Biogeosciences Discuss.,
doi:10.5194/bg-2016-419-AC5, 2016

Biogeosciences Discussions

Interactive comment on "Water availability limits tree productivity, carbon stocks, and carbon residence time in mature forests across the western United States" *by* Logan T. Berner et al.

Logan T. Berner et al.

logan.berner@oregonstate.edu

Received and published: 10 December 2016

Anonymous Referee #4

General comments:

This is a thorough, straightforward study using both field and satellite measurements to estimate forest productivity and carbon cycling along a spatial moisture index across the western US. The goals of the study were outlined well, and made use of two datasets that if assimilated properly, can reveal ecological trends and relationships that cross spatial scales. The results revealed, unsurprisingly, that as moisture index increased, so did both productivity and biomass; however this study is one of the more

thorough I have seen in both its spatial and methodological scale. The results suggest that climatic moisture availability is perhaps the most fundamental environmental control of forests in the Western US, and that the forest communities are extremely sensitive to this across large spatial scales. I feel this study is well conceived and publishable, but needs more explanation of methods, particularly with regards to data assimilation and validation. You mention in section 2.3 that you 'minimize[d] uncertainty' by using two different data types (field and remotely sensed), but you present no evidence of this. Also, though you present the Spearman coefficient in Table 2, I would have liked to see some cross-domain validation between data types; that is, a simple statistical comparison of how each median variable (NPP, BIO, CRT) value compares between field and satellite data.

RESPONSE: We appreciate the reviewer taking the time to comment on our manuscript. In the revised manuscript we removed the comment about 'minimizing uncertainty by incorporating both field and remote sensing data sets,' which was not phrased appropriately. In fact, we removed that section (2.3) entirely and incorporated select element into other parts of the manuscript. Following the reviewer's second comment, we compared field- and satellite-derived estimates of median NPP, BIO, and CRT, which showed that they were strongly correlated. We then added a sentence towards the end of the results section stating that, "Field- and satellite-derived estimates of median NPP, BIO, and CRT were strongly correlated (r_s=0.90-0.95; $p<0.001$)." We address the reviewer's remaining comments below.

Specific comments:

L52: Mention of ecosystem services seems unnecessary

RESPONSE: We removed the reference to ecosystem services.

L69: Suggest substituting 'risk' with 'frequency' or 'occurrence'

RESPONSE: We changes 'risk' to 'occurrence.'

L101: CRT should be defined before acronym is introduced.

RESPONSE: We edited these sentences to read (starting on line 91):

"Several of these earlier field studies also indicated that plant communities accumulated more BIO per unit of NPP in progressively wetter areas, suggesting slower turnover of plant BIO as climate became wetter (Whittaker and Niering, 1975; Webb et al., 1983). Mean carbon residence time (CRT) describes the average duration that a carbon molecule will remain in a specific pool (Waring and Running, 2007) and for CRT in live biomass can be computed as BIO/NPP assuming that BIO remains constant over time (Whittaker, 1961; Friend et al., 2014)."

L154: This sentence is very unclear. I don't understand what 'ensemble average' is referring to, nor what the 'previous work' revealed.

RESPONSE: We changed the two sentences to read (starting on line 161):

"We then reprojected these maps onto a uniform grid in an equal area projection, masked them to the common forest extent, and then averaged the AGB for each pixel across the three biomass maps. We used the biomass map ensemble average in the subsequent analysis, recognizing that pixel-wise estimates of AGC can vary notably among individual maps (Neeti and Kennedy, 2016)."

L196: Should it be climate 'data' sets? RESPONSE: Yes, thank you.

L196: Some context should be given for CMI values. What is the typical range? What constitutes extreme values on either end?

RESPONSE: We provide a summary of minimum and maximum CMIwy across the western US, as well as the average CMIwy in forested areas in the results section.

L229: Make sure use of 'Spearman' or 'Spearman's' is consistent

RESPONSE: We edited the manuscript to consistently use Spearman's .

L447: Changing natural disturbance regimes should be mentioned in the climate change implications section, given that you discuss it earlier in the context of carbon residence time.

RESPONSE: We ended up replacing the 'Climate change implication' section with a section called 'Predicting ecosystems response to environmental change,' which mentions the importance of changes in disturbance regimes. Part of this section reads (starting on line 485):

"Near-term effects of climate variability depend on the physiological characteristics of species in the extant plant community, yet ecoclimatic relationships derived from spatial data reflect gradual adjustment of community composition and population size to climate over long periods of time (Jin and Goulden, 2014; Wilcox et al., 2016). Furthermore, ecoclimatic models derived from spatial data cannot account for other ecophysiological impacts of environmental change, such as (1) enhanced plant water use efficiency from CO2 fertilization (Soulé and Knapp, 2015); (2) increased likelihood of tree mortality due to hotter drought (Adams et al., 2009); or (3) novel changes in disturbance regimes (Dale et al., 2001; Hicke et al., 2006). Consequently, predicting ecological response to environmental change over the coming century will require the use of mechanistic ecosystem models that account for physiologic, demographic, and disturbance processes at fine taxonomic and spatial scales (Hudiburg et al., 2013; Law, 2014)."

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-419, 2016.