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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.

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Dear Dr. M. Wallenstein We thank you and your graduate students for the detailed comments on our manuscript, which have been very helpful in improving our manuscript. Below are our point by point responses to your questions and suggestions.

Comment #1: 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

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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. Response: First, we would like to point out that the goal of this study was not only to resolve the slightly varying outcomes from our previous two papers using a different modelling approach, but also to estimate the effects of N fertilization on carbon and water fluxes in three different-aged stands during the four post-fertilization years and compare estimates using ANN and MLR models. In this study, the responses of C fluxes, ET, WUE and LUE to N fertilization were first investigated by using the ANN approach, and the differences among the three stands during the four post-fertilization years were compared. Regarding uncertainty in the measured C fluxes and those calculated with the three models, these are now included and discussed in Section 4.5 of the revised manuscript.

In addition, regardless of the confidence intervals and statistical analysis, we followed the procedure below as our objective criteria to determine the most optimal ANN model. First, due to the differences of stand age among these three stands, the ANN model was applied to each site individually. To ensure high precision in the period of model prediction, we used multi-year monthly climate variables and EC-measured C fluxes and ET before 2005 to train the ANN model. For this calibration period, aiming at obtaining the hidden node number and avoiding over-fitting in the training period, we used a trial and error method to select the optimal solution through altering the number of hidden nodes. Second, the trained model was verified with measurements in 2005 and 2006. At this step, the optimized ANN model was determined using the coefficient of determination and root mean squared error (RMSE) between predicted and measured C fluxes and ET. We selected the optimized network based on the objective criteria with maximum value of coefficient of determination and the minimum value of RMSE. For the validation period, the statistical parameters from the most optimal model were summarized and illustrated in Table 1 and Figure 5. Finally, once we were confident

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in the optimized ANN model which could successfully simulate the multiyear seasonal variations of C ïňĆuxes and ET, the input values for the post-fertilization period were brought into the trained ANN to predict GPP, R and ET for the period 2007 - 2010. The optimized model, with R2 close to 1, as shown in Table 1 and Fig. 5, was more effective to simulate the C and water fluxes during the post-fertilization period, due to its strong generalization ability in extrapolating the implied and captured law between environmental variables and C and water fluxes from the period of pre-fertilization to post-fertilization. The resulting differences between the measurements and predictions were used to discern the impact of fertilization. Although we didn't consider the confidence intervals in our study, we had selected the most optimal model for each stand to simulate the C fluxes and ET reliably, according to our criteria in the procedure above. Therefore, it was worthwhile as an alternative approach to assess the impacts of N addition.

Comment #2: 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. Response: All acronyms and abbreviation are now defined when they appear for the first time in the MS.

Comment #3: The labels for the different stands, distinguished by age, were 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. Response: We appreciate this suggestion. We have changed the stand labeling in revised manuscript. We now label the stands with 61yr, 22yr, 10yr such as 61 yr-old stand (DF49) as the site identifier in the entire revised manuscript. Comment #4: 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

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same study sight. Due to this setup of the hypothesis, the study doesn't sound very important. Response: See our response to Comment #1 above.

Comment #5: 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?) Response: It has been suggested that forest re-growth, CO2 fertilization, land use and climate change are the important driver factors determining the variation of C sink strengths in late twentieth century. Throughout this period, however, the land and oceans have also been undergoing a rapid change in N deposition. Therefore, this N supply is playing important role in productivity and C cycles in terrestrial and marine ecosystems. It is common knowledge that N limited takes place in most temperate and boreal forest ecosystems. As a result, N fertilizer may have positive impacts on forest CO2 uptake and consequent C storage, slowing the rise in atmospheric CO2 concentration and increasing wood supply. Furthermore, the net C sequestration in response to N deposition is not only a scientific issue, but also associated with political consequences through influencing the control of N emission from the international control protocols in the next generation. Furthermore, water and C cycles in terrestrial ecosystems are closely coupled. Nutrients may not only affect productivity and foliar biomass but also associated with evapotranspiration (ET) in forest and other ecosystems. Water use efficiency (WUE), is defined as the ratio of C gain (usually gross primary productivity, GPP) to water loss, i.e., ET. The impact of N deposition on ecosystem functioning is an important aspect of anthropogenic global change. With the increase of N deposition from fossil fuel combustion and application of artificial fertilizers, effects of N enrichment on ecosystem structure and functioning have been widely studied. C and water fluxes, and the effects of N fertilization, are significantly correlated with stand

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age. These aspects are now explicitly stated in the revised manuscript.

Comment #6: 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. Response: The three different-aged stands we investigated in this study are in the Pacific Northwest coastal forest region of the United States and Canada. Pacific Forests in this region between Oregon and Alaska cover approximately 105 km2 and play an important role in the global C cycle. In this region, due to their considerable distances from heavy industrial activity, there is very little atmosphere N deposition. As a result, the standard forest fertilization application rate in West Coast forests is 200 kg N ha-1 from prilled urea (Hanley et al. 1996). While fertilization of stands of mid-rotation trees (i.e., of commercial thinning size, 20–40 years old) can result in additional merchantable timber volumes, fertilization late in the rotation may be the most attractive alternative economically. Similarly, to boost growth of seedlings/saplings while avoiding competition from prolific brush on N-deficient soils, fertilization is done on the drip line of saplings at a lower rate of 50-60 kg N ha-1.Therfore, high N fertilization level (200 kg N ha -1) in a single application used in this study is a common forest management activity in N-limited forests (Hanley et al. 1996).

Comment #7: 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. Response: We agree with the referee that a number of factors independent of N addition could lead to observed changes in GPP, LUE, C

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exchange fluxes, and WUE. In this study we used model experiments to discern the effects of N fertilization. Based on the long time series of data without fertilization, we calculated what the fluxes would have been during the last four years if there were no fertilization, and then obtained the effect of fertilization by comparing these predicted fluxes with post-fertilization measured fluxes. The discussion of uncertainty in the effects of N addition on C and water fluxes has been added in Section 4.5 in the revised manuscript.

Comment #8: 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. Response: See response to Comment 6 above.

Comment #9: 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. Response: As mentioned in our response to Comment #2 above, all acronyms are now clearly defined.

Comment #10: 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. Response: We have now added the description of how NEE was partitioned into GPP and R in Section 2.3 of the revised manuscript.

Comment #11: 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. Response: The models calculated fluxes for control (no N added) conditions, i.e., assuming the stands were not fertilized. Therefore, there is no issue with different weights being used to model fluxes under control and fertilized conditions.

Comment #12: 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. Response: The optimized model has high R2, close to 1, even in the youngest stand (HDF00) ïijŇR2 =0.92, as shown in Table 1 and Fig. 5. Detailed procedure of our experiment in the study can be found in our response to Comment #1 above. We believe that the model can effectively simulate the C and water fluxes for the last four years if the stands were not fertilized because of its strong generalization ability in extrapolating the implied and captured law between environmental variables and C and water fluxes from the period of pre-fertilization to post-fertilization. Then the effect of fertilization can be obtained simply by difference as stated above (response to Comment #7).

Comment #13: 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. Response: We have re-drawn Figures 3-9 to make them clearer in the revised manuscript.

Comment #14: 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. Response: We agree that this is an important point and we have already addressed it in the second conclusion in Section 5. Similar to our study, Chen et al. (2011) and Jassal et al. (2010) used different modeling approaches (BEPS and MLR, respectively) to estimate the effect of N fertilization on C fluxes. Chen et al. (2011) found that annual NEP in the first post-fertilization year in the 61 yr-old stand (DF49) increased by 83% , which resulted primarily from an increase in annual GPP of 8% and a decrease in annual R of 5.7%. Jassal et al. (2010) found that N application during the post-fertilization two years increased GPP and R in all the three stands with increases in GPP being greater than R in the 61 yr-old stand (DF49) and 22 yr-old stand (HDF88) but smaller than R in the 10 yr-old stand (HDF00). They also found that fertilizer-induced increases in NEP during the post-fertilization two years was the highest in the 22 yr-old stand (HDF88) followed by the oldest stand (DF49) and small decrease in the youngest stand (HDF00). Since the previous two studies used only one (Chen et al. 2011) and two years (Jassal et al. 2010) post-fertilization data, we wanted to study the effect of a large N application over a longer period, i.e., 4 years. The different findings in above three studies were summarized and compared in Figure 8 and Tables 2 and 3 in the revised manuscript.

Comment #15: 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. Response: It is widely held that increased N deposition from atmospheric may enhance globally terrestrial C sequestration, especially in N limited temperate and boreal forest ecosystems (Magnani et al., 2007; Sutton et al., 2008). Moreover, Komarov and Shanin (2012) concluded that nitrogen deposition for forest ecosystems of European Russia played different relative roles acting together with climate changes in different climatic zones. Therefore, it is important to assess the effect of nitrogen deposition on forest C sequestration in the context of various future management and climate change. We have made above

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revision in the revised manuscript in Section 4.3 (see lines 10-16 page 22).

Comment #16: In regards to the structure and grammar, there were many spelling errors that could have been easily caught before the submission of this paper. Response: The revised paper has been carefully edited by native English speakers.

Refferences Chen, B., Coops, N. C., Andy Black, T., Jassal, R. S., Chen, J. M., and Johnson, M.: Modeling to discern nitrogen fertilization impacts on carbon sequestration in a Pacific Northwest Douglas-fir forest in the first-postfertilization year, Glob. Change Biol., 17, 1442-1460, 2011. Hanley, D. P., Chappell H. N., and Nadelhoffer E. H.: Fertilizing coastal Douglas-fir forests, Bull. 1800, Wash. State Univ. Ext., Pullman, Wash, 1996. Jassal, R. S., Black, T. A., Cai, T., Ethier, G., Pepin, S., Brümmer, C., Nesic, Z., Spittlehouse, D.L., and Trofymow, J. A.: Impact of nitrogen fertilization on carbon and water balances in a chronosequence of three Douglas-fir stands in the Pacific Northwest, Agr. For. Meteorol., 150, 208-218, 2010. Komarov, A. S. and Shanin, V. N.: Comparative analysis of the influence of climate change and nitrogen deposition on carbon sequestration in forest ecosystems in European Russia: simulation modelling approach, Biogeosciences, 9, 4757-4770, doi:10.5194/bg-9-4757-2012, 2012. Magnani, F., Mencuccini, M., and Borghetti, M.: The human footprint in the carbon cycle of temperate and boreal forests, Nature, 447, 848-50, 2007. Sutton, M. A., Simpson, D., Levy, P. E., Smith, R. I., Reis, S., Van Oijen, M., De Vries, and W. I. M.: Uncertainties in the relationship between atmospheric nitrogen deposition and forest carbon sequestration, Glob. Change Biol., 14, 2057-2063, 2008.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C3134/2014/bgd-11-C3134-2014-supplement.pdf

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