup>13</sup>C NMR spec-derived TOM characteristics and metrics? Please clarify.

---

Interactive comment on Biogeosciences Discuss., 11, 15087, 2014.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)