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Biogeosciences
Discussions

# *Interactive comment on* "Tracing inputs of terrestrial high molecular weight dissolved organic matter within the Baltic Sea Ecosystem" *by* B. Deutsch et al.

**Anonymous Referee #3**

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This manuscript focuses on using ïĄď13C-HMW DOC to trace the fate of terrestrial organic matter in the Baltic Sea. The authors concluded that terrestrial DOM contributes 43-83% DOM in the Baltic Sea, and that the terrestrial DOM is not subject to substantial removal once in the open Baltic Sea. The manuscript is well written and presented, and the data overall are solid. However, I feel that they over-interpreted the data a little bit, and this paper did not add much on what Alling et al. have previously presented.

The authors used the HMW DOM to calculation the contribution of the total DOM pool, yet the recovery rates of the ultrafiltration were only in the range of 13.1-27.3%. It is known that HMW DOM is generally more labile than the LMW one (work from Benner's



group), so their conclusions about the whole DOM may be flawed. This important fact was missed throughout the text.

The authors specifically chose the stations with salinities less than 7.5. I am wondering why they did not do the same on those stations with higher salinities. Those "real" coastal waters are more meaningful to the question about the fate of terrestrial DOM in marine environments. I do not feel very comfortable of making such a big deal out a dataset within salinity of 2-7.5, and del13C values of -27.25 to -25.25 ‰ (Fig. 3b), unless you have very restricted end members.

In the introduction, the Carlson book chapter was cited many times. To me, it's more appropriate to credit the original research papers.

P4487, line 3: The temperature is also important factor leading to isotopic fractionation.

P4489: Sampling depth should be reported. I am curious how the depth profiles would change, and how their conclusions would be affected if the system is not homogenous in terms of depth.

P4489, line 13: Do not start a sentence with a number; line 19: filtration rate well above 15? This is confusing.

P4490, line 23: Should report the 4 values to show the variability. Also, how come in their opinion that this averaged end member can reflect the whole Baltic Sea? Evidences are needed to back this up.

P4492, line 6: The 3 stations in the Oder Bight were off (Fig. 2), and they interpreted this as melting water of the ice. From Fig. 2, these 3 points have about the same salinity, but drastically different del18O values. How exactly can this pattern be explained by melting water?

P4493: They argued that DOC and salinity have a weak correlation (Fig. 3a), but a very good one between del18C-HWM-DOM and salinity (Fig. 3b). But to me, there is not much difference between Fig. 3a and 3b. They need to show the regression

equation of Fig. 3a to support their argument.

P4494, line 6: They argued that a “slight” deviation from the mixing curve (Fig. 3b). They further interpreted this as addition of DOC from marine sources. I do not think that the deviation they observed are statistically significant, considering the analytical errors involved and the assumption they made (end members). By the way, the line they drew on Fig. 3b does not seem to be a straight line, but a curve?

P4494, line 21: Delete the comma after the fact.

Fig. 2: The locations of SK, KT, BeS are not marked in the map (Fig. 1).

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Interactive comment on Biogeosciences Discuss., 9, 4483, 2012.