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Biogeosciences Discussions

Interactive comment on "Comprehensive description of the carbon cycle of an ancient temperate broadleaved woodland" *by* K. Fenn et al.

Anonymous Referee #2

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General comments: This is an occasionally useful addition to the literature on forest carbon cycling that uses standard measurement methods and many modeled inputs to construct a short-term carbon budget for an English deciduous woodland. Its pairing with eddy covariance results provides a potentially useful constraint on the annual flux estimates, although the lack of plot location information relative to the flux footprint compromises this comparison. The seasonal dynamics and annual NPP, Reco, and GPP estimates are unsurprising, but the relative contribution of ecosystem components to these estimates is. Particularly, the low relative contribution of soils and the high relative contribution of stems to Reco and the low ecosystem carbon use efficiency. Given the scarcity of belowground measurements in this study and the many

assumptions involved with scaling point measurements of stem 'respiration' to entire trees and stands, the component data presented in Table 2 must be viewed skeptically. The reported error estimates to these numbers unfortunately have little bearing on their accuracy.

Specific comments: The authors' statement in the abstract that there are 'very few comprehensive descriptions of the productivity and carbon cycling of forest ecosystems' simply is untrue. A little time spent in the publications section of the Fluxnet website (which is itself incomplete) will reveal a wealth of forest carbon cycling information, comprehensive and otherwise. It is interesting that nothing from the many studies conducted at Harvard Forest are referenced in this paper.

In the Methods section, 'companion papers' are mentioned, but never listed. We are given one dissertation and a submitted manuscript. This is not helpful. The Morecroft et al. 2008 paper cited as providing background information on the site is missing from the cited literature list.

A general problem is that we are given virtually no information on stand characteristics in this paper: stand area, tree age distribution, total basal area, maximum LAI, position relative to the flux tower, etc. For many readers, 'ancient woodland' suggests something like 'old growth forest', but that evidently is not the case here. Indeed, the trees in this stand may be relatively young. How representative is the 1 ha plot studied here to the rest of this forest?

For the allometric equations used to estimate tree biomass, were the size of harvested trees comparable to those in the study plot? These equations may be very problematic if larger trees are being measured for DBH than were harvested and weighed (a common occurrence).

How were coarse woody debris and reproductive inputs calculated? Were any attempts made to measure herbivore losses? They are assumed to balance new leaf production post canopy closure, but is the LAI 2000 sensitive enough to base this assumption on?

How sensitive are the daily and seasonal estimates of leaf respiration to the assumption of Q10 = 2? There are many reports in the literature of significant deviations from that value, with temperature acclimated plants typically having lower Q10 and unacclimated plants have a higher Q10.

The description of methods for measuring stem respiration leave many important questions unanswered. For example, were temperature relationships established or assumed so as to adjust spot measurements for variation in ambient temperatures? How was bole temperature measured (if at all)? Were the south facing measurements (presumably warmer) representative of other sides of the tree? Did bole surface CO2 flux show any relationship to sap flow? If so, it is likely that other sources of CO2 were contributing to the flux in addition to the underlying sapwood. Do we have any idea how accurate Equation 8 is in predicting total woody surface area of trees in an ancient woodland? How do their absolute values compare with other reports from deciduous forests of similar basal area? Because of these issues, and others, I have little confidence in the scaled measurements of stem respiration reported here.

In the Discussion the authors point to their belowground NPP data and remark that traditional studies focus on more easily measured aboveground components. I find this a bit remarkable given that with the exception of soil respiration, virtually all their belowground results are built upon untested model assumptions, rather than direct measurements. We really have no idea how accurate they are.

Interactive comment on Biogeosciences Discuss., 7, 3735, 2010.