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Discussions

Interactive comment on "Impact of nitrogen fertilization on carbon and water fluxes in a chronosequence of three Douglas-fir stands in the Pacific Northwest" *by* X. Dou et al.

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Note: I assigned my graduate Biogeochemistry class this paper to review, and this comment represents a composite of student comments.

The primary objective of Dou et al. is to resolve discrepancies between two previously published papers that used the same dataset. They attempt to resolve their slightly varying conclusions using a different model: an artificial neural network (ANN). The paper does not properly justify why an ANN is the best approach for simulating carbon fluxes. It also does not quantify the uncertainty in the three models and uncertainty when comparing them. Means are generally reported without confidence intervals,

which is insufficient when comparing results between models. There is no true statistical analysis conducted on the variances of the three models.

The introduction gives valuable insight into the current state of knowledge of regarding the effects of N on NPP in different ecosystems. The author identifies current gaps in knowledge and provides the set up for the study; i.e.-why certain questions were addressed. The author doesn't define some pertinent acronyms, such as EC. EC can stand for a few different methods and throughout the paper I thought they were talking about electric conductivity. The labels for the different stands, distinguished by age, was very confusion. The authors switched between different naming conventions throughout the paper for the different stands. This aspect has to be fixed in order to provide clarity to the study. There was no clear hypothesis or objectives. The hypothesis is somewhere contained within the authors' quest to distinguish between two previously published papers at the same study sight. Due to this setup of the hypothesis, the study doesn't sound very important. The authors do not present a strong case for the importance of understanding the effect of nitrogen fertilization on carbon and water cycles in forest stands of different ages. As a reader, I am left with the following questions: 1) Why is it important to know how nitrogen fertilization affects carbon sequestration and water use efficiency? 2) What is the real-world analogy to the nitrogen addition treatment? 3) Why is it important to know how N addition affects the C and water cycles through the development of a forest stand? (i.e. what is the significance of using a chronosequence?)

The methods were fairly clear in regards to being able to repeat the procedure. I question the use of high concentration of N. N can kill trees at such high concentrations. There was no justification as to why that high level of N concentration was used. Overall, the authors did a good job of trying to limit the variability through the multiple sites. I am concerned that there were controls set up in this study. Without a negative control treatment of no N addition, it is impossible to attribute the measured differences in forest responses to the N inputs. Any number of factors independent of N addition (including purely random chance) could have led to observed changes in GPP, LUE, C exchange fluxes, and WUE. I am aware that two previous papers using this data set and this methodology have been published, but this precedence shouldn't be justification for further publication. At the very least, the reason for the lack of this feature needs to be addressed. The different application method for N in the older forest stands versus the youngest forest stand defeats the purpose of a chronosequence. The older stands received an aerial spread of urea, while the youngest stand received a drip treatment directly to the bases of the trees as a consideration for the "young age of the planted trees and the competing understory." As the purpose of this chronosequence was to infer a timeline for the response to a N addition treatment, using the age of one of the sites as a justification for changing the treatment is wholly inappropriate. Furthermore, the "competing understory" is likely to be an important factor that contributes to how a forest responds to a N addition, and altering experimental protocol to mitigate this effect fails to incorporate its potential impact. Treatments should be applied as evenly as possible across experimental units, but in a chronosequence design with no replication it is essential.

Once again, this section has acronyms that aren't clearly described and this takes away from the clarity of the paper. For example, what is PAR? For a reader who isn't very familiar with this subject area, such as myself, this would add confusion. Two were two main problems I found in this section. The separation of GPP and R was a main objective of the paper. The author's don't describe how this separation was achieved though. I also believe the way the weights given to each variable in the model has some flaws. Characterizing the weights of each variable in a N-limited system, then using this model to character change when there is overabundance of N, I believe, would create problems. What if N-limited system employs different pathways than a non-N-limited system? This would change the weights of the variables within the model. I believe the weights of the variables should have be conducted using the non-N-limited system. The results were very interesting, although because of the previously stated misgiving in the methods, I am not sure if the results can be trusted completely. The figures

and tables were not very clear and didn't do a good job presenting the results. The figures could have used color rather than didn't shapes to provide more clarity. Also, the description the results was quite scattered and hard to follow. A major issue I found with the results section was how their results compared with the earlier studies at the same study site. I thought Chen found a reduction in R and not an increase in GPP. They never clearly stated whether the NPP increase was due to reduction in R or increase in GPP. Rather than comparing the results to other papers in this section, I believe they should have left this analysis for the conclusions. The discussion, although somewhat insightful, didn't put the results into a larger perspective as in light of climate change, etc. Some of the discussion seemed to contradict early stated facts in the introduction.

In regards to the structure and grammar, there were many spelling errors that could have been easily caught before the submission of this paper.

Interactive comment on Biogeosciences Discuss., 11, 2001, 2014.