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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.

Anonymous Referee #2

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The Dou et al. study uses a artificial neural network (ANN) to estimate the 'control' treatment CO2 and water fluxes of a nitrogen (N) addition experiment in a Douglas Fir chronosequence in the Pacific Northwest. The experiment was conducted in the footprint of three eddy-covariance (EC) towers meaning the scale of the experiment was much larger than most N addition experiments, and also that the use of a traditional control treatment was impractical.

Overall I think this is an interesting study and important for assessing the impact of N addition on on C and water fluxes in semi-natural forest systems. I think that the ANN approach is a nice way to address the impracticalities (cost and landscape heterogeneity) of using a traditional control in manipulations at the scale of EC measurements.

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Discussion Paper

However, I have one major concern with the ANN method and that is that the effect of stand development is not accounted for. Considering the data from both the HDF00 and HDF88 sites together, Figure 3 shows that there is a near linear relationship between GPP (or NEP) and stand age. The ANN does not consider stand age, which when accounted for could dramatically reduce the calculated N fertilization response of GPP and NEP. Stand age must be accounted for and I discuss this further below.

The manuscript will benefit from restructuring, focusing around the broader question on the the effects of N addition on C and water fluxes. The abstract is focused on the broader questions and the introduction mostly deals with them but then concludes that the primary goal of this study is to "resolve the slightly different findings . . ." of Jassal et al. (2010) and Chen et al. (2011).

Introduction

I found some of the introduction to be lacking in structure and detail. Additional quantitative detail should be added to the discussion of the effect of N deposition given the widely different estimates between the Magnani paper and for example Sutton et al. (2008).

The introduction to the Jassal and Chen studies comes out of nowhere. There needs to be some introduction to the site and the experiment.

Please develop the hypotheses/goals to be broader than just the resolution of differences between the Jassal and Chen studies.

Methods

As pointed out by M. Wallenstein, why was such a high N addition rate used? This is way above anything that may occur via N deposition. There needs to be some justification based on the goals of the study.

Chen et al. (2009) is referred to for the GPP and R partitioning method but some description should be added.

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NUE, WUE and LUE need defining in the methods. Especially NUE and LUE. Is LUE GPP over absorbed PAR or just PAR? Is NUE the sum of additional NEP over the four years, or is it the annual mean over the N addition? In fact NUE is a specific term, usually used to refer to NPP over N uptake. The term used by Sutton et al. 2008 is C:N response, please change NUE to something more in line with the literature.

Section 2.5 heading – I wouldn't describe the modeling as an experiment, it's a comparison of methods.

Results

The results section is disorganized and should be restructured to improve the flow and readability in the manuscript. The comparison between the MLR model and the ANN model is interlaced with discussion of the N effects. The results should be organized to: 1) Start with the observed C fluxes (Figure 2 & section 3.2), 2) present the ANN model validation and comparison to the MLR model (Figure 1, then Fig 4 & 5, sections 3.1, section 3.4 to ln 10 p 2015), 3) present the effect of N addition (Fig 3, Fig 8, Fig 9, section 3.3, 3.4 from ln 10 p 2015, 3.5).

Fig 6 & 7 can be added to Fig 4 & 5.

Why are GPP, R and NEP not presented in the same way as Figure 8? Please add another figure.

I agree that to properly compare the results from this study with those of Jassal and Chen an assessment of uncertainty needs to be made.

Fig 3. All the pre-fertilization points should be open shapes and all post-fertilization should be filled shapes.

Discussion

There are several issues with the discussion.

1) Figure 3 really suggest to me that there is a strong effect of stand development that

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has not been discussed at all. Fig 3a shows a near linear trend of GPP and NEP to stand age through the two sites HDF00 and HDF88. Does the ANN account for this? I'm assuming not. This needs discussing and further analysis to try to tease out the effects of stand development from the effect of fertilization. I don't think the ANN is able to do this. Without considering stand development it's impossible to support your second conclusion that N fertilisation was the cause of the increase in GPP measured by the EC towers at HDF00 and HDF88.

2) The discussion in the context of N deposition is poor. There is limited discussion of the difference between the large N addition rates in this experiment and how this might be different to the lower rates of N addition via deposition. It is surprising that the C:N response (what the authors term NUE) is the same order of magnitude as previous studies (e.g. Sutton et al. 2008) as one would expect the large N addition rate to be used much less efficiently that the N added via atmospheric deposition. Futhermore it is difficult to know if the comparisons are like-with-like as the C:N response shown by Sutton et al. 2008 and others are based on annual rates, and N arrives continually via deposition as opposed to a single fertilization event. There is no discussion that N would be expected to be used more efficiently by the forest subjected to the 60 kg N ha-1 addition, 200 kg N ha-1 is huge and more N under this treatment would likely be lost via leaching, volatization and denitrification.

3) There is limited uncertainty quantification. Both in the comparison of the Chen and Jassal work to this study and the effects of N fertilization using the ANN. Estimates of uncertainty should be added to Fig 8 and the data in Tables 2 and 3 in order to statistically asses the effect of N addition on C and water fluxes. However, this still would not be sufficient to account for the effect of stand age as discussed above.

Section 4.5 is necessary but could be more focused and precise. What does the "and limitation" in the section title refer to, limitation to what? Ln 7/8 what does "modeling experimental methods" mean?

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I like the discussion of the reduction of ecosystem respiration in response to N fertilization, this is an effect that is unlikely to be affected by stand development and is a major conclusion of interest to the ecosystem C cycling community and it would be good to see this discussed in greater depth.

Minor points

As noted by the previous reviews there are some acronym explanations missing, i.e. EC, PAR . I also found the site labeling difficult to follow, why not label the stands just with 61yr, 22yr, 10yr or at least add the age to the site identifier.

Section 3.3 heading – "variations" what do you mean by variations? Be more descriptive.

Section 3.5 ln 14-19 should be part of the model validation/comparison section.

There are several misleading/unnecessary conjunctives, e.g. "However" ln 24 p 2016 (one would expect WUE to increase WUE with no change in ET and an increase in GPP); "Moreover" ln 2 p 2017.

ln 27 "due to that it caused ..." change to "due to"

P 2003 ln 3 "ecosystemsmainly" needs to be corrected. There are a number of examples of this.

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