

Interactive comment on “Dissolved organic matter characteristics of deciduous and coniferous forests with variable management: different at the source, aligned in the soil” by Lisa Thieme et al.

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This manuscript presents a geographically and analytically broad study of dissolved organic matter (DOM) in forest types common throughout Europe. Little work has looked beyond DOC concentration and UV-vis absorption for all DOM fluxes along the rainfall-to-soil flow pathway in forests. In fact, this is one of only a handful of studies to include both throughfall and stemflow fluorescence, even fewer studies report biodegradable DOC proportions for these fluxes, and I'm aware of only 1 other study that has examined these fluxes using FTICR-MS. The large dataset appears to have been carefully examined and the methods are mostly well-described and appear well-done. Results

C1

shed new insights on DOM processes along the canopy-to-soil flow pathway and appear to support the theory put forth in the title (different at the source, aligned in the soil). However, there are a few weaknesses that I believe should be addressed before publication:

Biodegradability measurement – were the samples spiked with nutrients to achieve N, P > Redfield limitations? All other details of the bioincubation tests look good. But, if we want to test the biolability of the DOC, then it is important to release the microbes from as many common limitations as possible (e.g. the authors set an optimal, controlled temperature: page 6, line 2). As a test of how much DOC is utilizable by microbes, this is a test of DOC quality, not an environmental rate at which one would expect the DOC to be utilized. Thus, ensuring the microbes are released from nutrient limitations, arguably, should be standard to allow comparison of DOC quality across studies, sites, between research groups, and independent of differences in C:N:P across environments. If this was not done, I recommend the authors briefly discuss the implications (biodegradation of DOC could have been constrained).

There are no measurements/estimates/tests of soil geochemical interactions with infiltrating DOM. As indicated above, biodegradation will likely be limited in natural settings (compared to the bioincubation tests – even for bioincubation tests without the nutrient spiking). I noticed that the other reviewer also believed this to be a shortcoming of the manuscript. As gathering more data along this vein would be difficult (and is, of course, not possible for storms already past unless it was collected at the time), I recommend the other reviewers' solution: provide more discussion of geochemical controls over DOM processes within soils. Perhaps the discussion could have subsections dedicated to biological factors and geochemical factors?

The meaning of DOM “origin” is unclear. For example, in the abstract: “strong significant effects of origin of ecosystem fluxes” – what is the “origin”? (A) Is it the first contact between precipitation and terrestrial surfaces (in the tree canopy), thus species-specific throughfall v. stemflow v. litter leachate? Or, (B) Is it the origin of specific DOM

C2

fluorophores/molecular formulas? If (per A) the “origin” variable is used to indicate the initial DOM-enrichment process - throughfall or stemflow or litter leachate (for gap throughfall) - how is this different from the “species” variable? If the “origin” variable is used to indicate the origination of specific indicator fluorophores (like the component C1, “humic-like with terrestrial origin”) or FTICR-MS formulas (like the N-rich organic compounds assumed to have atmospheric origins [p. 15, lines 10-15]), then this should be explicitly defined.

Although there is little literature covering throughfall and stemflow DOM quality, the authors missed some studies. Normally, one cannot cite all the studies on a particular topic; however, in this case, since so few studies exist, I recommend their inclusion. Please note that, for one of these papers, I am the lead author and it is not my intention to push my own work, only to account for the few studies on the topic. Introduction and discussion: Throughfall and stemflow DOM concentration, flux and quality (including potential sources and fates) have been reviewed and evaluated by Van Stan & Stubbins, 2018, <https://doi.org/10.1002/lol2.10059>. Page 16, lines 26-30: The authors only reference Qualls & Haines (1992) biodegradation estimates for throughfall. But, they do not discuss the only study reported stemflow BDOC in Qualls’ recent Special Issue (Howard et al., 2018, <https://doi.org/10.3390/f9050236>).

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-478>, 2018.