Biogeosciences Discuss., 9, C2591–C2592, 2012
www.biogeosciences-discuss.net/9/C2591/2012/

Biogeosciences Discussions

Interactive comment on "Partitioning of soil water among canopy trees during a soil desiccation period in a temperate mixed forest" *by* M. Meißner et al.

Anonymous Referee #1

Received and published: 20 July 2012

The following comments were already mentioned during the access review:

1) The deltaD profile in soil suggests that there is very little isotopic discrimination below 0.3 m in depth, yet the strongest results from the mixing models imply large differences in uptake between the 0.3-0.5 m layer and the 0.5-0.7 m layer. How is this possible?

2) What is the significance of the soil water potential measurements? They could be useful but are not applied in the paper beyond description. Soil water potential increases strongly with depth, suggesting that it becomes increasingly easier for plants to extract water as rooting depth increases. Now, if two soil layers (shallow and deep)

with contrasting deltaD values were used as potential water sources, and plant deltaD were equal to the mean of these two soil deltaD values, we conclude that the plant extracts equal amounts of water from the two layers, but the fact that the deeper layer has a higher potential must mean that the tree actually must allocate greater root length to the shallow layer to extract as much water as it gets from the deep layer. None of this is discussed in the paper. This also means that volumetric water content as a function of depth does not measure water availability for the tree.

3) Why is only deltaD used and not delta18O, a more common water isotope for these studies? I believe deltaD values are more variable than delta18O values.

4) I am not sure that comparing the fractional water uptake among species layer by layer is the strongest statistical test. There are too many tests with weak statistical power. A joint analysis of the entire uptake profile would be more desirable. For example, a multinomial model could be fit to each species, and an analysis conducted to test whether specifying separate distributions for each species provides a better fit to the data than using a single distribution. Also, figures 5 and 6 should be combined, because the most interesting comparisons are not among species, but between the single and mixed species clusters.

5) The Phillips and Gregg method for constraining the mixing model for cases where there are more sources than variables is only an approximation, and the results depend on the assumptions of the analysis. A sensitivity analysis that examines how the results might respond to the assumptions made is warranted. In general, the authors might explain this technique further. The Phillips and Gregg (2003) paper cited (which appeared in Oecologia, not "Ecosystem Ecology") gives as examples systems with one isotope and three sources or two isotopes and five sources. Here we have one isotope and five sources. Isn't this pushing the bounds of this technique and if not, why not?

Interactive comment on Biogeosciences Discuss., 9, 5415, 2012.

BGD

9, C2591–C2592, 2012